HKUST IEMS
INSTITUTE FOR EMERGING MARKET STUDIES

The Great Proletarian Cultural Revolution, Disruptions to Education, and the Returns to Schooling in Urban China

John GILES, Albert PARK, Meiyan WANG

HKUST IEMS Working Paper No. 2015-21

March 2015

HKUST Institute for Emering Market Studies
The Hong Kong University of Science and Technology
Clear Water Bay, Kowloon, Hong Kong
T: +852 3469 2215 E: iems@ust.hk W: iems.ust.hk

Board Reforms and Firm Value: Worldwide Evidence

John GILES, Albert PARK, Meiyan WANG

HKUST IEMS Working Paper No. 2015-21
March 2015

Abstract

This paper provides new evidence on educational disruptions caused by the Cultural Revolution and identifies the returns to schooling in urban China by exploiting individual-level variation in the effects of city-wide disruptions to education. The return to college is estimated at 49.8% using a conventional Mincer-type specification and averages 37.1% using supply shocks as instruments and controlling for ability and school quality, suggesting that high-ability students select into higher education. Additional tests show that the results are unlikely to be driven by sample selection bias associated with migration or alternative pathways through which the Cultural Revolution influenced adult productivity.

Author's contact information

John Giles
The World Bank
MSN MC 3-311, 1818 H Street NW, Washington, DC 20433
T: (202) 473-3451
E: jgiles@worldbank.org

Albert Park
HKUST Institute for Emerging Market Studies, Division of Social Science, and Department of Economics
The Hong Kong University of Science and Technology
T : +852 2358 5981
E: albertpark@ust.hk

Meiyan Wang
Institute for Population and Labor Economics
Chinese Academy of Social Sciences

The Great Proletarian Cultural Revolution, Disruptions to Education, and the Returns to Schooling in Urban China[*]

John Giles[**]
The World Bank and IZA[†]

Albert Park
HKUST, CEPR, and IZA

Meiyan Wang
Institute for Population and Labor Economics
Chinese Academy of Social Sciences

This Version: March 16, 2015

Abstract

This paper provides new evidence on educational disruptions caused by the Cultural Revolution and identifies the returns to schooling in urban China by exploiting individual-level variation in the effects of city-wide disruptions to education. The return to college is estimated at 49.8% using a conventional Mincer-type specification and averages 37.1% using supply shocks as instruments and controlling for ability and school quality, suggesting that high-ability students select into higher education. Additional tests show that the results are unlikely to be driven by sample selection bias associated with migration or alternative pathways through which the Cultural Revolution influenced adult productivity.

Keywords: Returns to Schooling, Wages, Education, China
JEL Codes: I20, J24, J30, O15, O53

[*]We are grateful for useful conversations with Xin Meng and Fang Cai, and for helpful comments on earlier drafts from Belton Fleisher, Steven Haider, John Strauss, Jan Svejnar, Jeffrey Wooldridge, Dean Yang and Dennis Yang. The authors acknowledge grants to support field research from Michigan State University (Intramural Research Grants Program), the Chinese Academy of Social Sciences, The Ford Foundation (Beijing), the University of Michigan (Rackham Faculty Research Grant), the International Centre for the Study of East Asian Development (ICSEAD), the Australian Research Council and the World Bank (Beijing), and support for follow-up research from the Knowledge for Change Program at the World Bank.
[**]*Corresponding Author:* The World Bank MSN MC 3-311, 1818 H Street NW, Washington, DC 20433; phone: (202) 473-3451; e-mail: jgiles@worldbank.org.
[†]The research, discussions and conclusions presented in this paper reflect the views of the authors and should not be attributed to the World Bank or to any affiliated organization or member country.

The Great Proletarian Cultural Revolution, Disruptions to Education, and Returns to Schooling in Urban China

1. Introduction

One consequence of the Great Proletarian Cultural Revolution, a radical political campaign initiated by Chairman Mao Zedong in 1966, was the widespread disruption to China's educational system from 1966 to 1976. This disruption had a negative impact on the human capital accumulation of affected cohorts, with long-term consequences for labor productivity, inequality, and intergenerational mobility. The extent of the disruptions differed across cohorts, across time, and across cities, depending on how zealously new policies were interpreted and implemented locally.[2] As a result, for no other reason than the unlucky location and timing of their birth, the schooling of many Chinese citizens was delayed or cut short.

In this paper, we first provide new empirical evidence on the extent and nature of educational disruptions caused by the Cultural Revolution, taking advantage of detailed information on educational histories, parental characteristics, whether respondents were sent to the countryside during the Cultural Revolution, and contemporary hourly wages, and physical and psychosocial health enumerated in the 2001 China Urban Labor Survey. The rich data enable a more systematic assessment than in previous studies. Meng and Gregory (2002b) estimate that the Cultural Revolution reduced college completion rates of affected cohorts from 10.6 to 4.5 percent, but their data lack detailed enrolment histories and contains only limited information on parental characteristics. Deng and Treiman (1997) point out that the Cultural Revolution sharply reduced the intergenerational correlation in educational attainment, but can only examine patterns for sons and fathers who reside in the same households.

[2]See Pepper (1996) for case studies that describe differences across localities.

We estimate that the Cultural Revolution reduced high school and college completion rates by age 25 by 7.1 and 6.3 percentage points from rates predicted by pre- and post-Cultural Revolution trends (56.2 and 10 percent, respectively). We also find that the Cultural Revolution was a great equalizer of educational access. Prior to and after the Cultural Revolution, children's educational attainment in urban China was highly correlated with the educational attainment of their parents--a nearly universal finding throughout the world. However, among city-cohorts affected during the Cultural Revolution years, children's educational attainment became much *less* correlated with that of their parents and *more* correlated with whether parents held administrative positions.

After documenting these impacts of the Cultural Revolution on educational attainment, we then exploit changes in the correlation between parental characteristics and educational attainment to develop an instrumental variables approach to more accurately estimate the returns to schooling in urban China. In post-reform China, rising private returns to university education, especially during the 1990s, contributed to keen competition for entrance to university, and motivated an unprecedented expansion of higher education during the 2000s as well as earlier reforms that shifted the burden of financing education from the government budget to household tuition payments (Du and Giles, 2006). These policy changes can be justified if the measured private returns to education accurately reflect productivity gains from completing tertiary education. However, part of the measured returns could be picking up differences in the unobserved ability of college versus high school graduates. The inability of large numbers of college graduates to find work from the mid-2000s onward raises questions as to whether families received accurate signals of the true returns to a college education in China.

An attractive feature of our IV approach is that the combination of exogenous city-cohort specific disruptions to education and variation in parental education produces individual-level variation in schooling shocks, which enables us to control explicitly for unobserved characteristics of each birth cohort in each city through the inclusion of city-cohort fixed effects. We thus avoid a potential weakness of studies that identify the variation in years of schooling solely by cohort differences, such as Ichino and Winter-Ebmer's (2004) study of World War II educational disruptions affecting the 1930-35 birth cohorts in Austria and Germany and Meng and Gregory's (2002a) study of cohorts whose education were affected by the Cultural Revolution. In these studies, the authors are unable to control for unobserved cohort differences that are correlated with both schooling shocks and later productivity, such as those associated with differences in school quality, demographic or policy changes, or major political, social, or economic events that may affect the productivity of entire cohorts.

The analysis of the returns to education contributes to a literature summarized by Card (2001) in which measurement of the causal effect of education on labor earnings is facilitated using eligibility rules and supply-side factors as exogenous determinants of schooling outcomes. Instruments used in previous studies of the returns to education include quarter of birth (Angrist and Krueger, 1991; Staiger and Stock, 1997), geographic proximity to schools (Kane and Rouse, 1993; Card, 1995), changes in school systems or school leaving age (Harmon and Walker, 1995), special education subsidies for veterans (Lemieux and Card, 1998), and a national school expansion program (Duflo, 2002). In contrast to Duflo's paper which focuses on expansion of primary schools, this paper focuses on shocks that primarily influenced high school and college attainment. Using instrumental variables to identify the true returns to higher levels of educational attainment in developing countries is of particular interest because ability bias in such settings is likely to be substantial given the highly selective procedures often used to allocate scarce entrance slots in institutions of higher education to large pools of potential students. Developing country governments facing severe resource constraints must make difficult choices between investing in higher education or primary and secondary education. Accurate information on the true private returns to different levels of schooling can better inform such choices.

Studies that have estimated the returns to schooling in urban China using Mincer-type specifications generally have found that the returns to schooling remained very low by international standards well into the reform period (e.g., Byron and Manaloto, 1990), which began in 1978, but that starting in the mid-1990s there was a steady and dramatic increase in the returns to schooling, reaching very high levels even in comparison to other countries. The best set of consistent estimates over time uses repeated cross-sectional urban survey data collected by the National Bureau of Statistics (NBS) from 6 provinces in different regions from 1988 to 2001 and finds that over this period the increase in annual wages in urban China associated with an additional year of schooling grew from 4 percent to over 10 percent (Zhang et al., 2005). In 2001, the return to high school education compared to middle school was 21 percent, and the return to college education compared to high school was 37 percent (Zhang et al., 2005).[3] A more recent study finds that the returns to a year of education remained relatively stable at just less than 10 percent from 2001 to 2009, while the returns to college continued to grow slowly after 2001, reaching 49 percent by 2009 (Li et al., 2012). All of these studies use annual wage income; the returns to education have been found to be noticeably

[3] Similarly, using NBS urban survey data on annual wages in 6 provinces in 2000, Heckman and Li (2004) find an OLS return to college of 34.4% in their basic specification. They find much higher returns using parental characteristics as an IV for college attendance.

higher using hourly wages in both urban and rural China (Li, 2003; de Brauw and Rozelle, 2008).[4]

These simple estimates of the returns to schooling in urban China are subject to several sources of potential bias, including measurement error in self-reported educational attainment and unobserved ability or school quality. A recent paper by Li, Liu, and Zhang (2012) attempts to deal with these problems by comparing earnings differences between identical twins with different levels of educational attainment to reduce bias caused by unobserved ability. Analyzing data from five Chinese cities in 2002, the estimated returns to a year of schooling in terms of log monthly wages are only 2.7 percent (and 3.8% after using sibling reports as an instrument to correct for measurement error) compared to an OLS estimate of 8.4 percent. Although twins studies are not without potential problems[5], they reveal a much greater degree of ability bias in China than has been found in twins studies in the U.S.

Previewing our main findings, taking the means of our preferred IV estimates, we find that the returns to a year of schooling for hourly wages in urban China in 2001 are 8.0 percent compared to 9.6 percent using OLS with a standard set of covariates. The rich detail on school types and locations and parent occupation allows for OLS estimates with additional controls for ability, and these yield a return to schooling of 8.3 percent. The returns to college education versus high school range from 36.2 to 37.9 percent compared to 49.8 and 42 percent using OLS with standard and extended covariates, respectively. Thus, consistent with ability bias, IV estimates of the overall returns and the returns to college are lower than the OLS estimates, and significantly below the returns using a conventional set of covariates. Further, we find that

[4]The downward bias in returns using annual income is driven by the fact that, on average, workers with less education tend to work more hours each week, month and year than workers with higher levels of educational attainment.

[5] As has been pointed out by others, schooling differences between twins are unlikely to be random, so that twins-based estimates will still be subject to endogeneity bias (Bound and Solon, 1999; Neumark, 1999).

the IV estimates of the return to high school are higher than OLS estimates, which is consistent with selection into college creating a negative ability bias for those who complete only high school. We show that it is unlikely that our results are driven by sample selection bias associated with migration, or by alternative pathways through which the Cultural Revolution could have affected adult productivity.

The rest of the paper is organized as follows. In section 2, we describe the data and key variables used in the estimation. In section 3, we provide evidence on the disruptive effects of the Cultural Revolution on educational attainment. Section 4 presents the estimating equations for the determinants of log wages and discusses our identification strategy. In section 5, we present and discuss our estimation results, and section 6 concludes.

2. Data and measurement

The China Urban Labor Survey (CULS) was conducted at year-end 2001 by the Institute for Population and Labor Economics at the Chinese Academy of Social Sciences (CASS-IPLE), working with provincial and municipal government offices of the National Bureau of Statistics. The authors collaborated in the design and execution of the survey. The CULS was conducted in five cities: Fuzhou, Shanghai, Shenyang, Wuhan, and Xian. The cities were chosen to be broadly representative of China's different regions. Fuzhou and Shanghai are coastal cities, Shenyang is in the northeast, Wuhan is in central China, and Xian is in western China. Three of the cities are among China's six largest cities by population, and another ranks tenth. In each of the five cities, a representative sample of 700 households whose members were urban permanent residents (holding urban resident permits of the surveyed city) were surveyed.[6] Each household head was asked questions about the household, and then all household members above age 16 and no longer in school were interviewed individually. This resulted in 8109 individual observations. Of these 8109 individuals, 5787 were younger than mandatory retirement age, and 4076 were employed or reported labor earnings at the time of the survey. We exclude self-employed workers because their reported earnings reflect returns to capital as well as labor. We also exclude individuals born after 1978, because many could have still been in school at the time of the survey in 2001. This leaves us with 3614 observations of urban resident adults between the ages of 23 and 60 who were employed in November 2001.

One advantage of the CULS survey is that unlike the urban household surveys conducted by the National Bureau of Statistics, information was collected on hours worked per day and days worked per month, making it possible to examine hourly wages rather than monthly or annual incomes that do not account for hours worked. As noted earlier, using hourly rather than monthly or annual income is expected to increase the estimated returns to education.

By focusing on urban permanent residents, the survey excludes those with temporary residence permits or with no registration status, a group consisting primarily of rural migrants. China maintains a household registration (*hukou*) system, which discriminates against non-local residents in access to many public services and benefits as well as employment opportunities. Migrants tend to be much younger than urban residents, are likely to have attended schools of

[6]Within each city, a three-stage proportional population sampling approach was used to sample an average of 15 registered urban households in each of 70 neighborhood clusters (for details, see Giles, Park, and Cai, 2006). The survey had a non-response rate of 16.5 percent, of which 6.5 percent of households could not be found, 4.9 percent had moved, and 5.1 percent refused to be interviewed. This refusal rate compares favorably with the first round refusal rates of two influential surveys from developing countries: the Indonesia Family Life Survey (IFLS) and the China Health and Retirement Longitudinal Study (CHARLS).

poorer quality in rural areas, are more likely to be self-employed, and are a self-selected group who earn lower wages than urban residents even after controlling for observable characteristics (Meng and Zhang, 2001), either due to discrimination or other unobserved differences (e.g., school quality, ability, etc.). For all of these reasons, we focus only on the returns to schooling of urban permanent residents even though the CULS included a separate survey of migrants in each city. According to the 2000 population census, registered urban households comprised 76 percent of those living in the five sample cities.

The CULS survey instrument asks detailed questions about workers' educational histories. It records the year in which each level of schooling began (primary, lower secondary, upper secondary, vocational high school, college, vocational college, three-year college, graduate school) and the years of schooling completed at each level, including information on repeated grades. From this detailed information, we calculate multiple education disruption variables discussed in section 3 below.

The questionnaire also included questions about the schools attended at each level, which reflect both school quality and early assessments of individual ability that would be otherwise unobserved. These questions include location (city, county, town, or village); the school's province; and whether the student was in a magnet school, an accelerated class in a regular public school, a regular public school class, or a private school. Detailed questions also were asked about the parents of the respondent, including educational attainment whether present and alive or not, primary industry of employment, occupation, and technical and administrative status.

3. Education Disruptions and the Great Proletarian Cultural Revolution.

In 1966, Mao Zedong initiated a radical political campaign that incited millions of Chinese citizens to revolutionary struggle against corrupt cadres deemed to have betrayed China's Communist revolution. During this tumultuous period, the system of formal education in China's cities was thrown into chaos, while rural schooling was less affected (Meng and Gregory, 2002b). Urban elementary and secondary education was disrupted for at least six years, and for much of the period from 1966 to 1968 schools in many urban areas were closed altogether. Most 4-year universities and 3-year colleges (which often provide vocational training) were closed for six years. Upon re-opening, family political class background status served as an important eligibility criteria for college admission. Educational disruptions were worse for children with parents who had a bad political class background, and even those with a "middle" background could have been constrained by quotas favoring children of "poor farmers", "workers" and those with "revolutionary" backgrounds, especially at higher levels of education (Pepper, 1996). Universities did not return to merit-based enrollment of students until 1977.

The shock to post-secondary education is evident in Figure 1, which plots administrative data on enrollments in institutions of higher education by year. The figure shows that there were no new entrants to institutions of higher education, including 4-year universities and 3-year colleges, from 1967 through 1970, and a sharp drop in total enrollments during the Cultural Revolution. These disruptions are not nearly as evident when we plot the share of individuals with higher education degrees by birth cohort using data from the 2000 census (Figure 2). This may be due to the preponderance of correspondence schools and part-time degree programs in the 1980s and 1990s, which enabled individuals to obtain higher education degrees later in life, and the relatively low overall college enrollment rates as a share of the total population. Also, entering classes contain students from multiple birth cohorts, so that cohort differences in educational attainment will be less obvious than time differences in enrollment. In contrast, the census data plotted in Figure 2 does show clearly the shock to high school completion for Cultural Revolution cohorts.

Reinforcing disruptions created by closure of high schools and universities early in the Cultural Revolution was the "sent-down youth" program (also known as the "rusticated youth" program), under which youth from urban areas were sent to the countryside to live in rural communities. The stated ideological motivation for the program was to help young people get in touch with the revolutionary origins of the Party and to contribute to the country's rural development. However, the program also has been viewed as a pragmatic response to social unrest and unemployment in urban areas created by a vast youth population that was neither employed nor in school and stirred to revolutionary furor at the start of the Cultural Revolution (Meisner, 1986). Starting in 1968 large numbers of urban youth of high school and early college ages were dispatched to rural areas, guided by education officials and local revolutionary committees established during the Cultural Revolution. Families were typically allowed to choose one child to keep in the city, but all other children of appropriate age were sent to the countryside. As the Cultural Revolution progressed, students were sent to the country at progressively later ages and some students with favorable class backgrounds were allowed to defer service in the countryside. While rusticated youth were initially expected to spend two years in the countryside, many spent much longer periods of time there because of the lack of available urban jobs. Return from the countryside could be facilitated by college admission, which until reforms in 1978 required a favorable class background, by joining the military, or by using the connections of a parent, relative, or acquaintance with high enough administrative status to arrange reassignment to an urban job. Most but not all sent down youth were able to return to urban areas after the onset of economic reforms in 1978.

Being sent down may have disrupted education through multiple channels. In addition to the direct impacts of not attending school for two or more years, the delay in schooling increased the opportunity cost of continuing in school because children were older, prior learning depreciated with time out of school, and returning students faced increased competition for placement in the educational system because of accumulated cohorts of students competing for a limited number of slots. Li, Rosenzweig, and Zhang (2010) find a negative relationship between years sent down and years of education. In addition, the sent down youth program targeted those with bad class backgrounds (i.e., children of intellectuals) and was thus an equalizing shock to children of parents with different levels of education, which is distinct from the shock to the average level of educational attainment.[7]

In Table 1, we present descriptive statistics for the sample used in our analyses. We divide individuals into three cohort groups—those whose education was likely to be completed before the Cultural Revolution began (pre-Cultural Revolution cohorts), those whose education was potentially affected by the Cultural Revolution (Cultural Revolution cohorts, born from 1948 to 1963), and those who did not reach school age until after the Cultural Revolution ended (post-Cultural Revolution cohorts).[8] As seen in Table 1, the average share of individuals completing high school and college by age 25 were lower for Cultural Revolution cohorts (55 and 3.6 percent) than for pre-Cultural Revolution cohorts (59 and 6.2

[7] The size of each city's sent-down youth population also could have been a consequence of school closures in urban areas, in which case it still acts as an effective proxy for school disruption. Other factors influencing the quantity of the sent down youth in a specific city-cohort (such as contemporaneous city unemployment rates) will not bias our estimates unless they independently affect the same city-cohorts future labor productivity in a correlated fashion and do so differently for children whose fathers have more or less education.
[8] These cutoff years correspond to those whose high school education was likely to have been affected by the Cultural Revolution (see below).

percent) and post-Cultural Revolution cohorts (74 and 18.9 percent). Further, the modest increase in average years of education was less than that predicted by pre- and post-crisis trends. Finally, the decline in educational attainment of the Cultural Revolution cohort occurred even as the educational attainment of fathers and mothers increased in comparison to the pre-Cultural Revolution cohorts (Table 1).

We construct three measures of educational disruptions caused by the Cultural Revolution which are specific to each birth cohort in each city. First, separately for each city, we measure cohort shocks to high school and college education by calculating the deviation of the actual share of each Cultural Revolution cohort that completed each level of schooling from the cohort trend in these shares calculated from the educational attainment levels of pre- and post-Cultural Revolution cohorts. We calculate shocks to the completion of high school and to the completion of three- or four-year post-secondary degrees by age 25 conditional on graduating from middle school. We restrict the age of college completion to avoid the influence of degrees acquired later in life on our measure of disruptions during the Cultural Revolution period.[9]

In defining which cohorts experienced disruption to high school and college, we follow the definitions of Meng and Gregory (2002b). For high school, cohorts born prior to 1950 would have completed their high school degrees when the Cultural Revolution started. Those born in 1956 or later would have had access to all years of primary and secondary school. For each individual, the shock to high school faced by individual i in city j and birth cohort t, $hshock_{ijt}$, is calculated as:

$$hshock_{ijt} = htrend_{jt} - hmean_{-ijt} \qquad (1)$$

[9]When calculating educational shocks we make use of all available observations, and thus include information on individuals of working age who were not employed during the previous year.

where $htrend_{jt}$ is the trend share of middle school graduates from birth cohort t in city j completing high school and $hmean_{\text{-}ijt}$ is the actual share of middle school graduates who completed high school.[10] When calculating the value of $hmean_{\text{-}ijt}$ for each individual i, we exclude i's own report out of concern that mis-measurement of i's shock could be correlated with unobservables. In addition, out of concern that city-cohort sizes may be small for some years and introduce noise or spurious correlations, $hmean_{\text{-}ijt}$ is calculated using individuals who completed middle school from the $t\text{-}1$, t and $t+1$ birth cohorts.[11] We calculate shocks to post-secondary completion ($pshock_{ijt}$) in a similar fashion, except that the birth cohorts potentially affected by the Cultural Revolution are from 1948 to 1963 instead of 1950 to 1955 as for high school.[12]

A simple way to illustrate the extent of educational disruption is to compare the mean attainment of the Cultural Revolution cohort with the predicted attainment levels based on the fitted trend line linking pre- and post-Cultural Revolution cohorts (htrend). According to this calculation, if the Cultural Revolution had not occurred, the percentage of adults completing high school and college by age 25 would have been 56.2 and 10.0 percent, or 7.1 and 6.3 percentage points higher than actual attainment rates. Particularly striking is that the share completing college would have been twice as great in the absence of the Cultural Revolution.

---

[10]Specifically, the trend line is calculated as:

$$htrend_{jt} = hmean_{j47} + [((hmean_{j64} + hmean_{j65} + hmean_{j66})/3 - (hmean_{j45} + hmean_{j46} + hmean_{j47})/3)/17] \times (byear - 47)$$

where $hmean_{jt}$ is the share of the birth cohort born in year t and city j who completed high school.

[11]We find that the results of our analysis are robust to using education shocks for year t cohorts as an alternative to the three cohort average.

[12]Regular college enrollment was affected for a longer period because competitive examinations were not reinstituted for college admission until 1977 for the class entering in 1978, and those individuals leaving school after 1966 were allowed to sit for college entrance examinations through 1981.

Our third education disruption variable measures the scope of the sent down youth program for each cohort in each city. For individual *i* in city *j* from birth cohort *t*, $sdshock_{-ijt}$ is calculated as the share of individuals other than *i* participating in the program over the *t-1*, *t*, and *t+1* birth cohorts. Out of concern that urban residents might be sent to the countryside for a number of reasons other than the sent down youth program, the CULS carefully enumerated seven reasons adolescents and young adults may have been sent to the country.[13]

We plot average values of these three disruption variables for different birth cohorts in Figure 3. Positive values reflect greater disruption (lower educational attainment relative to trend or greater share of individuals affected by the sent down youth program). The negative disruption for high school completion for cohorts born after 1956 reflects the expansion of high school education that occurred during the latter half of the Cultural Revolution.[14] Perhaps most striking is the significance of the sent down youth program. For each cohort born from 1948 through 1958, 30 percent or more were sent to the countryside. The peak birth cohort was 1951, of which 50 percent were sent down youth.

As noted earlier, our identification strategy focuses on how educational disruptions altered the relationship between parental characteristics and educational attainment to create individual, within city-cohort variation, in the effects of the Cultural Revolution on educational attainment. Relative to pre-Cultural Revolution cohorts, we expect that parental education mattered less in determining children's educational attainment during the Cultural Revolution years. The children of elites (those with *bad* class backgrounds) likely had a harder time gaining access to education than the children of non-elites, but our identification approach works as

[13]Apart from the "rusticated youth" (or sent down youth) program, other possibilities included *reform through labor*, *sent with parents engaged in reform through labor*, *family persuaded to return to rural area*, *repatriated with family to rural area*, *hukou is in rural area*, and *other*.
[14]Pepper (1996) notes that after 1972 there were efforts to expand high school graduation rates which were later scaled back.

long as relative access to education of the children of educated parents was reduced during the Cultural Revolution period relative to other periods. This would have happened, for instance, if the Cultural Revolution made educational attainment more universally accessible, even if it did not discriminate against children of elites, or if overall access to higher education contracted and this had a larger effect on the educational attainment of children of elites.

We also hypothesize that parental administrative status became more important for educational attainment during the Cultural Revolution. We follow evidence suggesting that during the Cultural Revolution parents in high administrative positions could influence whether and where their children were sent to the countryside, reduce their time spent in the countryside as rural sent-down youth, or increase their chances of getting into college. Zhou and Hou (1999) find that children whose parents were cadres returned to cities earlier after being sent-down youths than children whose parents were not cadres. Students sent to better rural locations or who returned earlier to urban areas would have a better environment for self-study and thus increase their chances of passing the college entrance exam with the return of merit-based exams. In some cases, connected parents might even have been able to directly influence college admissions decisions during the period from 1972 to 1977 when such decisions were based on political, rather than academic, considerations. We define a parent to be a high administrator when his highest achieved administrative status was at the level of division manager (*chu*), bureau director (*ju*) or above.

To illustrate the changes in the intergenerational correlation of educational attainment associated with the Cultural Revolution for each city, we plot partial correlation coefficients for fathers' and children's years of schooling by birth cohort in Figure 4.[15] In each city, the correlation falls during the Cultural Revolution years and recovers thereafter, with the exact

[15] The partial correlations control for differences in fathers' administrative status

timing and magnitude of these changes varying across cities. The Cultural Revolution thus appears to have succeeded in temporarily weakening the strong correlation between the educational attainment of fathers and their children that is found in most societies. This finding echoes that of Deng and Treiman (1997), who analyze census data to examine trends in the intergenerational correlation in educational attainment for households in which parents co-reside with their adult children.

To examine these relationships more rigorously, we estimate a binary response probit model, $G(\cdot)$, where the determinants of the probability of individual *i* from city *j* and cohort *t* to complete a certain level of education, ED_{ijt}, is modeled as follows:

$$P(ED_{ijt}=1)=G(\gamma_1(fed_{ijt}\cdot eshock_{jt})+\gamma_2(fad_{ijt}\cdot eshock_{jt})+\mathbf{X}_{ijt}\boldsymbol{\beta}+\mathbf{cohort}_{jt}) \qquad (2)$$

Here $eshock_{jt}$ is a city-cohort specific shock to education (alternatively $hshock_{jt}$, $pshock_{jt}$ or $sdshock_{jt}$) that is interacted with father's years of education, fed_{ijt}, and also separately interacted with whether one's father held high administrative status, fad_{ijt}.[16] The interacted variables fed_{ijt} and fad_{ijt} are included among a vector of individual, family, and school characteristics, $\mathbf{X}_{ijt}$, to control for their direct relationship to child ability or home environment. Also included as controls are age and age-squared measured to the month as well as the school and ability variables described earlier. Finally, we include a vector of dummy variables for each city-cohort, $\mathbf{cohort}_{jt}$. These dummies as well as other city-cohort level variables in our estimated probit models are defined for three-year city-cohorts.[17] Given this rich set of control variables,

[16] We do not include interactions with mother's education or mother's high administrative status because most mothers have low levels of education (4.7 years on average compared to 7.0 years for men) and very few are even administrators (2.7%) let alone administrators with high rank (in contrast 6.1% of fathers have high administrative status). So there is limited variation, and mother's characteristics are also strongly correlated with father's characteristics, making it impossible identify interaction terms with both parents' characteristics.
[17]Specifically, cohorts are in the following three-year groupings at the city: 40-42, 43-45, 46-48, 49-51, 52-54, 55-57, 58-60, 61-63, 64-66, 67-69, 70-72, 73-75, and 76-78. Using one-year cohorts leads to a model that is exactly determined for some city-birth year cohorts. Nonetheless, estimated marginal effects do not differ qualitatively whether we use one-year or three-year cohorts.

the coefficients on the two interaction terms cleanly identify variation in the effects of education shocks on educational attainment associated with fathers' education and administrative status.

In Table 2, we present estimation results for the determinants of college and high school completion using equation (2) and different combinations of education shock variables. In the first four columns, we examine the effect of cohort shocks to education on the probability of completing college. We first look separately at the effects on college completion of shocks to college completion, shocks to high school completion, and shocks due to the sent down youth program. For each model, the coefficient on the interaction of the education shock variable with father's years of schooling carries a negative sign and the interaction with the dummy for whether the father has a high administrative position carries a positive sign. Moreover, in each of the models at least one of the two coefficients on the interaction terms is statistically significant (see results in columns 1, 2, and 3). The results suggest that a decline in the share of a cohort attending college or high school has a more pronounced negative effect on the probability of attending college for children with well-educated fathers, or alternatively, that fathers' education has less of a positive effect on children's educational attainment for cohorts subject to greater educational disruptions. We also find that if the father is a high administrator, children are relatively more likely to attend college if the education shock is greater in magnitude. In column (4), we present results for the case in which all three education shock variables are interacted with father's years of schooling and father's administrative status. In all models of college completion, the interaction terms are jointly statistically significant. When all three sets of interactions are included, they are statistically significant at the 99 percent confidence level.

In columns (5) through (7), we present the results for the determinants of high school completion using shocks to high school and shocks caused by the sent down youth program. Children with more educated fathers are relatively less likely to complete high school when the cohort shock to high school is greater, but there is a statistically and economically insignificant effect of father's administrative status (column 5). Similarly, a higher share of one's cohort participating in the sent down youth program is associated with lower probability of completing high school when father's have higher education (column 6). When both sets of interactions are included (column 7) the coefficients on the interactions are not independently significant, but they remain jointly significant at the 90 percent confidence level.

4. Estimating the Returns to Schooling

Starting with a standard Mincer-type wage regression, we model the log wage of individual *i* in birth cohort *t* in city *j* (w_{itj}) to be a linear function of schooling ($\mathbf{S}_{itj}$), measured either in years or as levels of attainment (college, high school and middle school). To control for city and cohort effects created by unobserved differences in school quality and local policies, we first include vectors of city and cohort dummies separately, and then interactions of these dummies to allow cohort effects to vary across cities. We use ordinary least squares (OLS) to estimate the following equation:

$$w_{ijt} = \beta_0 + \beta_1 \mathbf{S}_{ijt} + \mathbf{X}_{ijt} \boldsymbol{\beta}_2 + \sum_{j=2}^{J} \gamma_j + \sum_{t=2}^{T} \lambda_t + u_{ijt} + \varepsilon_{ijt} \tag{3}$$

where γ_j and λ_t are city and birth cohort effects, and $\mathbf{X}_{ijt}$ includes gender (dummy for female) age and age-squared (measured to the month). To highlight the likely endogeneity bias, we include an omitted variable, u_{ijt}, which is correlated with both $\mathbf{S}_{ijt}$ and w_{ijt} (e.g., unobserved ability).

After first estimating a simple Mincer equation, we gradually build up to our preferred specification. We first add city-cohort dummies, which can be expressed as follows:

$$w_{ijt} = \beta_0 + \beta_1 \mathbf{S}_{ijt} + \mathbf{X}_{ijt}\boldsymbol{\beta}_2 + \sum_{j=2}^{J}\sum_{t=2}^{T}\lambda_{jt} + u_{ijt} + \varepsilon_{ijt} \ . \tag{4}$$

Including city-cohort fixed effects strengthens identification by controlling for many possibly confounding factors such as city-specific shocks to school quality or other unobserved policies that affect human capital or labor market outcomes of different cohorts in different cities. We next add to the set of control variables, $\mathbf{X}_{ijt}$, so that they include height, the z-score within gender, gender*height, and parent and family background variables (father and mother's years of education, parent occupation dummy variables, whether father has a high administrative rank, number of siblings).[18] Finally, we add school quality and ability variables. We expect to observe a fall in the estimated coefficient on years of schooling as we introduce more variables that proxy for ability, family support, or school quality.[19]

China's selective educational system restricts access to higher levels of schooling to high ability students, so that u_{ijt} is likely to lead to upward bias in the estimated returns to schooling. Identifying the causal relationship between schooling and earnings requires a valid instrument that is correlated with $\mathbf{S}_{ijt}$ but uncorrelated with u_{ijt}. To implement the strategy described above, we first generate predicted probabilities for high school and college enrollment from the probit models (results in Table 2) and then use these predicted

[18]Controlling for height, which is most heavily influenced by early childhood nutrition which predates educational disruptions, may be important because early childhood nutrition strongly predicts later educational attainment (Almond, 2006). Occupation categories include farmer, worker, technical worker, administrator or manager, self-employed, owner of private business.
[19]We have pooled men and women in this model. We have also estimated returns to educations separately for men and women, but find little difference in the patterns of returns and how they change using our IV strategy. We therefore pool the samples to take advantage of the larger sample size.

probabilities as instruments.[20] Note that the models shown in Table 2 include the same rich set of covariates proxying for dimensions of unobserved ability that we include in the second stage of our IV models, and that identification comes from the interactions of cohort-specific education shocks with father's education and administrative status.

We first present three panels of results for OLS models (Table 3). In column (1) we estimate equation (3), regressing log of hourly wage on schooling as well as city and birth cohort dummy variables. Using this basic Mincer-type specification, we find that a year of schooling increases wages by 9.6 percent, and that the return to college education compared to high school is 49.8 percent.[21] In column (2) we estimate equation (4), including interactions of three-year birth cohort dummy variables and city dummy variables. This turns out not to have a major effect on the coefficient on schooling in any of the models estimated. In column (3), we add the additional covariates other than school quality. The estimated return to college falls from 49.8 to 47.1 percent, and the return to a year of education falls from 9.6 to 8.9 percent. In column (4) we add school quality and parent occupation and administrative status variables. The return to college education falls from 47.1 to 42.0 percent, while there is little appreciable change in the return to high school over middle school. The return to a year of schooling falls from 8.9 to 8.3 percent (Panel B). Finally, using piecewise linear spline models, we find that the return to a year of post-secondary schooling is 13.5 percent using the basic Mincer-type specification, and falls to 12.9 then 11.9 percent as we add additional covariates as well as birth cohort-city interactions. The returns to schooling before college also fall from 6.3 percent per year of schooling in the basic specification to 5.3 percent in the full OLS

[20]See Wooldridge (2002) pages 139-141 for a general discussion of the use of generated instruments and Chapter 18 for use of predicted probabilities from a *probit* model as instruments.

[21] This is much higher than the estimated return to college in 2001 of 37 percent reported in Zhang et al. (2004), but the difference is less pronounced if we use annual wage income as they do. Using annual wages, our estimated return to schooling is 43.4 percent.

specification. (Panel C). The greater decline in returns to post-secondary years of schooling suggests that upward ability bias may be more pronounced at higher levels of education.

We present the results from implementing our IV estimation strategy in Table 4. For reference purposes, OLS results from column (4) of Table 3 are reproduced in the first column of each panel. In Panel A, we use predicted probabilities from models (4) and (7) of Table 2 as instruments for college and high school, respectively. With probabilities from these models predicted by all available interacted cohort shocks affecting college and high school, our estimates in column (2) are exactly identified. Consistent with ability bias, IV estimates of the returns to college are lower than the OLS estimates. The return to college relative to high school falls by 12 percent from 42.0 percent to an average of 37.1 percent. This rate of return is 26% lower than the estimated return of 49.8 percent from a conventional Mincer-type specification with region and cohort fixed effects (Table 3 column 1). However, the return to high school relative to middle school rises from 20.9 to 28.2 percent when using the IVs. This contrasts with the findings of Li, Liu, and Zhang (2012) who report that for twins, the return to high school compared to not completing high school is not statistically different from zero. The higher IV estimate of the returns to high school compared to OLS is consistent with an ability bias story in which selection into college creates a negative ability bias for those who complete only high school. In column (3), we present IV estimates using predicted probabilities from pairs of interaction terms in columns (1), (2) and (3) of Table 2 for college, and columns (5) and (6) for high school. Our IV estimates of the returns to college and high school education are virtually unchanged. The instruments easily pass an over-identification test, suggesting that there is no statistical evidence against the validity of our instruments.

Overall, we observe a slight decline in the estimated return to a year of schooling from 8.3 percent to a range of 8.0 to 8.2 percent in our IV estimates (Panel B of Table 4). Examining the differences in the return to a year of post-secondary schooling relative to returns to a year of schooling for grades 1 through 12, we obtain results consistent with positive ability bias for college and negative ability bias for high school (Panel C). Comparing the results of OLS to IV, we find that the 11.9 percent per year return to college education falls to 9.9 percent per year when estimating the exactly identified IV regression reported in column 2, while the return to a year of high school education rises from 5.3 to 7.0 percent.[22]

5. Robustness

5.1. Migration and sample selectivity

One concern about our sample is that it consists of only individuals who were living in the sample cities at the time of the survey in November 2001. It is possible that a large number of individuals educated in each city had moved elsewhere, for example due to migration to other cities or because they were sent down youth who never returned to the city. If the extent and nature of sample selectivity varied across cohorts in a manner correlated with the magnitude of educational disruptions, this could introduce bias to our estimates. Changing selectivity could lead us not only to mis-measure the extent of disruptions caused by the Cultural Revolution for different cohorts, but also to misattribute changes in the correlations among variables (such as changing parent-child correlations in educational attainment) to educational shocks instead of changes in selectivity bias. Despite this potential, one would still require a specific story of how cohort specific selectivity bias is correlated with the magnitude of education disruptions for there to be a reason to suspect that our IV estimates are biased (we are unable to come up with one).

[22] Like many studies in this literature, despite the large magnitude of the effects on the estimated returns to schooling, the IV estimates are not sufficiently precise to be statistically significantly different from the OLS estimates using Hausman tests.

Nonetheless, it is possible to study the magnitude of sample selection by analyzing data from the 2000 census, which asked about respondents' current place of residence, the province in which they were born, and their current residential registration status (agricultural or non-agricultural *hukou*). We can then examine, at the province level, educational attainment outcomes for individuals registered as urban residents and currently living in urban areas, for urban-registered individuals born in the province and continuing to reside in the province, and for urban-registered individuals born in the province and living anywhere in the country. In Figure 5, we present evidence on educational attainment for these three different groups for each CULS province.

The results for Shanghai are particularly instructive because Shanghai is both a city and a province, so that the census and CULS cover the same urban population. Shanghai is also a city that experienced very large educational disruptions. Figure 5 shows that in Shanghai the disruption to education was substantial and virtually indistinguishable for individuals born in Shanghai and still living in Shanghai (the solid black line) and for individuals born in Shanghai and living anywhere in China (the dotted gray line). For the other provinces where CULS cities are located, we also find that the patterns of educational attainment across cohorts are nearly identical for current urban residents born in the province and still residing in the province (solid black line) and for urban residents born in the province but residing anywhere in China (grey dotted lines). Overall, the evidence presented in Figure 5 suggests that selectivity bias associated with migration is very unlikely to be important.[23]

---

[23] Figure 5 also shows that education disruptions due to the Cultural Revolution are not as apparent for provinces other than Shanghai. This could be driven by two factors: reporting of correspondence degrees as actual degrees and the fact that disruptions to education were not as great in smaller cities as in large cities. As the power base of the *Gang of Four*, who were viewed as responsible for many of the excesses of the Cultural Revolution, Shanghai was far more strident in promoting revolutionary policies during the Revolution.

5.2. Alternative pathways

Because the Cultural Revolution was a major political event involving many policy changes and significant social turmoil, one might be concerned that educational disruptions are associated with other unobserved impacts of the Cultural Revolution on individuals that are correlated with later productivity. For example, education disruptions could have created mental stress that influenced long-term health outcomes, or could have occurred at the same time as disruptions to the health care system (or be correlated with other policies) which affected health outcomes. The latter effect, however, is unlikely because it requires that correlated policies have age-specific effects similar to educational disruptions (for example, they should affect school-age children but not pre-school age children). For our estimates to be biased due to alternative pathways, it also would require that the magnitude of Cultural Revolution effects on alternative pathways be correlated with parental characteristics in the same way as educational outcomes.

One way to address the concern about alternative pathways is to evaluate the extent to which our instruments are correlated with measures of those alternative pathways, such as physical and psycho-social health outcomes. To do this, we regress dummy variables for chronic illness, physical deformity, and an index of psycho-social health on the same set of regressors and instrument sets used in the first stage regressions reported in Table 2. We present results of F-tests for the different instrument sets in Table 5. In no case are the instruments found to be jointly statistically significant, so there is no evidence that educational disruptions are systematically related to health outcomes. This contrasts sharply with the results presented in Table 4, which show that our instruments for college and high school enrollment have F-statistics well over 20 in every instance. The lack of any impact of the instruments on health outcomes also rules out concern that our shocks are correlated with exposure to shocks to the famine of 1959-61 in a way that might bias our estimates.[24]

Although we find no impacts of our instruments on depressive symptoms, it could be the case that experiencing educational shocks, in particular being a sent down youth, provided life experiences that had positive long-term impacts on the productivity of those sent down. Li, Rosenzweig, and Zhang (2010) analyze data on identical twins and find that each year spent as a sent down youth increases wages by 4.2 percent, which is even higher than an additional year of schooling, which increases wages by 3.0 percent. If being sent down reduces years of schooling but increases labor productivity, OLS estimates of the returns to education will be downward biased. On the other hand, they also provide evidence that parents were more likely to send down lower endowment (less productive) children, which would lead to upward bias. However, any net correlation between being sent down and unobserved labor productivity will not cause bias in our estimates unless the correlation varies systematically with fathers' education or fathers' high administrative status.

A possible concern in this regards is that parental characteristics could have influenced not only educational attainment but also early post-education employment opportunities to a greater extent when Cultural Revolution shocks were greater. First, this is unlikely because the timing of measured shocks to education do not match the timing of labor market entry. Second, and relatedly, labor market impacts were unlikely to persist in the same way as shocks to educational attainment because the onset of economic reform in 1978 significantly altered the distribution of employment opportunities. Finally, even if such bias exists, this should lead

[24] The famine also did not have nearly as great an impact in urban areas as it did in rural areas. We would also expect the interaction terms between famine shocks and parental education to have a positive rather than negative coefficient, since better educated parents and their children should have fared better during the famine.

to upward bias in our estimates of the returns to schooling assuming that more educated parents improved early labor market placements more when shocks were greater. Such bias cannot explain why the estimated returns to college education using IVs fall substantially compared to the OLS estimates or why the IV estimates compared to OLS estimates are lower for college returns but higher for high school returns.

6. Conclusion

In this study, we empirically estimate the impact that the Cultural Revolution had on the educational attainment of urban residents by analyzing a unique dataset with all of the detailed information required for a systematic assessment of such impacts. We find that the Cultural Revolution significantly reduced educational attainment of affected cohorts and that the educational attainment of children affected by the Cultural Revolution were much less correlated with parents' education than non-affected cohorts and more correlated with whether their parents held administrative positions.

We also exploit the heterogeneous impacts of educational supply shocks associated with the Cultural Revolution to identify the causal effect of schooling on wages. An advantage of our approach is that, unlike prior studies based on cohort comparisons, we examine variation in shocks to individuals within city-cohorts associated with parental characteristics, which enables us to control for the many cohort differences within cities that are likely to be correlated both with productivity and the timing of the Cultural Revolution.

Our estimates do not account for general equilibrium effects on the returns to schooling caused by the Cultural Revolution's impact on the aggregate relative supply of workers with different levels of education. Accounting for such effects would be important if our goal were to evaluate the welfare effects of the Cultural Revolution which is beyond the scope of this paper.[25] But such concerns do not undermine our use of the Cultural Revolution as an exogenous source of variation in educational attainment to identify the private returns to schooling in China's urban labor market in 2001.

We find that IV estimates of the returns to college are notably lower than OLS estimates, while the opposite is true for the returns to high school. We interpret these results to be evidence of selectivity bias associated with higher ability students entering college. This is an important finding because it suggests that simple estimates of the returns to schooling in countries with highly selective entrance exams that affect school progression may overestimate the private returns to higher education. If policymakers view the high return to college education as an indication of the scarcity of university graduates, rather than evidence of selection effects associated with entrance examinations, this may lead to support for more rapid expansion in the capacity of higher education institutions than would be otherwise warranted.

References

Almond, Douglas (2006). "Is the 1918 Influenza Pandemic Over? Long-term Effects of In Utero Influenza Exposure in the Post-1940 US Population," *Journal of Political Economy* v114, n4 (August 2006): 672-712.

Angrist, Joshua D., and Alan B. Krueger (1991). "Does Compulsory School Attendance Affect Schooling and Earnings." *Quarterly Journal of Economics* 106: 979-1014.

Bound, John & Solon, Gary (1999). "Double trouble: on the value of twins-based estimation of the return to schooling," *Economics of Education Review* 18(2): 169-182.

Byron, Raymond, and Evelyn Manaloto (1990). "Returns to Education in China," *Economic Development and Cultural Change* 38: 783-796.

[25] For example, if estimated returns to high school and college are taken to be exogenous, we estimate that wages of the the Cultural Revolution cohort would have been 9 percent higher in the absence of shocks to high school and college attainment.

Card, David (2001). "Estimating the Return to Schooling: Progress on Some Persistent Econometric Problems." *Econometrica* 69(5): 1127-1160.

Card, David (1995). "Using Geographic Variation in College Proximity to Estimate the Return to Schooling," in Lous N. Christofides, E. Kenneth Grant, and Robert Swidinsky, eds., *Aspects of Labour Market Behaviour: Essays in Honour of John Vanderkamp* (Toronto: University of Toronto Press).

de Brauw, Alan, and Scott Rozelle (2008). "Reconciling the Returns to Education in Off-Farm Wage Employment in Rural China", *Review of Development Economics* 12(1): 57-71.

Deng, Z., and Don J. Treiman (1997). "The Impact of the Cultural Revolution on Trends in Educational Attainment in the People's Republic of China." *American Journal of Sociology* 103(2): 391-428.

Du, Yang, and John Giles (2006). "The Impact of City-Level Labor Market Shocks to Employment on Household Educational Investment Decisions" [Chéngshì láodònglì shìchǎng shàng de jiùyè chōngjí duì jiātíng jiàoyù juécè de yǐngxiǎng] *Economic Research* [*Jingji Yanjiu*] 4(2006): 1-13.

Duflo, Esther (2001). "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from and Unusual Policy Experiment." *American Economic Review* 91(4): 795-813.

Giles, John, Albert Park and Fang Cai (2006). "How has Economic Restructuring Affected China's Urban Workers?" *China Quarterly*, 185 (March 2006): 61-95.

Harmon, Colm, and Ian Walker (1995). "Estimates of the Economic Return to Schooling for the United Kingdom," *American Economic Review* 85: 1278-1286.

Heckman, James, and Xuesong Li (2004). "Selection Bias, Comparative Advantage and Heterogeneous Returns to Education: Evidence from China in 2000," *Pacific Economic Review* v9, n3 (Special Issue October 2004): 155-71.

Ichino, Andrea, and Rudolf Winter-Ebmer (2004). "The Long-Run Educational Cost of World War II: An Example of Local Average Treatment Effect Estimation," *Journal of Labor Economics* 22(1) (January 2004): 57-86.

Kane, Thomas J., and Cecilia E. Rouse (1993). "Labor Market Returns to Two- and Four-Year Colleges: Is a Credit a Credit and Do Degrees Matter?" NBER Working Paper No. 4268.

Lemieux, Thomas, and David Card (1998). "Education, Earnings, and the 'Canadian G.I. Bill'." NBER Working Paper No. 6718.

Li, Haizheng (2003). "Economic Transition and Returns to Education in China," *Economics of Education Review* 22: 317–328.

Li, Hongbin, Lei Li, Binzhen Wu, and Yanyan Xiong (2012). "The End of Cheap Chinese Labor," *Journal of Economic Perspectives* 26(4): 57-74.

Li, Hongbin, Pak Wai Liu, and Junsen Zhang (2012). "Estimating Returns to Education Using Twins in China", *Journal of Development Economics* 97(2): 494-504.

Li, Hongbin, Mark Rosenzweig, and Junsen Zhang (2010). "Altruism, Favoritism, and Guilt in the Allocation of Family Resources: Sophie's Choice in Mao's Mass Send-Down Movement", *Journal of Political Economy* 118(1): 1-38.

Meisner, Maurice J. (1986). *Mao's China and After: A History of the People's Republic*, Collier-Macmillan: New York, 1986.

Meng, Xin, and R. G. Gregory (2002a). Exploring the Impact of Interrupted Education on Earnings: the Educational Cost of the Chinese Cultural Revolution. Unpublished manuscript, Australia National University.

Meng, Xin, and R. G. Gregory (2002b). "The Impact of Interrupted Education on Subsequent Educational Attainment: A Cost of the Chinese Cultural Revolution." *Economic Development and Cultural Change* 50(4): 935-959.

Meng, Xin and Junsen Zhang (2001). "The Two-Tier Labor Market in Urban China: Occupational Segregation and Wage Differentials between Urban Residents and Rural Migrants in Shanghai," *Journal of Comparative Economics* 29(3): 485-504.

Neumark, David (1999). "Biases in twin estimates of the return to schooling," *Economics of Education Review* 18(2): 143-148.

Pepper, Suzanne (1996). *Radicalism and Education Reform in Twentieth-Century China*. Cambridge University Press.

Staiger, Douglas, and James Stock (1997). "Instrumental Variables Regression with Weak Instruments." *Econometrica* 65: 557-586.

Wooldridge, Jeffrey (2002). *Econometric Analysis of Cross Section and Panel Data,* MIT Press, Cambridge.

Zhang, Junsen, Yaohui Zhao, Albert Park, and Xiaoqing Song (2005). Economic Returns to Schooling in Urban China, 1988 to 2001. *Journal of Comparative Economics* 33: 730-752.

Zhou, Xueguang, and Liren Hou (1999). "Children of the Cultural Revolution: the State and the Life Course in the People's Republic of China", *American Sociological Review* 64: 12-36.

Figure 1
Higher Education Entrants, Graduates, and Enrollments for the China, 1949-1990

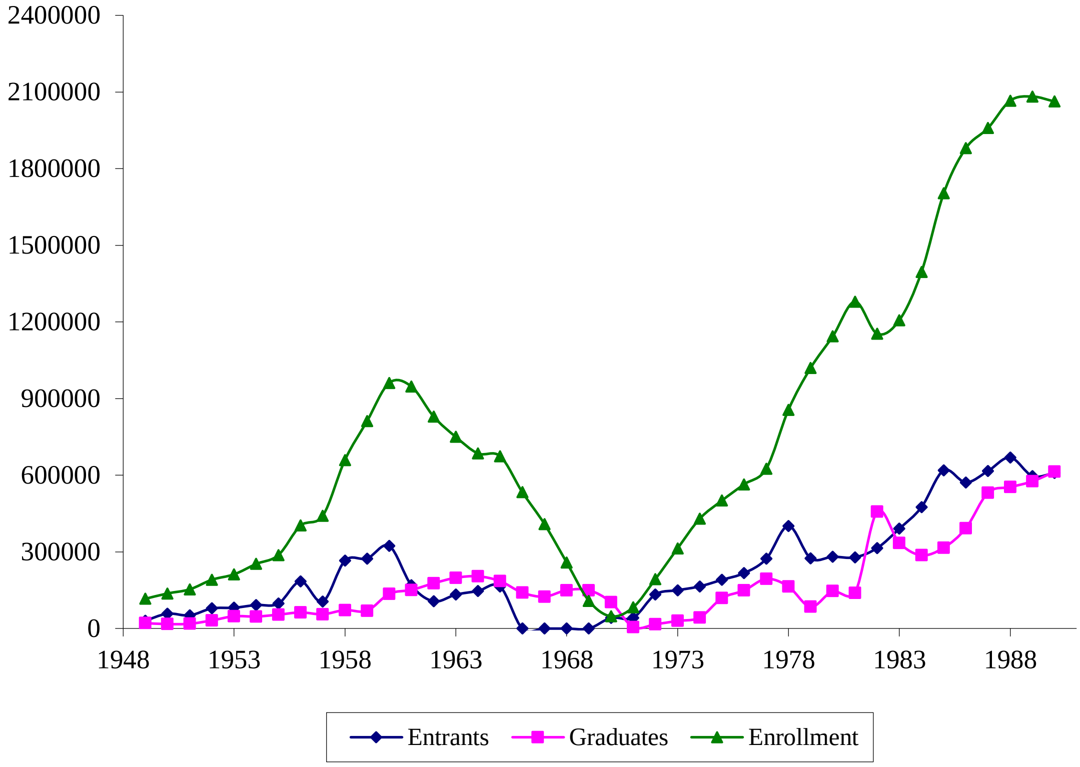


Figure 2
Educational Attainment by Age Cohort for China's Urban Population
Evidence from the 2000 Census

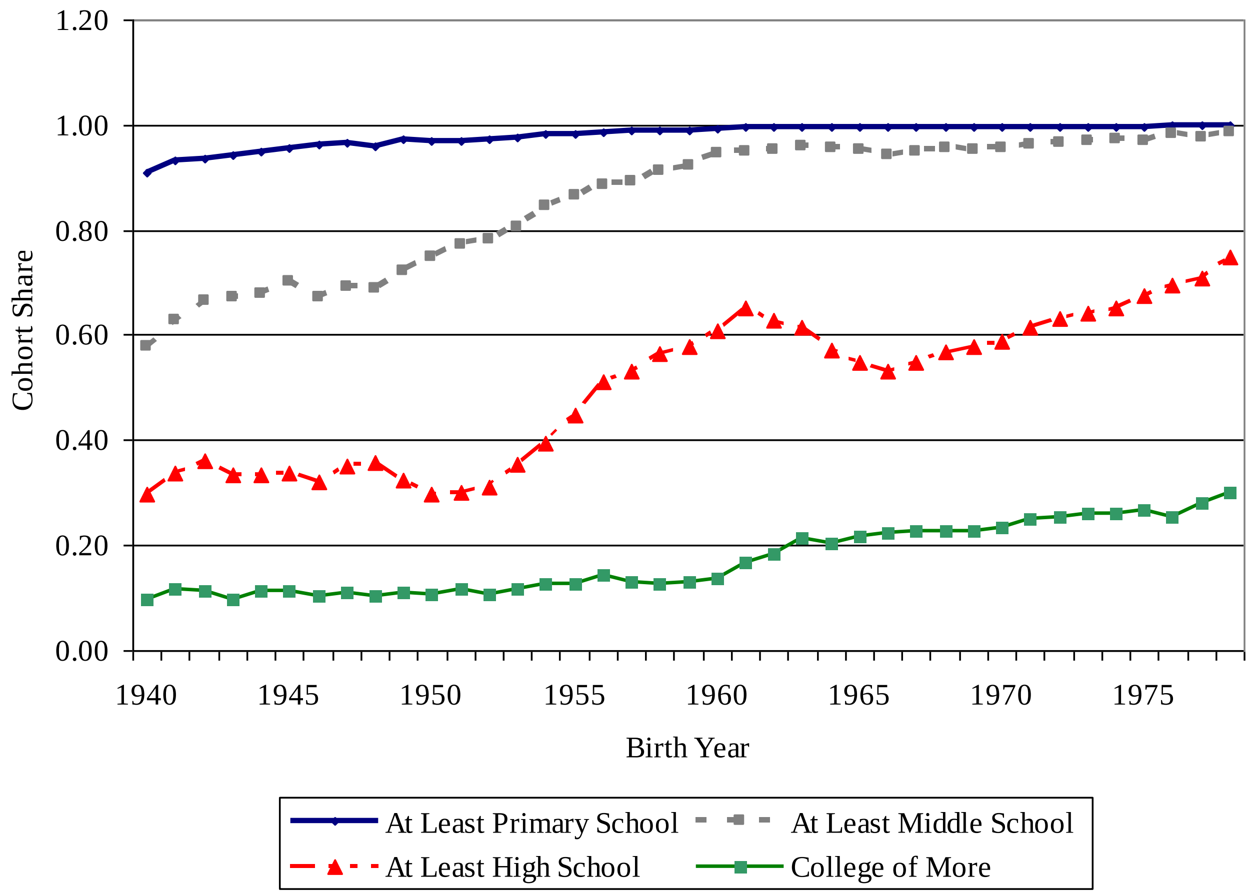
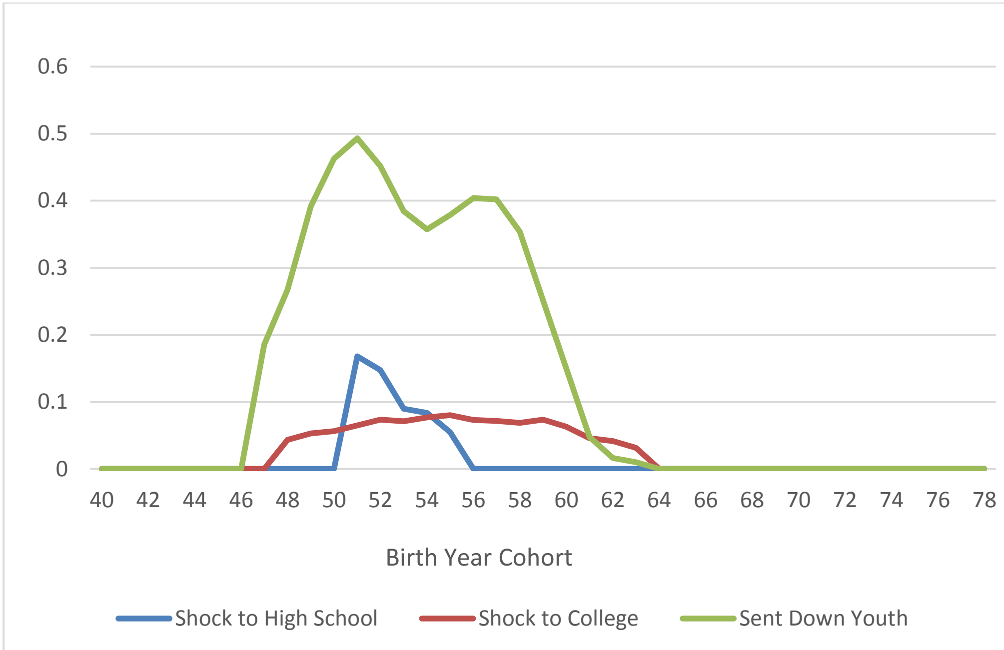
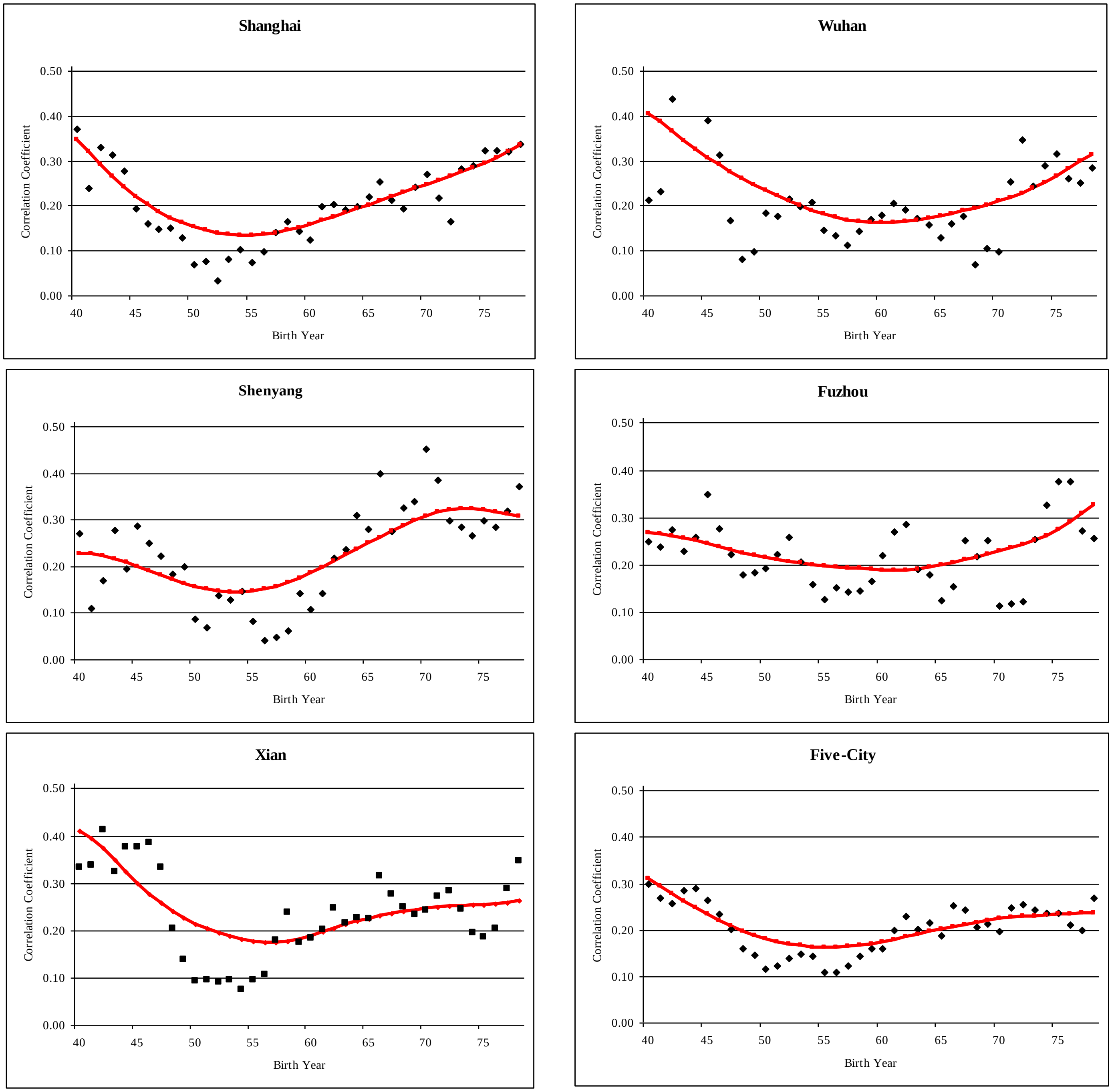


Figure 3
Evidence On Cultural Revolution Shocks



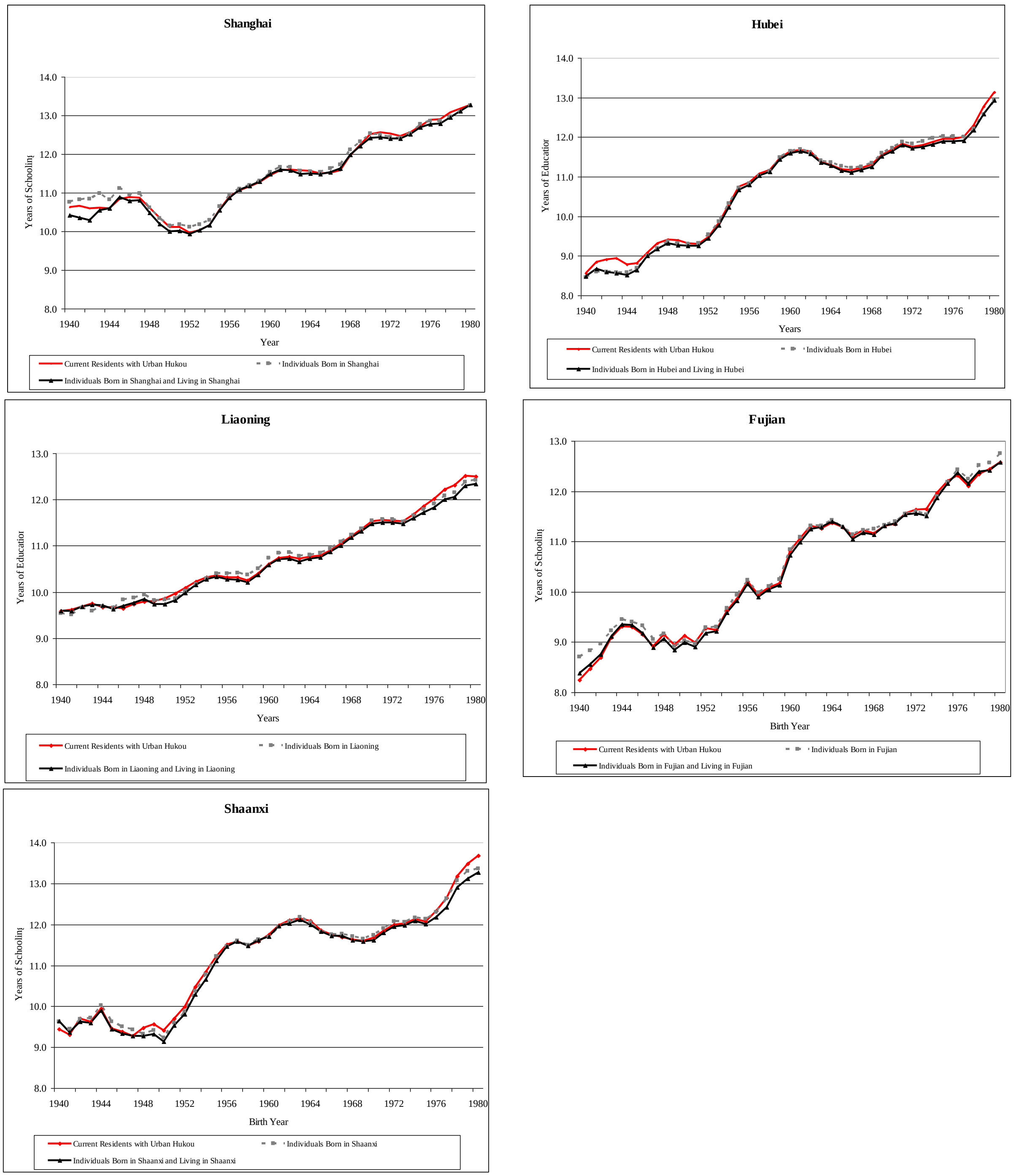
Note: A “positive” shock represents a negative shock experienced by the cohort. In the figure above, negative shocks to high school attainment reflect increases in availability of high school education during the later years of the Cultural Revolution.

Figure 4
Correlation of Educational Attainment with Father's Educational Attainment
(Lowess Plots of Correlation Coefficients Versus Birth Cohort)



Notes: Correlation of father's education with own child's education is calculated after controlling for whether or not father was a high administrator.

Figure 5
Does Selective Migration Influence Observed Patterns in Years of Schooling of the Urban Population in CULS Provinces?
Evidence from the 2000 Census



Source: 2000 Population Census.

Table 1
Descriptive Statistics By Birth Year Cohort
(Standard Deviation)

	Birth Year Cohort			All
	1940 to 1947	1948 to 1963	1964 to 1978	1940 to 1978
Years of Education	10.21	10.54	12.18	10.94
	(3.65)	(2.80)	(2.93)	(3.10)
Completed Middle School	0.82	0.93	0.97	0.92
	(0.38)	(0.26)	(0.17)	(0.27)
Completed High School	0.59	0.55	0.74	0.61
	(0.49)	(0.50)	(0.44)	(0.49)
Completed Post-Secondary	0.062	0.036	0.189	0.084
	(0.241)	(0.188)	(0.392)	(0.277)
Age	57.25	45.54	31.00	43.31
	(2.03)	(4.41)	(4.34)	(9.74)
Female	0.52	0.51	0.52	0.52
	(0.50)	(0.50)	(0.50)	(0.50)
Height	164.5	165.7	166.5	165.7
	(7.5)	(7.6)	(7.4)	(7.6)
Father's Years of Education	4.50	5.45	8.88	6.36
	(4.56)	(4.57)	(4.43)	(4.83)
Mother's Years of Education	2.12	3.27	7.19	4.30
	(3.57)	(4.11)	(4.46)	(4.58)
Father's Ed Missing	0.18	0.03	0.01	0.06
	(0.39)	(0.18)	(0.11)	(0.24)
Mother's Ed Missing	0.18	0.03	0.01	0.06
	(0.38)	(0.17)	(0.10)	(0.24)
Number of Living Siblings	3.08	3.00	2.21	2.81
	(1.66)	(1.41)	(1.25)	(1.45)
No Siblings Reported	0.29	0.12	0.32	0.21
	(0.46)	(0.33)	(0.47)	(0.41)
Father was a Farmer	0.244	0.111	0.116	0.130
	(0.429)	(0.315)	(0.321)	(0.336)
Father was a Worker	0.408	0.601	0.485	0.532
	(0.491)	(0.489)	(0.499)	(0.499)
Father was Self-Employed	0.032	0.019	0.019	0.021
	(0.176)	(0.138)	(0.137)	(0.144)
Father was a Private Businessman	0.015	0.004	0.005	0.005
	(0.123)	(0.063)	(0.072)	(0.076)
Father was an Administrator	0.035	0.102	0.144	0.103
	(0.186)	(0.303)	(0.351)	(0.304)
Father was a Technician	0.063	0.093	0.186	0.114
	(0.243)	(0.290)	(0.389)	(0.317)
Father was a High Administrator	0.016	0.049	0.065	0.048
	(0.128)	(0.217)	(0.246)	(0.215)
Father had High Technician Status	0.040	0.057	0.136	0.076
	(0.197)	(0.233)	(0.343)	(0.266)

Table 1 Continued on Next Page

Table 1 (Continued)
Descriptive Statistics By Birth Cohort
(Standard Deviation in Parentheses)

	Birth Year Cohort			All
	1940 to 1947	1948 to 1963	1964 to 1978	1940 to 1978
Mother was a Farmer	0.261	0.152	0.148	0.164
	(0.439)	(0.359)	(0.356)	(0.370)
Mother was a Worker	0.283	0.554	0.536	0.503
	(0.451)	(0.497)	(0.498)	(0.500)
Mother was Self-Employed	0.017	0.013	0.018	0.015
	(0.132)	(0.115)	(0.135)	(0.122)
Mother was a Private Business Owner	0.007	0.001	0.002	0.002
	(0.084)	(0.034)	(0.053)	(0.049)
Mother is/was an Administrator	0.005	0.016	0.036	0.020
	(0.077)	(0.127)	(0.186)	(0.140)
Mother is/was a Technician	0.027	0.050	0.131	0.069
	(0.163)	(0.218)	(0.338)	(0.254)
School Characteristics				
Attended Vocational Technical High School	0.027	0.025	0.106	0.048
	(0.163)	(0.156)	(0.308)	(0.214)
Attended Magnet High School	0.079	0.032	0.104	0.059
	(0.269)	(0.176)	(0.305)	(0.237)
In Magnet Class of Regular High School	0.035	0.051	0.105	0.064
	(0.186)	(0.220)	(0.307)	(0.245)
Elementary School in County Seat	0.043	0.029	0.050	0.037
	(0.203)	(0.168)	(0.218)	(0.190)
Elementary School in Town or Village	0.153	0.090	0.138	0.113
	(0.360)	(0.286)	(0.345)	(0.317)
Middle School in County Seat	0.047	0.030	0.056	0.041
	(0.213)	(0.172)	(0.230)	(0.198)
Middle School in Township or Village	0.080	0.065	0.114	0.081
	(0.271)	(0.247)	(0.317)	(0.274)
High School in County Seat	0.019	0.018	0.054	0.029
	(0.137)	(0.134)	(0.226)	(0.169)
High School in Township	0.015	0.021	0.034	0.024
	(0.123)	(0.143)	(0.182)	(0.153)
Observations	713	3530	1718	5961

Table 2
How Do Cultural Revolution Shocks Influence Ability to Attend High School and College?
Marginal Effects from Probit Models

Model DepVar	1 college ?	2 college ?	3 college ?	4 college ?	5 high?	6 high ?	7 high ?
College-Completion-by-25 Shock x Father's Education	-0.209 (0.130)			0.556 (0.305)			
College-Completion-by-25 Shock x Father High Administrator	6.577 (2.577)			7.239 (4.991)			
High School Shock x Father's Education		-0.193 (0.087)		-0.127 (0.105)	-0.211 (0.080)		-0.152 (0.094)
High School Shock x Father High Administrator		1.383 (3.339)		-0.252 (4.001)	-0.040 (1.767)		-2.234 (1.979)
Cohort Sent Down Youth Share x Father's Education			-0.063 (0.035)	-0.128 (0.061)		-0.074 (0.029)	-0.049 (0.036)
Cohort Sent Down Youth Share x Father High Administrator			0.903 (0.503)	-0.068 (1.096)		0.922 (0.697)	1.254 (0.781)
Age	0.098 (0.135)	0.104 (0.136)	0.119 (0.140)	0.115 (0.138)	-0.182 (0.142)	-0.204 (0.145)	-0.196 (0.145)
Age-Squared	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)	0.002 (0.002)	0.002 (0.002)	0.002 (0.002)
Female	-1.853 (2.096)	-1.803 (2.112)	-1.923 (2.095)	-1.945 (2.103)	1.539 (1.613)	1.512 (1.598)	1.488 (1.610)
Height (z-score, by gender)	-0.002 (0.008)	-0.002 (0.008)	-0.002 (0.008)	-0.002 (0.008)	0.009 (0.006)	0.009 (0.006)	0.009 (0.006)
Height*Female	0.001 (0.013)	0.010 (0.013)	0.010 (0.013)	0.010 (0.013)	-0.009 (0.010)	-0.009 (0.010)	-0.009 (0.010)
Father's Years of Schooling	0.042 (0.011)	0.044 (0.011)	0.051 (0.012)	0.045 (0.011)	0.052 (0.008)	0.062 (0.011)	0.061 (0.011)
Mother's Years of Schooling	0.026 (0.010)	0.026 (0.010)	0.026 (0.009)	0.025 (0.010)	0.012 (0.009)	0.013 (0.009)	0.012 (0.009)
Joint Significance of "Instruments"							
Chi-Square Statistic	10.12	8.96	5.13	18.56	7.87	6.19	13.54
Chi-Probability	0.025	0.011	0.077	0.005	0.019	0.045	0.019
Obs	3611	3611	3599	3599	3611	3599	3599

Notes:

(1) College-Completion-by-25 Shock: The deviation of average college attainment by age 25 (conditional on high school completion) for birth cohort relative to pre and post-Cultural Revolution trend.

(2) High School Shock: The deviation of average high school attainment (conditional on middle school completion) for birth cohort relative to pre and post-Cultural Revolution trend.

(3) All models include number of siblings, dummy variable for missing information on siblings and mother and father's education, and vectors of school quality and location variables and parent occupation dummy variables.

(4) All models include statistically significant (city) x (three-year birth cohort) interactions.

(5) All models show robust standard errors cluster corrected at the three-year city cohort level.

Table 3
Returns to Education
OLS Models

Panel A: Returns to Middle School, High School and College Attainment

Model	1	2	3	4
Variable	ln(wage)	ln(wage)	ln(wage)	ln(wage)
College	0.498	0.497	0.471	0.420
	(0.029)	(0.029)	(0.028)	(0.030)
High School	0.256	0.245	0.216	0.209
	(0.027)	(0.027)	(0.028)	(0.029)
Middle School	0.254	0.270	0.242	0.234
	(0.062)	(0.063)	(0.063)	(0.064)
Other Included Regressors				
City and Birth Year Dummy Variables	Yes	No	No	No
Three Year Birth Cohort x City Dummy Variables	No	Yes	Yes	Yes
Height, Female and Height x Female	No	No	Yes	Yes
Parent Education Variables and Number of Siblings	No	No	Yes	Yes
School Quality and Parent Occupation Variables	No	No	No	Yes
N	3614	3614	3611	3611
R-Squared	0.282	0.295	0.310	0.324

Panel B: Returns to Years of Schooling -- Linear in Schooling

Model	1	2	3	4
Variable	ln(wage)	ln(wage)	ln(wage)	ln(wage)
Years of Schooling	0.096	0.096	0.089	0.083
	(0.004)	(0.004)	(0.004)	(0.004)
Other Included Regressors				
City and Birth Year Dummy Variables	Yes	No	No	No
Three Year Birth Cohort x City Dummy Variables	No	Yes	Yes	Yes
Height, Female and Height x Female	No	No	Yes	Yes
Parent Education Variables and Number of Siblings	No	No	Yes	Yes
School Quality and Parent Occupation Variables	No	No	No	Yes
N	3613	3613	3610	3610
R-Squared	0.293	0.306	0.319	0.333

Table 3 (OLS Models, Continued)

Panel C: Returns to Years of Schooling -- Piecewise Linear Spline

Model	1	2	3	4
Variable	ln(wage)	ln(wage)	ln(wage)	ln(wage)
Years of Schooling for Years>12	0.135	0.135	0.129	0.119
	(0.007)	(0.007)	(0.007)	(0.007)
Years of Schooling for Years<=12	0.063	0.062	0.054	0.053
	0.007	0.008	0.007	0.008
Other Included Regressors				
City and Birth Year Dummy Variables	Yes	No	No	No
Three Year Birth Cohort x City Dummy Variables	No	Yes	Yes	Yes
Height, Female and Height x Female	No	No	Yes	Yes
Parent Education Variables and Number of Siblings	No	No	Yes	Yes
School Quality and Parent Occupation Variables	No	No	No	Yes
N	3613	3613	3610	3610
R-Squared	0.302	0.316	0.329	0.341

Notes:

All models include measures of age and age-squared (age is measured from month of birth to November 2001), height (in centimeters), an indicator for gender (female=1), and height **x** gender. Robust standard errors are cluster corrected at the city-birth cohort level.

Table 4
Returns to Education
IV Models

Panel A: Returns to Middle School, High School and College Attainment

Model #	1	2	3	4
Model	OLS	IV	IV	IV
Dependent Variable	ln(wage)	ln(wage)	ln(wage)	ln(wage)
College*	0.420	0.379	0.362	0.373
	(0.030)	(0.177)	(0.167)	(0.177)
High School*	0.209	0.288	0.301	0.287
	(0.029)	(0.121)	(0.124)	(0.122)
Middle School	0.234	0.192	0.190	0.197
	(0.064)	(0.087)	(0.089)	(0.088)
Significance of Instruments				
College: F-Test	-	53.51	59.44	22.28
F-Probability	-	0.00	0.00	0.00
High School: F-Test	-	134.82	132.61	56.47
F-Probability	-	0.00	0.00	0.00
Over-Identification Test				
Hansen J-Statistic	-	-	-	0.447
Chi-Square P-Value	-	-	-	0.930
Observations	3611	3611	3599	3599
R-Squared	0.324	-	-	-

Panel B: Returns to Years of Schooling -- Linear in Schooling

Model #	1	2	3	4
Model	OLS	IV	IV	IV
Dependent Variable	ln(wage)	ln(wage)	ln(wage)	ln(wage)
Years of Schooling	0.083	0.080	0.082	0.080
	(0.004)	(0.017)	(0.017)	(0.017)
Significance of Instruments				
Years of Schooling: F-Test		56.04	84.60	35.20
F-Probability		0.00	0.00	0.00
Over-Identification Test				
Hansen J-Statistic		0.104	0.040	0.283
Chi-Square P-Value		0.747	0.846	0.991
Observations	3610	3610	3598	3598
R-Squared	0.333	-	-	-

Table 4 Continued on Next Page

Table 4 Continued

Panel C: Returns to Years of Schooling -- Piecewise Linear Spline

Model	1	2	3	4
Variable	ln(wage)	ln(wage)	ln(wage)	ln(wage)
Years of Schooling for Years>12	0.119	0.099	0.096	0.098
	(0.007)	(0.039)	(0.037)	(0.040)
Years of Schooling for Years<=12	0.053	0.070	0.074	0.070
	(0.008)	(0.027)	(0.027)	(0.027)
Significance of Instruments				
Years of Schooling >12: F-Test	-	44.34	45.49	18.32
F-Probability	-	0.00	0.00	0.00
Years of Schooling<=12: F-Test	-	133.3	130.8	56.25
F-Probability	-	0.00	0.00	0.00
Over-Identification Test				
Hansen J-Statistic	-	-	-	0.227
Chi-Square P-Value	-	-	-	0.973
Observations	3610	3610	3598	3598
R-Squared	0.341	-	-	-

Notes To Table 3:

(1) All models include jointly significant three year birth-cohort X city dummies, school quality and location variables, parent occupation dummies and educational attainment variables, number of siblings, age and age-squared (with age measured from month of birth to November 2001), gender (female=1), height, and gender x height, and jointly significant three-year birth cohort x city dummy variables. We report robust standard errors that are cluster corrected at the three-year birth cohort x city level.

(2) Instruments for high school and college in model (2) are predicted probabilities of college completion calculated from model (1) and of high school completion from model (5) of Table 2. Note, this model is exactly identified in Panels A and C, and so we do not report an over-identification test.

(3) Instruments in model (3) are predicted probabilities from models (4) and (7) of Table 2, for college and high school, respectively.

(4) Instruments in model (4) are predicted probabilities of high school attainment from models (5) and (6) of Table 2, and predicted probabilities of college attainment using models (1), (2) and (3).

Table 5
Test of Potential Significance of Alternative Channels for Cultural Revolution Effects
F-Statistics on Instruments from Alternative First-Stage Regressions

Model		1	2	3
		OLS	OLS	OLS
Instrument Set		Chronic Illness?	Physical Deformity?	Pscho-Social Health
Instrument Set 1: Predicted Shocks to High School and College From Models (1) and (5) of Table 2	F-Test	1.67	0.40	0.97
	F-Probability	0.196	0.668	0.384
R-Squared		0.120	0.036	0.079
Instrument Set 2: Predicted Shocks to High School and College from Models (4) and (7) of Table 2	F-Test	1.85	0.36	1.58
	F-Probability	0.166	0.698	0.213
R-Squared		0.121	0.036	0.078
Instrument Set 3: Predicted Shocks to High School and College From Models (1), (2), (3), (5) and (6) of Table 2	F-Test	1.15	1.55	1.19
	F-Probability	0.345	0.171	0.321
R-Squared		0.122	0.037	0.079

Notes:
(1) All models are estimated using 3599 observations.
(2) Each model above is estimated as a reduced form with predicted shocks to high school and college included as regressors. All models include the non-shock control variables included the models of Table 4: number of siblings, dummy variable for missing information on siblings and mother and father's education, vectors of school quality and location variables, parent occupation dummy variables, and (city) x (three-year birth cohort) interactions.
(3) Instrument Set 1: Predicted probabilities from models (1) and (5) of Table 2.
(4) Instrument Set 2: Predicted probabilities from models (4) and (7) of Table 2.
(5) Instrument Set 3: Predicted probabilities from models (1), (2), (3), (5) and (6) of Table 2.