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ABSTRACT

The Long Shadow of the Chinese Cultural Revolution: The Intergenerational Transmission of Education*

Between 1966 and 1976, China experienced a Cultural Revolution (CR). During this period, the education of around 17 birth cohorts was interrupted by between 1 and 8 years. In this paper we examine whether, and by how much, this large-scale schooling interruption affected their children's education. We find a strong effect: more interrupted education for parents, less completed education for their children. On average the CR cohort had 2.9 years interrupted education. If they failed to catch up after the CR, this translates to a reduction of 0.87 years of schooling and a 9 percentage points (or 50%) reduction in the probability of completing a university degree for their children relative to the children whose parents did not have interrupted schooling. Our results have strong implications for developing countries prone to long-term conflicts which often adversely affect children's education. As human capital accumulation is one of the main drivers of economic development, these negative schooling shocks affecting current generation education levels will have an impact far beyond the immediate economic development of these war-torn economies and extend to the next generation.

JEL Classification: I24, I25, N3

Keywords: Chinese Cultural Revolution, human capital, intergenerational education transmission

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1 Introduction

Between 1966 and 1976 China went through a 10-year large-scale political upheaval: the Cultural Revolution (CR). In 1966 Mao launched the CR to reassert his authority over his opponents whom he believed were taking China in a wrong (capitalist) direction. Mao called upon Chinese youth to purge the anti-revolutionary elements of government, the party, and society more generally. In the initial phase, most government leaders and party officials, from the president (Shaoqi Liu) to many factory and village heads, were removed from power.

The Cultural Revolution affected many aspects of contemporary Chinese society. Particularly important were the schooling interruptions of as many as 17 birth cohorts. In the first 2-3 years of the CR (1966-1968), all schools including colleges and universities in urban China were closed. Later on, primary and junior high schools were reopened gradually. However, senior high school recruitment did not restart until 1972, while merit-based university enrollment did not resume until after the Cultural Revolution in 1977 (Deng and Treiman, 1997; Meng and Gregory, 2002). As a result, cohorts born between 1947 and 1963 (labelled as CR cohort or parent cohort hereafter) experienced various years of schooling interruption and many were denied a chance to obtain a university degree.

A question naturally arises as to whether, and by how much, this large-scale schooling interruption affected the education level of the next generation. High persistence of educational attainment across generations is well established in the literature for many countries. A large number of studies, relying on different strategies, have identified causal intergenerational educational links.[1] But for China, the literature on intergenerational transmission of education is limited. Few existing studies mainly examine the intergenerational educational correlation (see, for example, Magnani and Zhu, 2015; Hertz et al., 2007).[2].

The schooling interruption during the CR can be thought of as a natural experiment, but unlike other natural experiments in the area of education, schooling interruptions

[1]See, for example, Holmlund et al. (2011); Black and Devereux (2011); Rosenzweig and Wolpin (1994); Behrman and Rosenzweig (2002, 2005); Bingley et al. (2009); Sacerdote (2002, 2004); Plug (2004); Bjöklund et al. (2006, 2007); Tsou et al. (2012); Chevalier (2004); Black et al. (2005); Oreopoulos et al. (2006); Tsai et al. (2011); Chevalier et al. (2013).

[2]There are a few studies which investigate the intergenerational effects of the 1959-1961 China famine on health and wages and generally find statistically significant intergenerational effects (Susser et al., 2008; Almond et al., 2010; Fung and Ha, 2010; Kim and Fleisher, 2010)

during the CR affected a whole generation of youth (17 birth cohorts) and for as long as 1 to 8 years. An understanding of the intergenerational education impact of an event of this scale is very important. Recent literature has found that historical shocks at "crucial junctures" often have a long-lasting influence on a country's economic growth path (see, for example, Acemoglu et al., 2001; Nunn, 2008; Iyer, 2010), and that human capital accumulation is an important channel through which these historical events affect contemporary economic development (see Waldinger, 2012; Caicedo, 2014, for example). The Chinese experience indicates that intergenerational transmission of human capital may contribute significantly to such a long-term effect.

Understanding the intergenerational impact of this large-scale natural experiment can also increase our understanding of long lasting impact of political upheavals in developing world on long-run economic development through intergenerational education transmission. In today's world, political upheavals, wars, and other conflicts are far from being eradicated. Children in many parts of the world are being denied a chance of going to school because of such adverse shocks. As human capital accumulation is one of the main drivers of economic development, these negative schooling shocks affecting current generation education levels will have an impact far beyond the immediate economic development of these war-torn economies. Although the existing literature on intergenerational education transmission has identified the causal effect, the literature often focuses on rich countries with limited ***positive*** educational shocks at the lower end of the education distribution (such as the introduction of a compulsory schooling law). School interruption during the Chinese CR enables us to analyse intergenerational education transmission for larger-scale schooling shocks (between 1 and 8 years) at the higher end of the education distribution (by stopping people from obtaining senior high school, college, or university degrees). In addition, unlike the existing literature, the CR affected education ***negatively***. To the extent that education shocks may have asymmetric impacts, it is of great importance to observe the magnitude of the impact of these negative shocks, that are more in line with the experience of today's developing world.

In this paper, we first establish the link between parental schooling interruption during the CR and their children's schooling achievement by estimating a reduced-form model. We find that children whose parents had more years of interrupted schooling during the CR complete fewer years of schooling and are less likely to go to university relative to their counterparts whose parents either had fewer years of interrupted schooling or were not affected by the CR at all.

But the Cultural Revolution affected the parental generation in many other ways. Could it be that the measure of parental schooling interruption in our regression analysis is, in fact, capturing other adverse experiences parents went through during the CR? To understand this we identify two other likely events during the CR which some or most of the parental cohorts also experienced: one event is the violence that occurred during the initial years of the Cultural Revolution, and the other is the incidence of the Sent-Down Youth (SDY) program, whereby some of the CR cohorts were sent as teenagers to work in the countryside for many years.

Between 1966 and 1969 many cities experienced considerable unrest and violence, whereby pre-CR government or work unit leaders were targeted by the Red Guards and accused of being 'capitalist walking dogs'. At the same time, the Red Guards were divided into factions and violence between factions was widespread. Most of the parental cohort were in their early developmental age during this violent period. According to recent literature, children experiencing violence during developmental ages may respond with long-lasting behavioral effects (Bauer et al., 2014). If so, such adverse effects could also affect the parents' influence on their children, leading to the lack of educational achievement.

The Sent-Down Youth program was adopted during the CR to assign urban junior or senior high school graduates to rural areas to be 're-educated' and to resolve the problem of lack of city employment. Around 11% of the individuals from relevant cohorts were sent-down. It was not until the end of the Cultural Revolution that most of them were able to go back to their sending cities. For these individuals, this was a life-changing experience which may also have affected their attitude and behavior, which, in turn, could influence how they brought up their children.

We test whether the schooling interruption measure used in our estimation is in fact a proxy for these two events. To do so, we add direct controls to measure the two events in the reduced-form estimation between parental interrupted schooling years and their children's education achievement. Our results suggest that controlling for these potential confounding factors does not change the statistically significant negative relationship between parental schooling interruption during the CR and their children's education achievement. This supports the view that there is a strong independent parental schooling interruption effect on the education of the next generation.

We then examine the potential channels through which parental interrupted schooling may affect children's years of schooling. Essentially, in this part of the analysis we use parental human-capital related outcome variables as the dependent variables and examine whether parental schooling interruptions during the CR has an impact on each of these outcome variables. Based on the data available we include parental final education achievement, health (measured by self-assessed health and height), and fertility behaviors. Our results show that there is a strongly negative relationship between parental interrupted schooling and their educational attainment. Controlling for parental education achievement, the relationships between parental interrupted schooling and parental self-assessed health, height, and fertility are neither economically meaningful nor statistically significant. These results suggest that parental interrupted education impacts children's education through parental educational achievement but not through these other variables we included.

This insight gives us some confidence that perhaps we could use parental interrupted schooling years during the CR as the instrument for parental education and estimate an intergenerational education transmission coefficient with 2SLS regression. Using LATE interpretation of our results, we find that a one-year decrease in parent's schooling as a result of school interruption during the CR leads to around 0.3-year decrease in the child's schooling and 3.1–4.4 percentage points reduction in the child's probability of obtaining a university degree on average. This is a large effect. On average the CR cohort parents had 2.9 years interrupted education. This translates to a reduction of 0.87 years of schooling for their children relative to their counterparts whose parents did not have interrupted schooling during the CR. In the extreme case, for children whose parents had 8 years of schooling interruptions their years of schooling are reduced by 2.4 years. Regarding university attainment the effect is also large. On average the probability of obtaining a university degree among the children of CR cohort was reduced by almost 9 percentage points. Considering that only less than 20% of the children's generation in our data has a university degree, this is almost a 50% reduction. We also carry out a large number of robustness tests for potential violation of the exclusion restrictions.

The rest of this paper is organized as follows. Section 2 describes how the Chinese

Cultural Revolution affected educational attainment of certain birth cohorts, and describes two other relevant historical events which were part of the CR experience of the parental cohorts. Section 3 discusses the data. Section 4 investigates the reduced form relationship and tests for potential confounding factors. Section 5 examines the channels through which parental schooling interruption during the CR may affect children's education achievement and Section 6 presents the 2SLS results and robustness tests. Section 7 concludes the paper.

2 Historical Overview

The Chinese Cultural Revolution occurred between 1966 and 1976. It was a political movement that disrupted everyone's lives. Although this paper focuses mainly on the interrupted education impact, two other events, which occurred during the CR, could also have significant impacts on the cohorts of our interest.

2.1 *School interruptions during the CR*

The lives of everybody who lived through the CR period were affected by the political upheavals during the period, but only those who were at school during that period experienced school interruption, and the degree of school interruption differed across cohorts and cities.

The normal education system was massively disrupted in China (mostly in cities) during the CR.[3] In the first 2-3 years of the CR (1966-1968), all schools in urban China were closed, including colleges and universities. During 1968-1969, primary schools and junior high schools were reopened gradually so that those students who would have completed primary education during 1966-1968 were able to go to junior high school and those aged 7-9 could begin primary school. However, the normal school curriculum was not followed. Students during those years spent most of their school time working in factories or the countryside to be re-educated by peasants and workers. For the remaining time, when they were at school, classroom teaching was focused mainly on political propaganda or practical knowledge. During this period, senior high schools and colleges/universities remained closed. Those students who should have graduated from junior or senior high

[3]See Deng and Treiman (1997) and Meng and Gregory (2002) for the details of the impact of the Cultural Revolution on education.

schools during 1966-1969 were given diplomas, even though they had missed several years of schooling, and then sent to work in the countryside or factories. Senior high schools did not reopen until 1972. By then, the primary and junior high school education system had returned to the normal curriculum.

A few universities and colleges began to enroll new students in 1970, but recruitment examinations were abolished. Candidates were not selected from senior high school graduates but from those who were working in factories, countryside, and the army. Because of this they were labeled *Worker-Peasant-Soldier (WPS) Students*. The main selection criteria for university admission during that period became family 'class' background and party loyalty (Meng and Gregory, 2002). Children from families which were deemed to be enemies of the country[4] were almost completely excluded from gaining university admission. For example, in 1970, of the 8,966 students enrolled by the seven universities and colleges in Beijing, only 0.2% were from a 'bad' family background (Cheng, 2001, p: 398). The admission criteria did not change until 1977 when the university entry examination was resumed. In addition to lack of academic criteria in student selection, the duration of university education was reduced from four years to 2-3 years and the quality of education was seriously compromised. Furthermore, entry quotas for university enrollment during this period were very small. A total of 42,000 students were recruited in the two years of 1970 and 1971, while in 1965 alone – the last year before the CR – universities and colleges admitted 164,000 students (NBS, 1999). After the CR, those admitted to universities/colleges during 1970-1976 (the WPS graduates), were not awarded a university degree but received a college diploma.[5] Due to the limited number of *WPS* graduates, we will include them in our main estimation but will test the sensitivity of the results against this inclusion.

Meng and Gregory (2002) summarized school interruption during the CR by birth cohort into two main impacts: the number of years of schooling interrupted and the number of years delayed (potential) university entry. We reproduce these cohort-varying schooling interruptions in Table 1. Below, we take the 1955 birth cohort as an example to explain the table. This cohort was aged 11 when the CR began in 1966. During

[4]The enemies of the country normally included those who used to be landlords or rich farmers before the communists took over power, the counter-revolutionaries, criminals, the rightists, the within party 'capitalist running-dogs', and intellectuals.

[5]There are two types of tertiary education in China: four-year university and three-year college. The former graduates are awarded a bachelor's degree, while the latter receive only a diploma.

the pre-CR period, the normal age for school enrolment was seven. If this cohort started primary school on time, they had completed four years of primary education when the CR began. Because all schools were closed between 1966 and 1968, their primary education was cut short by two years. In 1969 these students went directly into junior high school even though their primary education was not finished. In junior high school, from 1969 to 1971, the students mainly took excursions to factories and countryside to work rather than learning in classrooms, and hence they 'missed' another three years of junior high school education. In 1971, although they graduated from junior high schools, the senior high schools were not opened for admission. As a result, this cohort did not have the opportunity to obtain a senior high school education (three years). After the CR finished, the National College Entrance Examination System was resumed in 1977, and if they attended and passed the exam, they could start their tertiary education in 1978. By then they were aged 23 – had their education not been disrupted during the CR, they would have gone to university at the age of 19.[6] Thus, this cohort missed two years in primary school, and three years in junior high school, three years in senior high school during the CR; and the possibility of their admission to university was delayed by at least four years.[7]

To sum up, the table shows that the education of those who were born between 1947 and 1963 was interrupted by the CR differentially – missing between 0 and 8 years in primary and high schools and delaying university entry between 0 and 12 years. However, for those who were born before 1947 and after 1963, their education was not directly affected by school interruption during the CR.

To establish that schooling interruptions during the CR indeed occurred and that they indeed affected different birth cohorts differentially, we present detrended education outcomes (years of schooling and university degree attainment) by birth cohorts from China's 1990 and 2000 Population Census data. Intuitively, if there had been no schooling interruption, educational attainment should increase over time. Thus, we use the linear time trend to approximate the counterfactual educational attainment in the case of no

[6] Here, the possibility of being admitted to university/college during the CR is not considered given the low quotas and the admission criteria in those years.

[7] The schooling system may differ slightly across regions. These calculations of years of 'missed' schooling and years of 'delayed' university entry are based on the most commonly used schooling system in China: six-year primary education, three-year lower-secondary education and three-year upper-secondary education.

school interruption, while the deviation of actual educational attainment from the trend should be able to measure the shock of schooling interruption.

Figure 1 presents these detrended education outcomes. The left panel of the figure presents the original education across different cohorts and the linear time trend, while the right panel shows the detrended education. The figures clearly show that after accounting for the time trend, there are considerable variations in schooling outcomes of different birth cohorts who experienced schooling interruptions during the CR.[8]

In addition to cross-cohort variations on schooling interruptions, for the same cohort across different cities, schooling interruption during the CR also differs. During the CR, the central government's power was weakened dramatically. Local 'revolutionary' factions across cities constantly implemented their own rules and regulations. Local violent conflicts also lasted for different periods of time across cities (Vogel, 1971; Walder and Su, 2003). Consequently, the exact length and degree of schooling interruption varied across cities. Figure 2, which presents data from detrended years of schooling for a selection of cities, shows clear cross-city variation.

In the analysis of the remainder of this paper, the schooling interruption for the CR cohorts will be measured in two ways: the total number of years of schooling missed (column 8 of Table 1) and the detrended years of schooling by cohort and city, derived from the Census data (some examples are shown in Figure 2).

2.2 *Violence and chaos*

During the early chaotic years of the CR (mainly 1966-1968), violence occurred in many cities at differential levels. While schools were closed for normal curriculum learning, students in junior, senior high schools and universities were called upon to participate in the struggle against class enemies within their own schools, communities, and even families. People were divided into different factions and widespread violence between factions occurred in many regions (Walder and Su, 2003; Bai, 2015). During this period, many

[8]Note, however, that the cohorts that are in the trough of Figure 1 do not exactly match with the number of years of missed schooling by each birth cohort presented in Table 1. This arises because although schooling of these cohorts was interrupted, they were still given the relevant certificates (either junior or senior high schools). During the census or surveys, people reported their education levels based on their final certificate obtained, not taking into account schooling interruptions. If we subtract these mis-reported years of schooling from individuals' reported schooling, we observe that the birth cohorts that experienced the greatest schooling interruption as indicated in Table 1 are also those with the lowest average years of schooling (see Appendix Figure B.1).

of our treatment cohorts were also in their developmental ages, some participated in the violence directly, some witnessed large-scale armed conflicts, and others were themselves or had family members subjected to such violence.

Recent literature examining the impact of war and conflicts on individuals' social preference has indicated that the adverse experiences associated with these events may induce impatience and risk-seeking behaviour (Voors et al., 2012; Callen and Sprenger, 2011). Some studies also find that being subjected to these violent experiences impacts people's preference most if they are within a developmental window starting from age 7 and ending in early adulthood (around 20 years of age) (see, for example, Bauer et al., 2014).[9] Although this literature mainly focuses on intergroup conflict, to the extent that violence per se affect individuals' preference with such an age-varying impact, we should be vigilant of its potential confounding effect relative to schooling interruption impact.

2.3 *Sent-Down Youth Movement (SDY)*

Some individuals, whose education were interrupted during the CR, also experienced another life-changing event: the Sent-Down Youth Movement. The SDY movement was initiated long before the CR in the late 1950s as a policy response to provide additional employment opportunities for urban youth and to accelerate rural development. During the CR period, because senior high schools did not reopen until 1972 and universities did not start to recruit fresh senior high school graduates until the CR was finished in 1977, the government faced difficulty in finding urban jobs for many cohorts of urban junior and senior high school graduates. Conveniently, it resorted to the SDY to help to resolve the problem (see, for example, discussions provided in Thomas, 1977; Bonnin, 2013). However, this was only one employment solution which was complementary to other employment opportunities in cities, such as employed as factory workers and other service workers. As a result, cohorts from cities which happened to have more factories and other urban employment opportunities were less likely to be sent to the countryside. Based on the data collected by the authors from Chinese county and city Gazetteers (Jin and Jin, 2014), between 1966 and 1976, the total number of relevant cohorts being sent down was around 11.2 million,[10] which is only around 10% of the total urban *hukou*

[9] For a similar literature, also see Voors et al. (2012).

[10] During the CR, some adult urban residents were also sent down to the countryside, but only those who were recent junior or senior high school graduates at the time (so-called 'educated youth' or '*Zhi Qing*'

population for the relevant cohorts (those born between 1947 and 1963).[11]

While most individuals who were sent down also had interrupted education, the probability of being sent down should not coincide closely with schooling interruptions during the CR. Two survey data sets (the China Income Project Survey (CHIPs), 2002; and the China Urban Labor Survey (CULS), 2001) have sent-down youth information. We plot by birth cohort the number of schooling years interrupted and the proportion of individuals who had Sent-Down experience in Figure 3. The data indicate that all parent cohorts in our study (born between 1940-1963) had a non-zero probability of having sent-down experience. Not all cohorts had schooling interruptions, however. Education interruptions only started from those who were born in 1947. In addition, for SDY, the highest proportion of individuals with Sent-Down experience is those born in 1951 (27% of the cohort), whereas the cohort which has the most years of schooling interruption is those born in 1955.

3 Data

To focus on the intergenerational education effect, we need to match children's education with their parents' education. This is quite challenging. Normal household surveys do not have a representative sample of parents-children pairs as they only survey household members (members of the family who live at the same residential address). In general, parents or children who were not residing in the same address at the time of the survey are not included in the survey. Thus, normal household surveys only include the selected sample of parents and their children who happened to be living in the same address at the time of the survey.

To this end, we are fortunate to have three survey data sets which satisfy the requirement of having a representative sample of parents-children pairs. They are the China Urban Labor Survey (CULS, 2001), the China Income Project Survey (CHIPS, 2002), and the Urban Residents Education and Employment Survey (UREES, 2005). All three

in Chinese) were counted in our calculation.

[11] The population figure for the cohorts born between 1947 and 1963 with urban *hukou* is obtained from the 1990 Census when these individuals were aged 27 to 43. Inevitably the figure is lower than the actual population size for the cohorts during the CR due to the accumulated mortality. However, given that in 1990 even the oldest cohort was still quite young, the deviation from the actual number should not be large. Note that the total number of SDY since 1950s, cited in Bonnin (2013), is 17.9 million, but his figure might have included Sent-Down adult urban residents.

surveys collected not only the demographic, education, and employment information of the coresiding household members, but also that of non-coresiding parents of the household heads and their spouses.[12]

Although these three data sets were collected in different years and for different regions, they were all collected in the early 2000s and the sampling frames used were the same as the Urban Household Survey (UHS), conducted annually by National Bureau of Statistics of China. The UHS is based on probability proportional to size (PPS) sampling with stratifications at the provincial, city, county, town, and neighborhood community levels. Households are randomly selected within each chosen neighborhood community.

Appendix Table A.1 presents the survey design and implementation organizations, the geographic coverage of the survey, and the number of households in these three surveys. Although the three surveys using the same sampling frame, they differ in regional representativeness. For example, UREES (2005) and CHIPS (2002, urban sample) cover cities and towns at the provincial, prefecture, and county levels; while CULS (2001) only covers four provincial capital cities and one provincial-level city. Because schooling interruption during the CR mainly occurred in the urban areas, while education in rural China was much less interrupted (Han, 2000), we need to ensure all observations used in this study were urban citizens during the CR. To do so, we exclude all county-level cities from the sample as many of these cities could still be rural areas in the 60s and 70s. Even if they were not, they were more likely to have followed the rules for rural schools rather than city schools.

Appendix Table A.2 presents the sample restrictions we used for each of the data sets. The data presented in the first row show the total number of households, while the second row presents the total number of matched parent-child pairs who live in prefecture-level cities and the parents possessing urban *hukou*. The next 3 rows, respectively, exclude matched pairs whose children have not completed schooling, where either the parent or

[12]CULS also includes non-coresiding children and parents of all household members. CULS (2001) defines the household member as a person who lived in the survey residential address for at least 6 months at the time of the survey. In addition to normal questions about household members, each member was required to report information about their non-coresiding parents and children. UREES (2005) defines household members as household heads, their spouses, heads' and spouses' parents regardless of whether they live in the same residential address as the household head or not, plus up to three children, up to two grandchildren and one other relative. CHIPS (2002) survey is similar to UREES. It includes all household members plus the heads' and spouses' coresiding and non-coresiding parents. To control for the difference in sample coverage in the different surveys, we include both survey dummy variables and the dummy variables indicating the type of parent-child match in our regressions. We also test the sensitivity of including different types of parent-child match to our results.

child has missing values for the education variable, and where the age difference between the parent and the child is less than 14 years or either the parent or child has missing values for age. The next restriction (the 6th row) excludes parents who were born before 1940 and those who were born after 1963. This restriction excludes around 76.7% of the sample and majority of the exclusion (76%) is due to parents born before 1940 and only 0.7% due to parents born after 1963. The reason for excluding parents who were born before 1940 is that these parents obtained their education mainly under the pre-communist system, which may differ from the system that operated after 1949, and part of their education may have also been interrupted by World War Two and the Chinese Civil War. The reason for excluding parents born after 1963 is to ensure that there is no overlap in birth year between children and parent cohorts. The next row excludes the parent-child pairs where the child was born before 1965, to ensure that no one in the children cohorts experienced schooling interruption due to the Cultural Revolution. Finally, we exclude two cities which only have pre-CR cohorts. The final sample comprises a total of 11,113 parent-child pairs, with 4,910 father-child and 6,203 mother-child pairs.[13] The parents in the final sample were born between 1940 and 1963, while children were born between 1965 to 1980.

The key variable in this study is education. We use two variables to measure both children's and their parents' educational attainment: years of schooling and university degree attainment.[14]

The other set of important variables is related to the number of years schooling interruption parents experienced during the CR. Based on the discussion in the last section, we construct two alternative measures. The first is the 'predicted total number of years of schooling interrupted during the CR' (hereafter refereed to as "years of missed schooling"). This variable is derived from Column 8 of Table 1 and it varies only across different

[13] There are more mothers than fathers because of the birth cohort restrictions we impose on parents. As most fathers are older than mothers, imposing the birth cohort restrictions on both parents would leave us with unbalanced mother-child and father-child pairs.

[14] All three surveys ask about the highest level of education achieved, while only the UREES and CHIPs have information on the years of formal education. For the years of schooling variable, we convert the highest education level to it in the CULS data, and the conversion is based on the most commonly used schooling system: six-year primary school, three-year junior high school, three-year senior high school, three-year college and four-year university; and in the other two data sets, we use the variable of years of formal education, and impute it using the highest education level if it is missing or have a unreasonable value, for example, those who reported formal years of schooling being longer than 25. The university degree attainment, based on the highest education level in all three surveys, is directly derived from whether the individual obtains a university degree or above.

birth cohorts. It is constructed assuming that (1) individuals begin school at the age of 7 (the official school starting age for urban China during those years); (2) the complete years of primary, junior high, and senior high school education lasted 6, 3, and 3 years, respectively; and that (3) school interruptions during the CR were uniform across all the cities. In reality, however, the exact length and degree of school interruptions may vary across cities. To utilize this additional level of variation, we use the 1% sample of China's 1990 Population Census data to generate a second cohort-city varying measure of schooling interruption. In particular, we use a sample of urban *hukou* population who were born between 1939 and 1966 to estimate an equation with the years of schooling as the dependent variable and a linear time (cohort) trend and city dummies as the independent variables.[15] We then obtain residuals from this regression, take a three-year moving average of the residual, and use it as the other measure of schooling interruption during the CR (hereafter, "detrended years of schooling"). This "detrended years of schooling" variable can be viewed as shocks to the general trend of educational attainment over time. This is essentially the same as the concept of 'shocks' used in macroeconomics or finance, which usually use ARMA or ARMA-GARCH model to forecast and define the forecast errors as the shocks. In our case, we use a time trend to predict the trended educational attainment and let the deviations from the trend to capture the shocks. This variable varies across both cohorts and cities. This is exactly the same as the way we generated Figure 2. From Figure 2 it is clear that the cohorts/cities which suffered the most from the schooling interruption during the CR (such as those who born in the mid 1950s and those resided in Beijing or Shanghai) have larger negative values of detrended yeas of schooling while cohorts/cities which did not suffer as much may have less negative or even positive values for this variable. What is also clear from the figure is that the degree of schooling shock across different birth cohorts for different cities differs significantly.

The third set of important variables is related to the two other important events which may have affected parents during the CR and which, in turn, may have affected children's education: the severity of violence and chaos by city during the CR period (hereafter, "CR severity") and the Sent-Down Youth (SDY) movement.

Two of the three data sets (CHIPS, 2002 and CULS, 2001) utilized in this paper have information on whether an individual during the CR was sent down or not. Using this

[15]We also use city-specific trends and the results are very similar.

information we generate a dummy variable equal to 1 if a person was sent-down during the CR, and zero otherwise.

It is not easy to measure CR severity. We use three different measures. The first two are from Walder (2014), where he and his team collected detailed numbers of unnatural deaths as well as victims of political persecution including imprisonment, public beatings, expulsion from homes, and charges of counter-revolutionary activity that resulted from political activities during the period 1966-1971.[16] We use both 'deaths' and 'victims' variables, supplementing them with the population size of each city in 1964 (based on 1964 Census data). In addition, to take into account the developmental window during which children are more likely to be influenced by violence and conflicts based on the literature (age 7 to 20) (Bauer et al., 2014), we weight the 'deaths' variable by the number of years each individual falls into this window during 1967 and 1970 period. For the 'victims' variable, the period counted for is 1968-1970.[17] The third measure used to proxy the CR severity is the extent to which normal urban industrial production was interrupted during the CR. Using provincial-level industrial output for the years 1966 to 1971, relative to the average level of industrial output for the three years prior to the CR (1963-1965), we calculate the 'growth' rate (often negative) for each of these years during the CR.[18] The idea is that the severity of chaos during the CR, often accompanied by violence, resulted in factories ceasing production. Using these 'growth' rates we should be able to proxy the most disruptive year during the CR for each province. We then interact this lowest growth rate with a vector of dummy variables indicating each individual's age at that year to measure the CR severity across different regions for individuals at different ages.

The final set of important variables is a set of human capital related outcomes of the parents, which may be affected by the schooling interruptions during the CR, and which may, in turn, affect their children's education outcome. Here, in addition to parental education achievement, we include parental health measures (self-reported current health condition which is available from CHIPS and CULS, and parental height as a measure

[16] The data were collected mainly from China's local annals (Gazetters) supplemented with other materials they collected. The coding rules were very restrictive and they believe that the actual numbers should be much higher. In using these data, the team assumes that the data are not seriously biased and that the cross-regional variation of the numbers reflect the underlying distribution.

[17] Although the data collected covers the period from 1966 and 1971, Figures 3 and 4 in (Walder, 2014) show that the deaths mainly occurred during the 1967-1970 period, while the victims mainly experienced persecution between 1968 and 1970 period.

[18] The industrial output data are not available at the city level for pre-economic reform years.

of long-term health, which is available from CULS) and parental fertility (the number of children the parent has). This variable is available from UREES (2005) and CULS (2001). By limiting the sample to parents aged 45 and above, the number of children can capture parental complete fertility. While the first two sets of parental outcome variables affect children's education via their parents human capital accumulation, fertility may affect children's education through the quantity-quality tradeoff channel.

Summary statistics of these important variables are presented in Table 2. On average the children in our sample were aged 31 years at the time of the survey. Their average schooling was slightly below 13 years and around 19% of them had a university degree. The fathers and mothers in the sample were on average 58 and 57 years of age, respectively. The average years of schooling for fathers was 9.45 years, and for mothers 8.13 years. 7% of fathers and 2% of mothers obtained university degrees.

For the total sample, fathers on average had 0.9 years of schooling interruption during the CR, while for mothers it was longer - 1.41 years. The difference is due mainly to the larger proportion of mothers experiencing schooling interruptions during the CR (44% for fathers and 55% for mothers). Overall, fathers' detrended years of schooling is higher than that for mothers (0.12 vs. 0.05 years).

For the sample of parents whose education was interrupted during the CR (cohorts born between 1947 and 1963), the mean interrupted schooling years was 2.3 years (2.2 for fathers and 3.1 for mothers) and their mean detrended schooling years is -0.27.[19] Relative to the pre-CR years, the average worst year of output growth is 0.2 percent. However, the large standard deviation suggest there existed many negative growth regions and periods. The average death rate is around 0.07%, while the victim rate is much higher at around 2.6%.

In the sample, mothers are more likely to have the Sent-Down Youth experience than fathers. On average around 8% of our total sample had SDY experience. Around a quarter of our parents reported that they have excellent or good health, while the average height of fathers was 170 cm and mothers, 159 cm. The number of children the parental generation have was approximately 2.4, whereas for those aged 45 and above at the time of the survey the complete fertility was 2.6.

[19]Note that the positive values of the parental years of interrupted schooling implies a negative shock in parent schooling whereas a negative value of detrended years of schooling also indicates a negative shock.

4 Reduced Form Estimates

To provide evidence that interrupted education during the Cultural Revolution indeed affected the next generation's education, we estimate the following OLS regression relationship:

$$Edu^{c}_{ijyn} = \alpha_0 + \alpha_1 SchInt^{p}_{ijyn} + X^{'}_{ijyn}\alpha_2 + u_y + u_n + \varepsilon^{c}_{ijyn}, \quad (1)$$

where the superscripts c and p index the child and the parent (either mother or father), respectively; Edu^{c}_{ijyn} is an educational attainment measure of the parent j's child i, who was born in year y and resided in city n; $SchInt^{p}_{ijyn}$ is a variable capturing the level of school interruption the parent j experienced during the CR; X_{ijyn} is a vector of control variables including gender of the child and the parent, the type of parent-child matching based on the definition provided in footnote 11, and a set of dummy variables indicating the data sources; u_y and u_n represent child's birth cohort and residential city fixed effects, respectively; and ε^{c}_{ijyn} is the child-specific error term. α_1 is the coefficient of interest which measures the relationship between parent school interruption during the CR and the education achievement of the child.

Equation 1 is estimated using the whole sample of parent-child pairs, as well as for father-child and mother-child pairs, separately. The main variable $SchInt$ is measured in two ways: the 'years of missed schooling' and the detrended years of schooling. The results are reported in the first two panels of Table 3. Note that as the variable 'years of missed schooling' only varies across parental birth cohorts, and there are only 23 parental birth cohorts in the total sample, the standard errors presented in the table are obtained using the few-cluster cluster robust variance estimate (CR2VE, the SEs are presented in the square brackets) and the Wild bootstrap methods (the p-value for the significant level of the coefficients using this method is presented in the parentheses) (Cameron et al., 2008; Angrist and Pischke, 2009; Cameron and Miller, 2015).

Panel A of Table 3 presents the results from using parental years of missed schooling to measure schooling interruption during the CR. The results suggest that if a parent had 1 additional year of interrupted schooling during the CR, her child's years of schooling is reduced by 0.04 years and their probability of going to university is reduced by 0.5 percentage points. Thus, for the parent cohort which were affected the most, with 8 years of schooling interruption, their children's education is reduced by 0.32 years and the probability of going to university by 4 percentage points. The results for mother and father separately show that in general the effect seems to be larger from mothers to children than that from fathers to children. The results for father-child pairs using either CR2VE or Wild bootstrap methods are not precisely estimated, but the magnitudes are comparable to the results observed for the whole sample. However, using Donald and Lang (2007) two-step estimation, the estimates for father-child pairs are also statistically significant at the 5% or 10% levels (see Appendix Table A.3).[20]

Panel B of Table 3 reports results from using city-cohort detrended years of schooling to measure parental schooling interruptions.[21] For this variable, the shock of schooling interruption during the CR on parental education attainment are represented as negative values. Thus, we observe positive coefficients. All the coefficients on parental schooling interruption are statistically significant. Before discussing the magnitude of the coefficients it is important to note that because 'years of missed schooling' and 'detrended years of schooling' measure the school interruption in different ways and have different distributions, the magnitudes of the coefficients in Panels A and B are not directly comparable. To compare the two sets of the results, we multiply the estimated coefficients by the standard deviations of the two measures of schooling interruption. The results after these transformations can then be compared. Thus, the results from Panel A suggest that one standard deviation increase in parental years of missed schooling reduces a child's education by 0.08 years, while the result from Panel B shows that one standard deviation reduction in detrended parental years of schooling reduces a child's schooling by 0.10 years. For university degree obtainment, the effects of one standard deviation change in both measures of school interruption reduce children's probability of obtaining a university degree by 1 percentage point.

These results indicate that parental schooling interruptions during the CR are indeed related to their children's education achievement. What is not clear, though, is whether the parental schooling interruption measures used here are proxies for other adverse parental experiences during the CR, such as the Sent-Down Youth experience, the

[20] Wooldridge (2003) and Angrist and Pischke (2009) suggest touse the two-step regression proposed by Donald and Lang (2007) to correct the bias in standard errors when the number of clusters is small - approximately 50 or above is viewed as large enough (Angrist and Pischke, 2009; Cameron and Miller, 2015).

[21] The standard errors reported for this set of regressions are clustered at the cohort-city level. We also cluster standard errors at the city level. There are only trivial differences and the standard errors become even smaller in some cases.

violence, and the general experience of having gone through such large political upheavals at a young age. To examine this issue, we conduct the following tests:

First, we examine whether the correlation between parental missed schooling and their children's education captures the parental cohort Sent-Down Youth experience. We include parental SDY experience in Equation 1 in addition to the parental 'years of missed schooling' variable to detect whether it will take away the impact of parental missed schooling on children's education achievement. Using the two data sets which have information on parental SDY experience, we estimate Equation 1 with and without the SDY indicator (see Panel A of Table 4). We report in the first two and next two columns the coefficients for parental 'years of schooling missed' without and with controlling for SDY experience, respectively. The results show that SDY experience is associated positively with children's schooling. That is, the SDY experience actually increases the child's education outcome. It also shows that SDY may be correlated with parental schooling missed. When the SDY variable is included, the coefficient for parental 'years of missed schooling' variable is increased. Thus, without controlling for SDY the coefficient on parental missed schooling may be biased downward. The results from using 'detrended parental years of schooling' to measure parental schooling interruption during the CR are consistent with this finding (see columns 5-8 of Panel A). The issue of why parental SDY experience has a positive impact on children's education achievement deserves a separate study. Perhaps SDY experience changed individuals' perception about education, which, in turn, affected their effort towards helping their children (Liu, 2016).

Next, we test whether the relationship between parental missed schooling and children's education achievement may be a proxy for parental experience of violence and chaos during the early onset of the Cultural Revolution. The potential concern is that experiencing violence at different ages may have a differential effect on shaping individuals' social preference, which, in turn, may affect their attitude towards their children's education (Voors et al., 2012). We include variables which capture the severity of the CR during the early chaotic years at the city or provincial level in addition to the parental schooling interruption variables in Equation 1.

The CR severity variables include (1) an estimate of the number of unnatural deaths from political persecution between 1968-1970 for the city; (2) an estimate of the number of victims from the political persecution during 1966 to 1971 in the city; and (3) the lowest industrial growth rate relative to the three pre-CR years for the region. As discussed in the last section, all these variables are adjusted by individuals' age at those years to address the issue of the most important developmental period when one's preference and behavior may be affected (see Section 3 for detailed discussion as to how the variables are adjusted/weighted).

The results of these estimations are reported in Panels B and C of Table 4. Panel B shows that although the interaction terms between the lowest industrial growth rate and the dummy variables indicating the parental age at the year of the lowest industrial growth (total of 23 interaction terms) are jointly significant, including these interaction terms does not reduce the impact of the parental schooling interruption variables on children's education achievement. If anything, as a result of including them, the relationship became even stronger with slightly larger coefficients. The same results are obtained when we control for death and victim variables, though only in one occasion out of four is the death variable statistically significant, while the victim variable is never significant.

Finally, we test whether narrowing the age spread of the cohorts to those who were born between 1944 and 1953, three cohorts before and seven cohorts during the schooling interruptions make a difference. The idea is that if there are other CR effects which affect people of different age differentially and which may be captured by schooling interruption variables, by narrowing the cohorts in the sample we can further weaken these potential heterogenous CR effects. In particular, the cohorts which were affected by schooling interruptions (birth cohorts of 1947-1953) were all in the critical age of development (teenage, aged 13-19). We control for SDY variable in Panel D and then add in the deaths, the victims, and interaction term of age with the lowest growth year during the CR variables in Panel E. For these narrower cohorts the relationship between parental missed schooling and children's education achievement are larger than for the whole sample.

The above tests seem to suggest that although parental CR experience may have affected children's education achievement in many different ways, the impact of parental interrupted education during the CR is largely independent of other factors.

5 Mediating channels between interrupted schooling and children's education

The results above show that parental interrupted schooling during the CR has had a significant negative impact on their children's education. The question naturally arises as to through which channels such an effect is transmitted to their children.

The immediate impact of parental education interruption during the CR should be on their own education achievement, which, in turn, should have direct and indirect impacts on their children's education, perhaps through lack of ability to supervise children's study, lack of positive attitude towards education, lack of health knowledge, assortative mating (marrying less-educated spouses with lower income and less knowledge on how to bring up children), or through lack of resources (income, good job, hence, good connections) to provide adequate education input to their children (e.g., Rosenzweig and Wolpin, 1980; Pischke and von Wachter, 2008; Janet and Moretti, 2003; .etc) But schooling interruption during the CR could also affect children's education through other channels rather than parental educational achievement, such as parental health (learning bad health habits during schooling interruption years) and age of marriage and hence fertility (those who stopped school earlier often marry earlier and may have more children as a result). These factors could potentially affect children's education due to lack of income (bad health affects earnings ability) or quality-quantity tradeoff (earlier marriage, higher fertility, and more children to compete for limited resources) (Black et al., 2003; Black and Devereux, 2011; Piopiunik, 2014). To this end, we examine these potential channels in addition to parental education empirically to see how parental schooling interruptions during the CR may have affected children's education through one or more of these channels. Such a test could also validate whether the parental interrupted education during the CR could be used as a valid instrument for parental completed education in the analysis of intergenerational education transmission, which we are also interested in. Specifically the following equation will be estimated:

$$Y^p_{jn} = \beta_0 + \beta_1 SchInt^p_{jn} + X^{'}_{jn}\beta_2 + u_n + \varepsilon^p_{jn}, \tag{2}$$

where Y^p_{jn} refers to parent j's outcome variables, including education, self-assessed health, height, and number of children; X_{jn} is a vector of control variables including parental age and its squared term, parent years of schooling (only included in the health and fertility regressions), a dummy for parental gender, a dummy for whether the parent was residing in the address, and dummies for surveys; ε^p_{jn} is the parent-specific error term; whereas u_n

represents the city fixed effects as in Equation 1.[22] As Equation 2 investigates outcomes of the parental generation, we make some adjustments for the sample used: (1) for those parents who have more than one child and hence counted more than once in estimating Equation 1, only one observation is kept; (2) all individuals who have urban *hukou* and valid education and birth cohort information are included regardless of whether they have children, their children's age, and whether there are missing values for children's information; and (3) the birth cohorts used are extended to 1940-1970.

The parental education achievement is measured as years of schooling and whether the parent has a university degree or not. Measures of parental health are available in two data sources: the CHIPS and CULS. Both surveys ask interviewees to rate their own heath relative to their own age group in five categories: 1. Very good; 2. Good; 3. Fair; 4. Bad; 5. Very bad. Using this variable we generate a dummy variable – healthy – which equals 1 if the respondent answers very good or good, and 0 otherwise. The number of children parents had is available in CULS and CEES surveys.

The results are reported in Table 5. The first four columns of the table report the parental schooling interruptions on their final school achievement, with the first two columns using parental years of schooling as the dependent variable and Columns 3 and 4 using parental university attainment as the dependent variable. The number of years of missed schooling has a negative and significant impact on both measures of parental schooling achievement. Every additional year of missed schooling during the CR reduces parental final years of schooling by 0.1 years and reduces their chance to go to university by 0.8 percentage points. Given that in our sample only 4.4% of parents have a university degree, this is a sizeable effect. The detrended years of schooling variable indicates that if a city-cohort cell has one year less education than the general trend, the final education years of parents who belong to that city-cohort cell would be reduced by 0.6 years and his/her chance of going to university would reduce by 1.2 percentage points. Thus, the schooling interruptions during the CR have had a large, negative, and strong impact on parental final education achievement.

Columns 5-6, 7-8, and 9-10 of Table 5 report the results of parental schooling interruption on their self-assessed health, height, and fertility, respectively. As can be seen from the table, after controlling for parental education (years of schooling), neither

[22]Note that the subscripts i and y as well as the child's birth cohort fixed effects u_y are dropped in the model, because the dependent variables are parental outcomes.

parental years of missed schooling nor detrended parental years of schooling has a significant and/or economically meaningful relationship with either of these outcomes. These results suggest that parental interrupted education have direct effect on parental educational achievement, but no independent effect on the other outcome variables we consider that might impact on children's education.

6 The intergenerational education transmission

Section 5 suggests that the most important channel through which parental schooling interruptions during the CR affected children's education achievement is through parental final education achievement. Assuming that parental education is the only pathway between parental schooling interruptions during the CR and children's education,[23] in this section we use the 2SLS method to more concisely estimate the impact of interrupted education during the CR on education of their offspring through the reduction on parental education achievement.

The reduced form estimation provided in Section 4 is important in understanding the average overall effect of the schooling interruption during the CR on children's education. Our search for the exact channels seem to conclude that such an impact is mainly through parental final education achievement: parents whose education was interrupted during the CR on average have fewer final years of education, which, in turn, negatively affected their children's education. However, not all individuals whose schooling was interrupted during the CR reduced their final education achievement. Hence, the effect estimated from the reduced form estimation should be a weighted average effect of parental schooling interruption on children's education among those parents whose final education was affected (compliers) and those whose final education was not affected (always takers and never takers) by the interruption. While the reduced form estimate is important and interesting, it is not a precise estimate of how much parental education reduction, due to interrupted education during the CR, affected the education of children whose parents' final education level was reduced because of the schooling interruption. Such an estimation should be more policy-relevant than the reduced-form estimates due to two reasons. First, it can help us to understand the cost of the education interruption during the CR to a particular group of the cohort whose final educational achievement is sensitive to

[23]We are fully aware that this is an untestable assumption.

the adverse shocks, likely to be a disadvantaged group (Meng and Gregory, 2002). The long-term bearing of an adverse event on a particular group of the society should be of significant distributional relevance. Second, it can also help us to understand the causal intergenerational education transmission, the policy relevance of which has been well documented in the literature (Black et al., 2005; Black and Devereux, 2011; Behrman and Rosenzweig, 2002; Carneiro et al., 2013). Given the large-scale of the CR impact, it is likely that the causal effect estimated here should also have a general implication.

In this section, we conduct a 2SLS estimation of the intergenerational transmission of education, using schooling interruption during the CR as the instrument. Specifically, we estimate:

$$Edu^c_{ijyn} = \delta_0 + \delta_1 Edu^p_{ijyn} + X'_{ijyn}\delta_2 + u_y + u_n + \varepsilon^c_{ijyn}, \tag{3}$$

$$Edu^p_{ijyn} = \theta_0 + \theta_1 SchInt^p_{ijyn} + X'_{ijyn}\theta_2 + u_y + u_n + \varepsilon^p_{ijyn}. \tag{4}$$

where Edu^p_{ijyn} is parent j's final years of schooling, and the other notations are the same as in Equation 1. Hopefully the analysis conducted in the last two sections has, to some extent, convinced the reader that the schooling interruptions during the CR can act as a valid instrument. Later in this section we will conduct further robustness checks on this issue.

6.1 *The Results*

Selected results of the OLS, first stage, and the IV estimations are reported in Table 6.[24] Columns 1-3 and 4-6 report results using children's years of schooling and university degree attainment as dependent variables, respectively. Panel A presents the OLS estimation of Equation 3 and standard errors presented in this Panel are clustered at the parents-children (family) level.[25] Columns 1 and 4 present the results for all parent-child pairs.

[24]Note that using parental 'years of missed schooling' during the CR as the instrument we face the same problem of having small numbers of clusters. Even though the few-cluster cluster robust variance estimate has been well developed in OLS (Cameron and Miller, 2015), it is not clear yet in the IV estimation how the issue should be dealt with. In the overview of cluster-robust inference (Cameron and Miller, 2015), the only discussion about few cluster in IV is: "We speculate that if additionally there are few clusters, then some of the adjustments discussed in Section VI (*about few-cluster in OLS*) would help." However, because in the reduced form estimation we find that the usual cluster-robust standard errors do not deviate far from those few-cluster cluster robust ones (CR2VE), the IV estimation below will use the usual clustering. Using 'detrended years of schooling' as the IV, however, is not subject to this problem.

[25]In the literature on intergenerational education transmission some studies, e.g., Farré et al. (2012) put both parents' education into the regression; while others, e.g., Black et al. (2005) only put one parent's education into the regression. The reason for the latter specification is due mainly to assortative mating which causes high multicollinearity between parents' education. This is also the case for our paper. Here

They show that for each additional year of schooling a parent receives, there is an increase in child schooling of 0.23 years. An additional year of parental schooling also increases the child's probability of obtaining a university degree by 1.9 percentage points. When the sample is split into father-child pairs (Columns 2 and 5) and mother-child pairs (Columns 3 and 6), we observe that the intergenerational transmission coefficient between father's and child's years of schooling is slightly larger than that between mother and child and the same pattern is observed for university degree attainment.

Relative to other countries, the intergenerational education transmission coefficients estimated here seem low. Hertz et al. (2007) list coefficients for 42 countries and find that the coefficient for Brazil, the U.K, the U.S., and Norway are 0.95, 0.71, 0.46, and 0.40, respectively. Even for rural China, their results show a coefficient of 0.34.[26] Our 0.23 estimate for urban China would be ranked at the very bottom of their list. Part of the reason may be related to the particular cohorts used in this study.

First, as we mentioned in the background section, during the CR, students might be granted a diploma even though they missed all or part of the school education; and when at school the curriculum also deviated from normal. Both of these factors imply lower 'quality' of education for parents affected. Assume that the intergenerational transmission of education is partially driven by a better child-rearing skill an educated parent possesses, parents with lower quality of education at a given level of education should have lower intergenerational transmission than normal. Put it another way, if we assume that each year of schooling is not only a quantity measure, but also possesses a certain unit of the quality of education, then the same years of education with less quality can be regarded as years of schooling with measurement error. As measurement errors generate under-estimate in the OLS regression, we should observe a lower transmission coefficient.

Second, assume that the intergenerational transmission of education is partially driven by innate ability, smart parents and children both have more education because they are smart. For the parents in our sample, who experienced school interruption during the CR, however, their education was abruptly stopped regardless of their smartness. Thus, smarter parents may have more shortened education relative to what they would have achieved. Their children, on the contrary, did not experience this interruption and

we pool parent-child together to increase the sample size, but the cost is that we might underestimate the standard error if we do not take into account the intra-family correlation. Therefore, we adjust the standard error by clustering at the family level.

[26]The schooling interruption during the CR only occurred in urban China (Meng and Gregory, 2007).

obtained education levels based on their ability. Naturally, the variation of education among parents is smaller than among the children cohorts, and hence the transmission coefficient between parental education and children's education for the CR cohort would be lower than if the CR had not occurred. We formally prove that in the case where an exogenous shock has a heterogenous effect over the ability distribution, the OLS estimation may be lower than the case when such heterogenous effects do not exist (see Appendix C).

Panel B of Table 6 reports the first-stage results. Columns 1 and 4 are the results using the total sample. Similar to the results presented in the first two columns of Table 5, parental schooling interruption during the CR has a sizeable negative and statistically significant effect on parental final educational achievement. For the sample of parents matched with children who were born after 1965, the magnitudes of the coefficients are larger. The rest of the columns present fathers' and mothers' results separately, indicating that schooling interruptions have larger effect on fathers' completed education than on the mothers'. At the bottom of the panel, we also show the F-test for the strength of the instrument. All six estimates passed the strong instrument rule-of-thumb test.

The next two panels (Panels C and D) report the IV results using the parental 'years of missed schooling' and the 'detrended years of schooling' by city and cohort as the IV, respectively. In general, the results using the two sets of IVs are largely consistent. The results show that every 1 year lost in parent schooling, due to the school interruption during the CR, reduces the child's years of schooling by around 0.31 years (Column 1 of Panels C and D in Table 6), and reduces the child's probability of completing university by 3.1 to 4.4 percentage points (Column 4 of Panels C and D in Table 6).

In the last panel (Panel E) we use both parental 'years of missed schooling' and the 'detrended years of schooling' as the instruments at the same time. The estimated results are quite close to those using the two instruments separately; and we also show that they pass the over-identification test.

The education transmission coefficient seems to be much larger for mother-child pairs than for father-child pairs. Columns 2 and 3 of Table 6 indicate that one year reduced schooling for a father due to the schooling interruption during the CR reduces his child's schooling by 0.2 years, whereas the effect from mother to child is double the father's effect to 0.4 years.[27]

It is noticeable that the IV estimates are larger than the OLS estimates. For years of schooling, the IV results are around 40% higher for the total sample and up to 83% higher for mother-child pairs. For university degree attainment, the difference is even larger, ranging from 83% higher for the total sample and up to 244% higher for mother-child pairs. Why are the IV estimates larger? If the unobserved children's ability is expected to be positively correlated with parental education, the OLS method, which does not take into account this correlation, should over-estimate the true causal intergenerational transmission coefficients. Our results are the opposite. Previous studies using changes in the Compulsory Schooling Law (*CSL*) as instruments (see for example, Oreopoulos et al., 2006) also found larger IV estimates than OLS results. They offer two explanations: (1) the measurement error effect dominates the bias generated by the omitted variables, and hence, the OLS estimates may be an under-estimate instead of an over-estimate; (2) the changes in the *CSL* affected individuals at the bottom end of the educational attainment distribution, where the marginal effect of education should be higher (Angrist and Krueger, 1991; Oreopoulos et al., 2006; Harmon and Walker, 1995; Staiger and Stock, 1997; Oreopoulos, 2007).

In our case, the higher IV estimates relative to the OLS estimates may be related to the following reasons. First, the measurement error in parental education generated by the nature of schooling interruptions during the CR will cause the OLS to be an under-estimate (see Sub-section 2.1).

Second, also a point mentioned before, the potential heterogenous effect of school interruption during the CR on educational achievement of parents with different ability could also induce the OLS estimation to be an under-estimate of the intergenerational transmission coefficient (see Appendix C for detailed explanations). This can be labeled the first-stage heterogeneity.

The third reason is related to the Local Average Treatment Effects (LATE) interpretation of the IV estimates. Based on this interpretation, if the treatment effect is heterogeneous (the second-stage heterogenous effect), then IV estimates indicate the treatment effect for the sub-group of individuals whose treatment status is changed by the instrument.

[27]The literature has mixed findings regarding whether mothers' or fathers' education has a larger effect depending on countries and population studied as well as methodology used (see, for example, Behrman and Rosenzweig, 2002; Black et al., 2005; Sacerdote, 2007).

Thus, our IV estimates indicate the effect of intergenerational education transmission for the group whose parents' final education levels were reduced because their education was interrupted during the Cultural Revolution (the compliers).[28] In particular, this group does not include three types of parents: those who had finished their education when the CR began in 1966 (the pre-CR cohort), those who re-invested in their education after the CR (always-takers), and those who would not have had a different level of education had the CR not occurred (never-takers). For these three types of parents, their final educational attainments were not affected, even though they also experienced school interruption during the CR.

It may be the case that the compliers in our sample are more likely to be those who have relatively high ability than never-takers and would not have stopped their education had the schooling interruption not occurred. If so, combining the potential first-stage heterogenous effect with that of the second-stage heterogenous effect, we can say that our IV estimates identifies the compliers for whom the OLS estimates are likely to be more under-estimated than the never-takers. If at the same time this group should in fact have higher intergenerational transmission of education, as identified by the second-stage heterogenous effect, then the difference between the OLS and IV should be larger. Note that the literature on LATE interpretation of the IV estimation has not discussed the potential first-stage heterogenous effect before, but we believe that in our case this may be the main driver for the larger IV estimates.

6.2 *Robustness tests*

The IV results presented above assume that our instruments are valid. Even though Sections 4 and 5 have examined whether the impact of parental schooling interruptions on children's education captures other CR effects and whether the schooling interruptions affected other parental outcomes and through which affected children's education, further robustness tests are still needed.

In Section 4 we show that parental SDY experience is related to children's education achievement positively and controlling for it further strengthens the negative relationship between parental schooling interruption and children's education achievement. In this

[28]In the classical LATE context, the instrumental variable and endogenous variable are both binary. However, Imbens and Wooldridge (2007) point out that the LATE interpretation is also applicable to the continuous instrumental/endogenous variable case.

sub-section we first investigate whether direct control for sent-down youth experience will alter our IV estimates of intergenerational education transmission coefficients. The results are presented in Panel 1 of Table 7. We observe no change in intergenerational transmission coefficient for either the years of schooling or the university attainment equations when we control for parental sent-down youth experience. If anything, the estimated parental education impact with controlling for sent-down youth experience is larger than that without such a control.

Second, we test whether another group of omitted variables could be that different levels of CR severity differentially affected individuals at different ages, which, in turn, affected children's education. We include the CR severity measures (unnatural death and victim measures, as well as the lowest industrial growth rate during the initial period of the CR) used in Section 4 to the IV estimation in addition to SDY control. The results are presented in Panel 2 of Table 7, which show that such controls do not change the estimated intergenerational transmission coefficient.

Third, we test for other potential parental omitted variables which may be affected by their schooling interruptions during the CR, and which, in turn, may have a direct impact on children's education. To this end, we add parental health indicator variables (self-assessed health and parental height) and parental fertility, which may be correlated with parental schooling interruption and may also affect children's education through the income channel and/or quantity-quality trade-off channel. It is important to note, though, that to the extent that these factors were affected by parental education, they should be regarded as the total parental education effect, rather than an independent effect on children's education. However, to be thorough in controlling for any potential violation of exclusion restriction of our IV we try to directly control for these variables. Panels 3-5 of Table 7 show that controlling for these measures do not change the estimated impact of parental education on children's education.

The next test we conduct is on the issue of defiers. One important assumption for the LATE interpretation of IV results is the monotonicity assumption; that is, there are no defiers in the data. In our case, this requires that in the sample no individual's education level was increased because of the school interruption during the CR. As discussed in Section 2 and in Meng and Gregory (2002), there were a small group of individuals who went to college or university during the CR largely because of party loyalty and family 'class' background. These people, although not directly due to the interrupted schooling during the CR, were able to go to college or above education because of the CR. They may constitute a defier group. Fortunately, in two of our data sources we have information on when the individual obtained their degree or admitted to university. Using that information we can identify the number of potential defiers. Here, we test whether our results are sensitive to the exclusion of these individuals who went to the university during the Cultural Revolution (between 1966 and 1976). The results are shown in Panel 6 of Table 7, and as can be seen, no difference is observed excluding this group of individuals.

In Panel 7 of Table 7 we use the a narrow parental birth cohort (1944-1953), hoping to further weaken the potential heterogeneous other CR effects on individuals at different age. The main estimation results remain the same as in the whole sample, except that when using 'detrended years of schooling' as the IV, the estimate for child's university degree becomes insignificant, but the magnitude remain at the same level.

Finally, we estimate the model with parental cohort fixed effects. Given that our first instrumental variable - parental 'years of missed schooling' - only varies across cohorts, controlling for parental cohort fixed effects is not feasible, this exercise will only be conducted using the 'detrended years of schooling' as the IV. The results are reported in Panel 8 of Table 7. The OLS results remain almost the same. For IV estimations the magnitude become larger, while standard error also increased. When using children's years of schooling as the dependent variable the result remains statistically significant, whereas the result in the university attainment equation is not precisely estimated. Overall, this final test should be more convincing as any additional potential confounding factors should be controlled for in the cohort fixed effects.

7 Conclusions

This paper examines one aspect of the long-term adverse effect of the Chinese Cultural Revolution: the impact of parental interrupted education during the CR on their children's educational achievement. We show that 1 additional year increase in parental schooling interrupted during the CR reduces their children's education by 0.04 years. For the birth cohort of parents whose education were interrupted by 8 years, their children's schooling on average were reduced by 0.32 years, which is 11% of a standard deviation for the total sample of children, and 13% for the children of the cohort. For university attainment, an 8-year reduction in parental education reduces their children's chance to obtain a university degree by 4 to 4.8 percentage points, which is a 22% to 27% reduction relative to the average university attainment for children.

We tested whether the measures of the school interruptions during the CR were in fact proxies for other types of adverse parental experiences during the CR, such as the sent-down youth experience, and the severity of the CR in their city when they were at particularly sensitive ages. We find that directly controlling for these experience does not reduce the impact of schooling interruption on children's schooling achievement, suggesting that parental schooling interruption during the CR has strong and independent effect on children's final schooling achievement.

We further tested through which channels did the parental schooling interruption affected children's education. Here we examined outcomes including parental education achievement, parental health, as well as parental fertility (the quality-quantity tradeoff channel). The results suggest that the most important channel is through parental educational achievement. Controlling for parental final education achievement, none of the other outcome variables is affected by parental schooling interruption during the CR.

Based on these analyses, we went a step further to examine how parental interrupted education affected intergenerational transmission of education among the group of children whose parents' final education level were actually affected, which should be a fairly large group among the CR cohort. Using 2SLS estimation we find that if a parent's schooling was reduced by 1 year because of school interruption during the Cultural Revolution, the child's schooling was reduced by approximately 0.31 years; and the child's probability of obtaining university degree is 3.1-4.4 percentage points lower than if the Cultural Revolution did not occur. These are the local average treatment effects for individuals whose final education achievement was reduced because of the CR (the compliers). These are large effects. On average the CR cohort parents had 2.9 years interrupted education. If they failed to catch up after the CR, this translates to a reduction of a 0.87 years of schooling and a 9 percentage points (or 50%) reduction in probability of going to university for their children relative to the children of parents who did not have interrupted schooling during the CR. For the compliers in the cohort which had the most number of years schooling interrupted (8 years), their children's years of schooling is cut short by 2.4 years and their probability of going to university is reduced by 24 to 30 percentage points, which completely wiped out their chance of going to university.

Our results confirm that the Cultural Revolution has had a long-lasting large adverse effect not only on the cohort themselves but also on the second generation. Although the Cultural Revolution came to a halt in the late 1970s and China has since embarked on a much-needed economic reform and modernization process, political upheavals, wars, and conflicts are still far from being eradicated in the world. It is important to remember that any adverse impact on human capital accumulation of the current generation may have long-lasting impact on many generations to come.

Table 1: Schooling interruption during the Cultural Revolution

Birth year	Age in 1966	Years of schooling in 1966	University entry age	No. of years missed in primary school	No. of years missed in junior high school	No. of years missed in senior high school	Total No. of years missed in schools	No. of years delayed university/ college entry
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1963	3	0	19	1	0	0	1	0
1962	4	0	19	2	0	0	2	0
1961	5	0	19	3	0	0	3	0
1960	6	0	19	4	0	0	4	0
1959	7	0	19	5	0	0	5	0
1958	8	1	20	5	1	0	6	1
1957	9	2	21	4	2	0	6	2
1956	10	3	22	3	3	0	6	3
1955	11	4	23	2	3	3	8	4
1954	12	5	24	1	3	3	7	5
1953	13	6	25	0	3	3	6	6
1952	14	7	26	0	2	3	5	7
1951	15	8	27	0	1	3	4	8
1950	16	9	28	0	0	3	3	9
1949	17	10	29	0	0	2	2	10
1948	18	11	30	0	0	1	1	11
1947	19	12	31	0	0	0	0	12

Source: Meng and Gregory (2002)

Table 2: Descriptive statitics

	Father-child			Mother-child			Parent-child		
	Mean	S.D.	N	Mean	S.D.	N	Mean	S.D.	N
Outcomes and demographic variables									
Parent's years of schooling	9.45	3.70	4910	8.13	3.84	6203	8.71	3.84	11113
Parent's university degree attainment	0.07		4910	0.02		6203	0.04		11113
Parent's age	58.27	3.98	4910	57.19	4.32	6203	57.67	4.21	11113
Child's years of schooling	12.63	2.84	4910	12.62	2.82	6203	12.62	2.83	11113
Child's university degree attainment	0.19		4910	0.19		6203	0.19		11113
Child's age	31.04	4.10	4910	31.57	4.21	6203	31.34	4.17	11113
CR related variables (CR Cohorts)									
Years of missed schooling	2.55	2.19	1755	3.06	2.33	2852	2.87	2.29	4607
Detrended years of schooling by Cohort-City	-0.25	0.37	1755	-0.27	0.37	2852	-0.27	0.37	4607
Lowest Ind. growth rate druing CR relative to pre CR Ind. output (%)	0.37	23.14	1755	0.10	22.40	2852	0.20	22.68	4607
Weighted deaths during the CR (per 1000 prersons)	0.65	1.34	1442	0.67	1.39	2347	0.66	1.37	3789
Weighted victims during the CR (per 1000 persons)	24.91	47.99	1437	26.41	48.22	2343	25.84	48.13	3780
Matching types									
HH heads/spouses matched with their parents	0.63		4910	0.60		6203	0.61		11113
HH heads/spouses matched with their children	0.36		4910	0.38		6203	0.37		11113
Other matched parent-child pairs	0.02		4910	0.02		6203	0.02		11113
Surveys									
UREES	0.58		4910	0.57		6203	0.57		11113
CHIPS	0.25		4910	0.22		6203	0.23		11113
CULS	0.17		4910	0.21		6203	0.19		11113
Other parental chracteristise and outcomes									
Sent-Down Youth experience (total sample) (CHIPS & CULS)	0.06		2077	0.09		2671	0.08		4748
Sent-Down Youth experience (CR Cohorts) (CHIPS & CULS)	0.15		578	0.18		1073	0.17		1651
Healthy (CHIPS & CULS)									
Proportion with very good or good health	0.26		2077	0.23		2671	0.24		4748
Height (CULS) (cm)	170.67	4.88	556	159.33	4.49	935	163.56	7.18	1491
Number of children (UREES & CULS)	2.33	1.25	2665	2.42	1.24	3570	2.38	1.25	6235

Note: The CR Cohorts are the 1947-1963 birth cohorts.

Table 3: Reduced form results

	Children's Years of Schooling			Children's University Degree		
	Parent-child	Father-Child	Mother-Child	Parent-child	Father-Child	Mother-Child
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Parental years of missed schooling	-0.037**	-0.034	-0.043**	-0.005**	-0.006	-0.006***
	[0.018]	[0.025]	[0.020]	[0.002]	[0.005]	[0.002]
	(0.056)	(0.238)	(0.046)	(0.050)	(0.254)	(0.010)
Dummy for daughter	-0.224***	-0.243***	-0.209**	-0.038***	-0.035**	-0.040***
	[0.073]	[0.079]	[0.086]	[0.010]	[0.014]	[0.013]
Dummy for mother	0.070			0.002		
	[0.044]			[0.006]		
Observations	11113	4910	6203	11113	4910	6203
R-squared	.106	.116	.103	.049	.054	.052
Number of clusters	23	23	22	23	23	22
Panel B						
Detrended parental years of schooling	0.200***	0.179*	0.224**	0.020**	0.019	0.021*
	[0.072]	[0.100]	[0.090]	[0.010]	[0.014]	[0.012]
Dummy for daughter	-0.206***	-0.227***	-0.190***	-0.035***	-0.033***	-0.037***
	[0.055]	[0.080]	[0.073]	[0.008]	[0.012]	[0.010]
Dummy for mother	0.068			0.001		
	[0.050]			[0.007]		
Observations	11113	4910	6203	11113	4910	6203
R-squared	0.105	0.115	0.102	0.048	0.052	0.050
Number of clusters	1282	1047	1169	1282	1047	1169

Notes: All regressions control for city fixed effects, children's birth cohort fixed effects, dummies for different surveys and dummies for different type of parent-child matching. Robust standard error in square brackets, which are few-cluster cluster robust (CR2VE) for panel A and are clustered at the cohort-city level for Panel B.The numbers in parentheses below standard errors of parental years of missed schooling in Panel A are the Wild bootstrap p-values for few-cluster regressions. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table 4: Reduced form results (extra tests)

	Parental years of missed schooling during the CR				Detrended parental years of schooling by Cohort-City			
	No additional control		With additional control		No additional control		With additional control	
	Child Ysch	Child Uni	Child Ysch	Child Uni	Child Ysch	Child Uni	Child Ysch	Child Uni
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Sample with measures of Sent-Down Youth experience								
Parental interrupted schooling	-0.078*	-0.011**	-0.091*	-0.013**	0.457***	0.043***	0.512***	0.050***
	[0.045]	[0.005]	[0.048]	[0.006]	[0.103]	[0.015]	[0.104]	[0.015]
	(0.106)	(0.056)	(0.066)	(0.042)				
Dummy for SDY			0.580***	0.076***			0.634***	0.079***
			[0.132]	[0.023]			[0.158]	[0.026]
Observations	4748	4748	4748	4748	4748	4748	4748	4748
R-squared	0.059	0.031	0.061	0.034	0.062	0.032	0.065	0.035
Number of clusters	20	20	20	20	633	633	633	633
Panel B: Sample with lowest ind. growth year interacted with age dummies at that year								
Parental interrupted schooling	-0.038**	-0.005**	-0.041**	-0.006**	0.200***	0.020**	0.213***	0.021**
	[0.018]	[0.002]	[0.017]	[0.002]	[0.072]	[0.010]	[0.071]	[0.009]
	(0.056)	(0.050)	(0.046)	(0.048)				
Lowest Growth×age dummies	No	No	Yes	Yes	No	No	Yes	Yes
Observations	11113	11113	11113	11113	11113	11113	11113	11113
R-squared	0.105	0.048	0.107	0.049	0.105	0.048	0.108	0.049
Number of clusters	23	23	23	23	1282	1282	1282	1282
p-value for joint sig. test			0	0			0	.0000475
Panel C: Sample with Deaths and Victims info. Weighted by number of year parents aged 7-20								
Parental interrupted schooling	-0.043*	-0.006**	-0.046*	-0.007***	0.226***	0.023**	0.224***	0.024**
	[0.022]	[0.002]	[0.026]	[0.003]	[0.078]	[0.010]	[0.081]	[0.011]
	(0.068)	(0.034)	(0.100)	(0.020)				
Deaths			0.072**	0.009			0.060	0.007
			[0.036]	[0.009]			[0.040]	[0.007]
Victims			-0.010	0.000			-0.010	-0.000
			[0.012]	[0.002]			[0.011]	[0.002]
Observations	9,160	9,160	9,160	9,160	9,160	9,160	9,160	9,160
R-squared	0.102	0.046	0.102	0.046	0.102	0.046	0.102	0.046
Number of clusters	22	22	22	22	948	948	948	948
Panel D: Narrow Cohorts (born 1944-1953) with control for SDY								
Parental interrupted schooling	-0.094**	-0.012	-0.112**	-0.013*	0.392***	0.046**	0.454***	0.052***
	[0.047]	[0.007]	[0.050]	[0.008]	[0.126]	[0.019]	[0.129]	[0.020]
	(0.080)	(0.148)	(0.056)	(0.116)				
Dummy for SDY			0.616***	0.059***			0.612***	0.058**
			[0.136]	[0.020]			[0.173]	[0.028]
Observations	2562	2562	2562	2562	2562	2562	2562	2562
R-squared	0.055	0.038	0.059	0.040	0.055	0.038	0.060	0.040
Number of clusters	10	10	10	10	315	315	315	315
Panel E: Narrow Cohorts (born 1944-1953) with control for SDY, lowest ind. growth×age dummies & deaths & victims								
Parental interrupted schooling	-0.094**	-0.012*	-0.138***	-0.013**	0.392***	0.046**	0.493***	0.051***
	[0.044]	[0.007]	[0.045]	[0.005]	[0.126]	[0.019]	[0.125]	[0.019]
	(0.088)	(0.130)	(0.100)	(0.150)				
Dummy for SDY			0.660***	0.062***			0.653***	0.062**
			[0.129]	[0.018]			[0.173]	[0.028]
Deaths			-0.152*	-0.015			-0.188*	-0.018
			[0.081]	[0.017]			[0.114]	[0.018]
Victims			0.052**	0.002*			0.038**	0.001
			[0.021]	[0.001]			[0.019]	[0.002]
Lowest Growth×age dummies	No	No	Yes	Yes	No	No	Yes	Yes
Observations	2562	2562	2562	2562	2562	2562	2562	2562
R-squared	0.055	0.038	0.066	0.045	0.055	0.038	0.066	0.045
Number of clusters	10	10	10	10	315	315	315	315

Notes: All regressions control for city fixed effects, children's birth cohort fixed effects, dummies for different surveys and dummies for different type of parent-child matching. Robust standard error in square brackets, which are few-cluster cluster robust (CR2VE) in Columns (1) to (4) and are clustered at the Cohort-City level in Columns (5) to (8).The numbers in parentheses in Columns (1) to (4) below standard errors of parental interrupted schooling are the Wild bootstrap p-values for few-cluster regressions. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 5: The impacts of school interruption on parental outcomes

	Parental Education				Parents being healthy		Parental height		Number of children	
	Years of schooling		University degree attainment							
	Years of missed schooling	Detrended years of schooling	Years of missed schooling	Detrended years of schooling	Years of missed schooling	Detrended years of schooling	Years of missed schooling	Detrended years of schooling	Years of missed schooling	Detrended years of schooling
	(1)	(2)	(3)	(4)	(7)	(8)	(9)	(10)	(11)	(12)
Parental interrupted schooling	-0.100***	0.619***	-0.008***	0.012***	-0.003	-0.003	-0.019	-0.142	-0.025	0.062
	[0.024]	[0.070]	[0.002]	[0.004]	[0.002]	[0.011]	[0.044]	[0.180]	[0.032]	[0.069]
Wild bootstraping p-value	(0.006)		(0.006)		(0.224)		(0.686)		(0.496)	
Observations	19,899	18,709	19,899	18,709	10,099	9,285	4,656	4,132	4,817	4,817
R-squared	0.188	0.168	0.040	0.026	0.253	0.240	0.570	0.569	0.271	0.271
Number of clusters	31	1787	31	1787	31	929	31	130	19	768

Notes: All regressions control for city fixed effects, dummies for different surveys, dummies for parental gender, parental age and the age squared, a dummy indicating whether the parent resides in the address or not. In the regressions on parental health, parental height and fertility, parental years of schooling is also controlled for. Robust standard errors in square brackets, which are based on few-cluster cluster robust variance estimate (CR2VE) when parental interrupted schooling is measured with years of schooling missed, and clustered at Cohort-City level when parental interrupted schooling is measured with detrended years of schooling. The numbers in parentheses below standard errors of parental interrupted schooling in odd-number columns are Wild bootstrap p-values for few-cluster regressions. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table 6: OLS and IV estimation results

	Parent-Child	Father-Child	Mother-Child	Parent-Child	Father-Child	Mother-Child
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: OLS						
	Child's Years of Schooling			Child Has a University Degree		
Parental years of schooling	0.229***	0.248***	0.218***	0.019***	0.021***	0.018***
	[0.009]	[0.011]	[0.010]	[0.001]	[0.002]	[0.001]
Observations	11113	4910	6203	11113	4910	6203
R-squared	0.185	0.204	0.175	0.077	0.085	0.076
Number of clusters	6299	4460	5541	6299	4460	5541
Panel B: First Stage						
	Parents' Final Years of Schooling					
Parental years of missed schooling	-0.123***	-0.164***	-0.105***			
	[0.030]	[0.035]	[0.035]			
Detrended parental years of schooling				0.641***	0.761***	0.561***
				[0.094]	[0.135]	[0.116]
Observations	11113	4910	6203	11113	4910	6203
R-squared	0.167	0.140	0.162	0.169	0.142	0.164
Number of clusters	23	23	22	1282	1047	1169
F-statistic for the strength of the IV	30.29	22.81	12.46	50.98	31.79	21.37
Panel C: 2SLS (IV=Years of missed schooling)						
	Child's Years of Schooling			Child Has a University Degree		
Parental years of schooling	0.310***	0.213*	0.414**	0.044***	0.038	0.057**
	[0.116]	[0.119]	[0.189]	[0.016]	[0.023]	[0.024]
Observations	11113	4910	6203	11113	4910	6203
Number of clusters	23	23	22	23	23	22
F-statistic for the strength of the IV	30.29	22.81	12.46	30.29	22.81	12.46
Panel D: 2SLS (IV=Detrended years of Schooling)						
	Child's Years of Schooling			Child Has a University Degree		
Parental years of schooling	0.312***	0.235*	0.399**	0.031**	0.024	0.038*
	[0.105]	[0.124]	[0.157]	[0.015]	[0.018]	[0.021]
Observations	11113	4910	6203	11113	4910	6203
Number of clusters	1282	1047	1169	1282	1047	1169
F-statistic for the strength of the IV	50.98	31.79	21.37	50.98	31.79	21.37
Panel E: 2SLS (IV=Years of missed schooling and Detrended Schooling)						
	Child's Years of Schooling			Child Has a University Degree		
Parental years of schooling	0.311***	0.228**	0.403***	0.035**	0.029**	0.044**
	[0.093]	[0.106]	[0.106]	[0.014]	[0.014]	[0.019]
Observations	11,113	4,910	6,203	11,113	4,910	6,203
Number of clusters	23	23	22	23	23	22
F-statistic for the strength of the IV	30.70	20.33	12.72	30.70	20.33	12.72
Overidentification test statistic	0	.012	.003	0.430	0.258	0.605
p-value	0.992	0.912	0.954	0.512	0.612	0.437

Notes: All regressions control for city fixed effects, children's birth cohort fixed effects, dummies for different surveys and dummies for different type of parent-child matching and dummies for parent's and child's gender. The instrumental variable in all IV estimations is parental detrended years of schooling by Cohort-City. Robust standard error in square brackets, which are clustered at family level for OLS and clustered at cohort level in the first stage regression and IV estimations when the instrumental variable is parental years of missed schooling or the two instrumental variables are used at the same time, and clustered at cohort-city level for the first stage regression and IV estimations when the instrumental variable is parental detrended years of schooling by Cohort-City. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 7: Robustness tests

	Child's Years of Schooling			Child's University Degree		
	OLS	IV: years of missed schooling	IV: de-trended years of schooling	OLS	IV: years of missed schooling	IV: de-trended years of schooling
	(1)	(2)	(3)	(4)	(5)	(6)
Paenl 1: Sent-Down Youth Test	No control for Sent-Down Youth experience					
Parental years of schooling	0.223***	0.309**	0.446***	0.019***	0.043***	0.042**
	[0.013]	[0.142]	[0.100]	[0.002]	[0.017]	[0.018]
F-statistic for the strength of the IV		41.76	53.69		41.76	51.16
	With control for Sent-Down Youth Experience					
Parental years of schooling	0.222***	0.327**	0.452***	0.019***	0.046***	0.046***
	[0.013]	[0.135]	[0.092]	[0.002]	[0.015]	[0.015]
Dummy for SDY	0.301*	0.196	0.071	0.049	0.023	0.023
	[0.169]	[0.124]	[0.168]	[0.032]	[0.023]	[0.029]
F-statistic for the strength of the IV		49.98	64.22		49.98	64.22
Observations	4748	4748	4748	4748	4748	4748
Number of clusters	2678	20	633	2678	20	633
Panel 2: SDY and CR Severity	No control for SDY and CR severity					
Parental years of schooling	0.228***	0.343***	0.391***	0.020***	0.044***	0.041***
	[0.014]	[0.117]	[0.101]	[0.002]	[0.016]	[0.014]
F-statistic for the strength of the IV		38.07	52.52		38.07	52.52
	With control for SDY and CR severity					
Parental years of schooling	0.226***	0.447***	0.457***	0.019***	0.053***	0.053***
	[0.014]	[0.131]	[0.101]	[0.002]	[0.014]	[0.019]
Dummy for SDY	0.354*	0.140	0.130	0.052	0.020	0.020
	[0.187]	[0.129]	[0.183]	[0.034]	[0.018]	[0.032]
CR Victims	0.944	2.020	2.067	-0.263	-0.099	-0.099
	[2.213]	[1.843]	[1.793]	[0.251]	[0.169]	[0.232]
CR Deaths	3.073	52.834	55.029	9.317	16.913	16.913
	[113.800]	[80.685]	[106.885]	[17.715]	[16.222]	[16.025]
Lowest Growth×age dummies	Yes	Yes	Yes	Yes	Yes	Yes
F-statistic for the strength of the IV		31.79	52.70		31.79	33.39
Observations	4259	4259	4259	4259	4259	4259
Number of clusters	2365	20	492	2365	20	492
Panel 3: Parental Health	No control for Parental Health					
Parental years of schooling	0.223***	0.309**	0.446***	0.019***	0.043***	0.042***
	[0.013]	[0.142]	[0.100]	[0.002]	[0.017]	[0.014]
F-statistic for the strength of the IV		41.76	53.69		41.76	53.69
	With control for Parental Health					
Parental years of schooling	0.223***	0.310**	0.446***	0.019***	0.044***	0.042***
	[0.013]	[0.142]	[0.099]	[0.002]	[0.017]	[0.014]
Dummy for parents being healthy	0.086	0.057	0.013	0.019	0.012	0.012
	[0.124]	[0.101]	[0.120]	[0.018]	[0.014]	[0.017]
F-statistic for the strength of the IV		42.09	54.20		42.09	54.20
Observations	4748	4748	4748	4748	4748	4748
Number of clusters	2678	20	633	2678	20	633

Continued on the next page.

Table 7 (continued)

	Child's Years of Schooling			Child's University Degree		
	OLS	IV: years of missed schooling	IV: detrended years of schooling	OLS	IV: years of missed schooling	IV: detrended years of schooling
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 4: Parental Height			No control for Parental Height			
Parental years of schooling	0.281***	0.664***	0.460***	0.028***	0.077**	0.053**
	[0.027]	[0.233]	[0.174]	[0.004]	[0.033]	[0.026]
F-statistic for the strength of the IV		12.08	21.74		12.08	21.74
			With control for Parental Height			
Parental years of schooling	0.281***	0.661***	0.460***	0.028***	0.075**	0.051*
	[0.027]	[0.231]	[0.172]	[0.004]	[0.032]	[0.026]
Parental height	0.003	-0.003	0.000	-0.002	-0.003	-0.003
	[0.018]	[0.021]	[0.018]	[0.003]	[0.003]	[0.003]
F-statistic for the strength of the IV		12.30	21.72		12.30	21.72
Observations	1,491	1,491	1,491	1,491	1,491	1,491
Number of clusters	676	19	77	676	19	77
Panel 5: Parental Fertility			No Control for Parental Fertility			
Parental years of schooling	0.240***	0.531*	0.294**	0.021***	0.074**	0.037**
	[0.012]	[0.288]	[0.122]	[0.002]	[0.036]	[0.018]
F-statistic for the strength of the IV		8.28	37.11		8.28	37.11
			With Control for Parental Fertility			
Parental years of schooling	0.233***	0.534**	0.316***	0.021***	0.067***	0.037**
	[0.012]	[0.208]	[0.112]	[0.002]	[0.024]	[0.017]
Total Number Parental Fertility	-0.153***	-0.005	-0.112*	-0.013***	0.010	-0.004
	[0.047]	[0.116]	[0.063]	[0.005]	[0.014]	[0.009]
F-statistic for the strength of the IV		14.15	45.40		14.15	45.40
Observations	6235	6235	6235	6235	6235	6235
Number of clusters	3420	23	931	3420	23	931
Panel 6: Excluding WPS Degree Holders						
Parental years of schooling	0.229***	0.315***	0.309***	0.019***	0.046***	0.031**
	[0.009]	[0.116]	[0.106]	[0.001]	[0.016]	[0.015]
F-statistic for the strength of the IV		30.62	51		30.62	51
Observations	11090	11090	11090	11090	11090	11090
Number of clusters	6292	23	1282	6292	23	1282
Panel 7: Narrow Cohorts 1944-1953						
Parental years of schooling	0.225***	0.277**	0.357**	0.019***	0.038**	0.031
	[0.011]	[0.117]	[0.142]	[0.002]	[0.016]	[0.020]
F-statistic for the strength of the IV		33.19	24.65		33.19	24.65
Observations	6854	6854	6854	6854	6854	6854
Number of clusters	4276	10	766	4276	10	766
Panel 8: Including Parental Cohort Fixed Effects						
Parental years of schooling	0.229***		0.517**	0.019***		0.042
	[0.009]		[0.244]	[0.001]		[0.032]
F-statistic for the strength of the IV			10.82			10.82
Observations	11113		11113	11113		11113
Number of clusters	6299		1282	6299		1282

Notes: All regressions control for city fixed effects, children's birth cohort fixed effects, dummies for different surveys and dummies for different type of parent-child matching and dummies for parent's and child's gender. The instrumental variable in all IV estimations is parental detrended years of schooling by Cohort-City. Robust standard error in square brackets, which are clustered at family level for OLS and clustered at cohort-city level for IV estimations. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Figure 1: The effect of school interruption during the Cultural Revolution on educational attainment of the parental cohorts

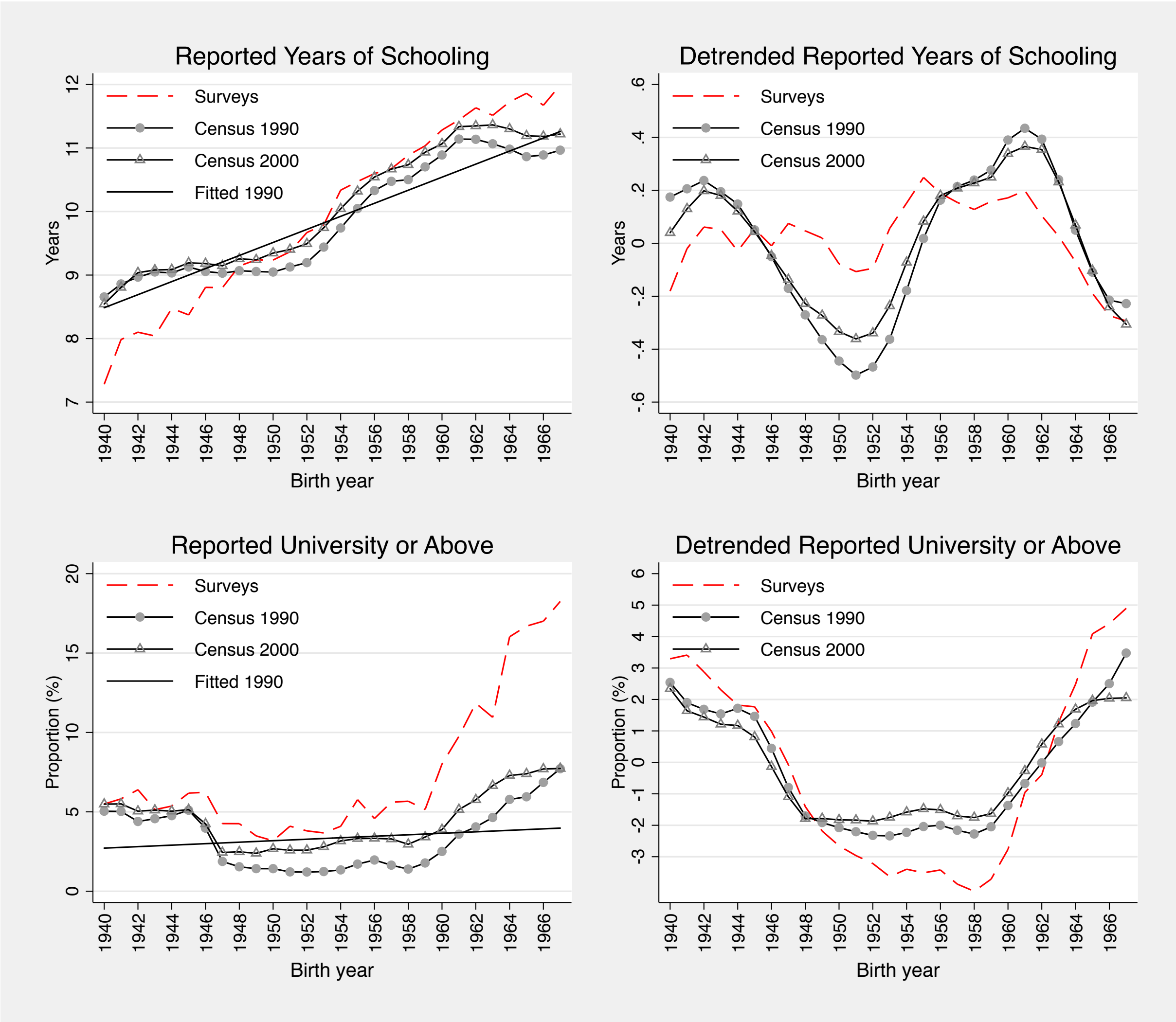


Figure 2: Detrended years of schooling in a few major cities of China

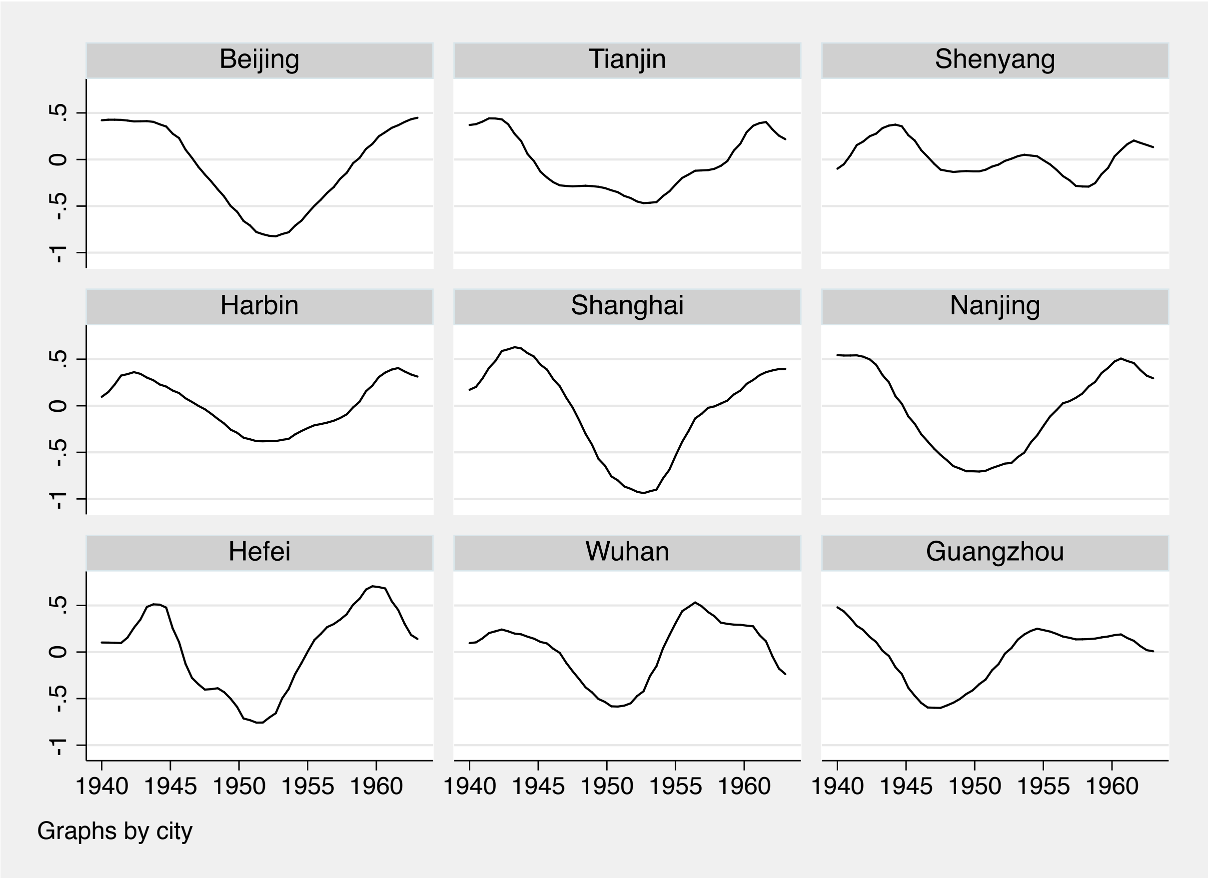
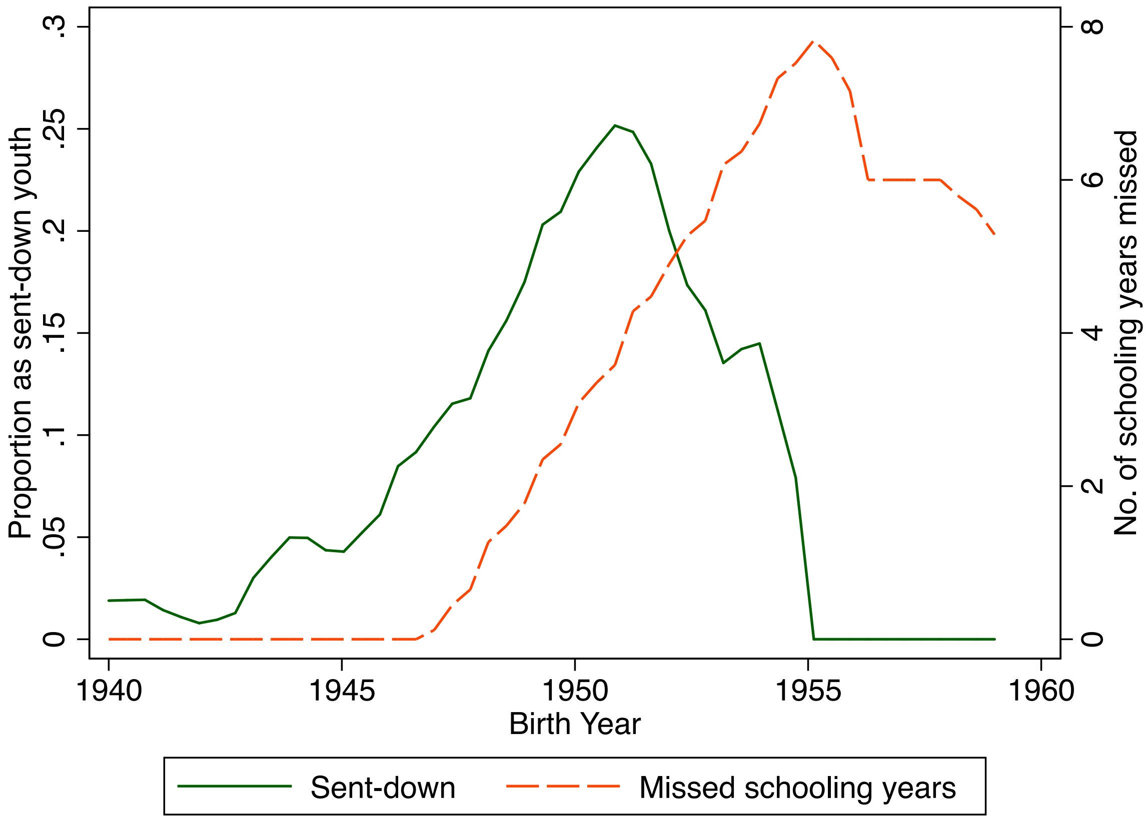


Figure 3: Missed Years of Schooling and sent-down Experience



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A Appendix tables

Table A.1: Designs and implementation of CULS, UREES and RUMiC

Surveys	Design and implementation	Geographic coverage	No. of Households
UREES (2005)	National Bureau of Statistics (NBS)	Urban areas of 12 provinces: Bejing, Shanxi, Liaoning, Zhejiang, Anhui, Hubei, Guangdong, Sichuan, Guizhou, Shaanxi, Gansu.	10,000
CULS (2001)	Institute of Labour and Population Studies at CASS and NBS	1 municipality and 4 provincial capital cities: Shanghai, Wuhan, Shenyang, Fuzhou, Xi'an.	3,499
CHIPS (2002, urban sample)	Institute of Economics at CASS and NBS	77 districts/counties in 12 provinces: Beijng, Shanxi, Liaoning, Jiangsu, Anhui, Henan, Hubei, Guangdong, Chongqing, Sichuan, Yunnan and Gansu.	6,835

Table A.2: Sample restrictions

Sample restrictions	CULS (2001)	CHIPS (2002)	UREES (2005)
(1) Number of of households	3499	6835	10000
(2) matched child-parent pairs in prefectural or above level cities and the parents posses urban Hukou	22623	20282	38857
(3) exclude the children who had not completed schooling[a]	17813	17998	26089
(4) exclude the pairs that either parents or childs education is missing	15078	17986	25853
(5) exclude the pairs with unreasonable or missing age[b]	14558	17986	22762
(6) exclude the pairs that the parent was born before 1940 or after 1963[c]	2573	3049	7258
(7) exclude the pairs that the child was born before 1965	2167	2611	6365
(8) exclude the observations in two cites which have only Pre-CR cohorts	2167	2581	6365

Notes: [a] The category 'children who had completed schooling' refers to those who had left school and were aged more than 24 years in the survey year.

[b] The 'unreasonable' age here refers to that less than 14 years younger than the parent, or parents less than 14 years older their child.

[c] After the above restrictions, only two parents were born after 1963 and no one was born afterward. So here we exclude these two two parents.

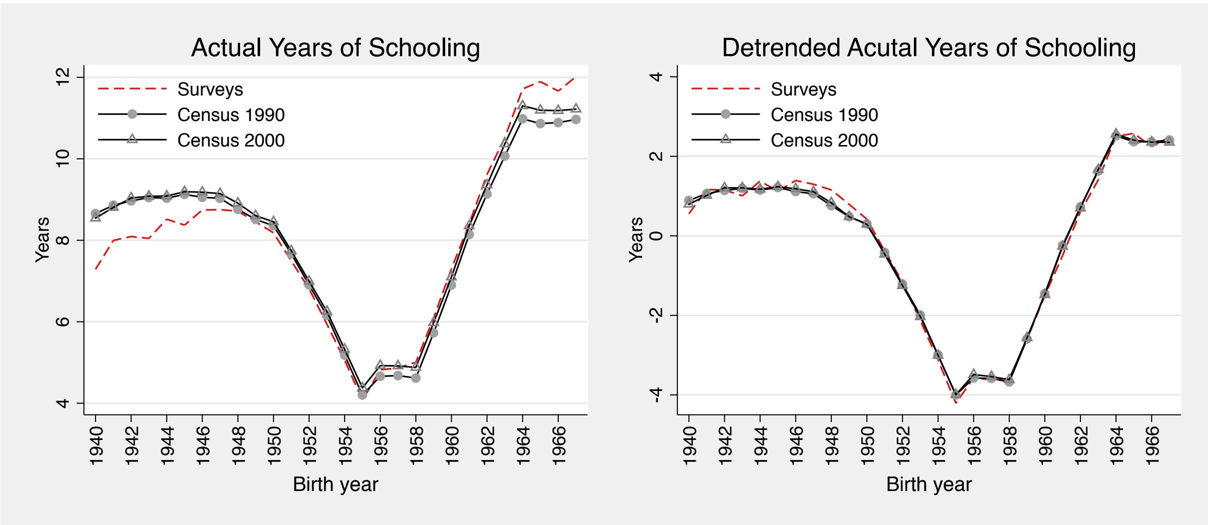
Table A.3: D-L two step regression results of the reduced form model

	Parent-child	Father-Child	Mother-Child
	(1)	(2)	(3)
Panel A:		Children's Years of Schooling	
Parental No. of years missed in school	-0.044***	-0.031*	-0.053**
	(0.013)	(0.016)	(0.020)
Observations in the first step	23	23	23
Observations in the second step	11113	4910	6203
Panel B:		Children's University Degree	
Parental No. of years missed in school	-0.006***	-0.006**	-0.006**
	(0.001)	(0.002)	(0.002)
Observations in the first step	23	23	23
Observations in the second step	11113	4910	6203

Notes: All first-step regressions control for city fixed effect, children's birth cohort fixed effect, dummies for different surveys and dummies for different type of parent-child matching. The Number of observations in the second step is exactly the same as the number of the clusters in the first step; the second-step regression is weighted with the cluster size. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

B Appendix Figure

Figure B.1: "Actual" years of schooling



C Understanding the correlation between children's unobserved abilities and parental education

The model introduced in the Methodology section describes the causal relation between parental education and children's education, and also suggests why parental education may be correlated with children's unobserved abilities. This appendix further analyses the direction of the correlation when an exogenous shock has a heterogenous effect over ability distribution. To focus on this objective, we drop the control variables, assume that there is only one measure of schooling interruption during the CR, and allow the impact of schooling interruption varies over individuals' ability distribution. Then we obtain the following model:

$$Edu^c = \alpha_0 + \alpha_2 Edu^p + \varepsilon^c, \varepsilon^c = u + v^c \tag{5}$$

$$Edu^p = \theta_0 + \theta_1(u) SchInt^p + \varepsilon^p, \varepsilon^p = u + v^p, \tag{6}$$

where the common unobserved inheritable ability u (with zero mean and variance σ_u^2) leads to the correlation between parental education and children's unobserved abilities; v^c and v^p, as idiosyncratic error terms, are orthogonal to Edu^p as well as u; $SchInt^p$, as an exogenous shock, is independent of individuals' characteristics including u, v^c and v^p.

By normalization, we can always let higher value of $SchInt^p$ imply more serious schooling interruption, and let $E[SchInt^p] > 0$. In addition, schooling interruption during the CR had stronger impact on more able individuals (Meng and Gregory, 2002). These imply $\theta_1(u) < 0$ and $\theta_1'(u) < 0$. Based on the above assumptions, the OLS estimate of α_2 is

$$\begin{aligned} \hat{\alpha}_2 &= \frac{cov(Edu^c, Edu^p)}{var(Edu^p)} \\ &= \alpha_2 + \frac{(u + v^c, \theta_1(u) SchInt^p + u + v^p)}{var(Edu^p)} \\ &\approx \alpha_2 + \frac{\sigma_u^2 + cov(u, \theta_1(u) SchInt^p))}{var(Edu^p)}. \end{aligned} \tag{7}$$

The approximation (7) comes from the assumption of uncorrelation between u and v^c, and when they are independent, the equality can strictly hold. The second term in the left hand side of approximation (7) is the bias in the OLS estimate. When the impact of schooling interruption is homogenous, $\theta_1(u)$ is a constant and therefore, the bias is just $\sigma_u^2/var(Edu^p)$, which is positive. However, when the impact varies over ability distribution,

$$\begin{aligned} cov(u, \theta_1(u) SchInt^p)) &= E[u\theta_1(u) SchInt^p], \quad \text{since } E[u] = 0 \\ &= E[u\theta_1(u)]E[SchInt^p], \quad \text{since } SchInt^p \text{ is independent of } u \end{aligned}$$

where $E[u\theta_1(u)] < 0$ given $\theta_1'(u) < 0$, and $E[SchInt^p] > 0$ as we assumed above. Therefore, $cov(u, \theta_1(u) SchInt^p)) < 0$. Consequently, (i) when $|cov(u, \theta_1(u) SchInt^p))| > \sigma_u^2$, the OLS estimate is an underestimate rather than an overestimate of the causal effect; (ii) when $|cov(u, \theta_1(u) SchInt^p))| = \sigma_u^2$, the OLS is just an unbiased estimate; and (iii) when $|cov(u, \theta_1(u) SchInt^p))| < \sigma_u^2$, the OLS estimate is still downward biased, but the bias is smaller than that with homogenous impact of schooling interruption.