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Estimating returns to education in urban China: Evidence from a natural experiment in schooling reform☆

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ABSTRACT

Whereas there is a large literature on estimating returns to education in China, few studies have attempted to address the endogeneity of schooling choices. We explore the arguably exogenous changes in the duration of secondary education as the instrumental variable to identify the causal effect of years of schooling in urban China. The schooling years in most middle schools were reduced from three years to two years during the Cultural Revolution. The Chinese government gradually restored the middle school education years from two years to three years after 1978. An important feature of these policy changes is their large geographic variations. From local gazetteers, we find out the exact years when education years were reduced from three to two and when they were restored from two to three. Using the exogenous variation in schooling reform, we estimate that the returns to education are 12.7% for both monthly wage and disposable income.

1. Introduction

Education is an important variable that determines economic outputs and explains economic growth in various countries (Griliches, 1970; Becker, 1994). Returns to education determine the allocation of human capital and therefore become one of the most widely estimated parameters in economics since Mincer's seminal work (Mincer, 1974). Estimating a correct return to education is of crucial importance in designing education policies.

Getting the correct number is difficult, however. That education is an endogenous variable is widely acknowledged. Since the late 1990s, economists have proposed various instrumental variables to address the concerns of endogeneity (see Card, 1999 for an excellent review). One important approach is to exploit changes in the education system (Harmon and Walker, 1995; Duflo, 2001; Oreopoulos, 2006; Devereux and Hart, 2010; Devereux and Fan, 2011). The goal of this paper is to estimate the returns to education in urban China. We exploit the secular and geographical variations in the duration of middle school[1] education that results in part of the population receiving a two-year middle school education while the other part receives a three-year education.

The Chinese labor market is an important one to study. China is currently the most populous country and the second-largest economy in the world. However, not much is known about the causal returns to education in China, although a large body of literature addresses the correlation between education and income. Previous research has shown that the returns to education in China are lower compared to those in western countries in the 1980s (Byron and Manaloto, 1990; Xie and Hannum, 1996; Johnson

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[1] In this paper, the term "middle school" refers to both junior high school and senior high school.

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and Chow, 1997; Zhao, 1997; Fleisher et al., 2005). The estimates range from 3.1% to 4.3%. These numbers fall far below global average (11%) during that period (Psacharopoulos, 1985). With the progress of market-oriented reforms, the returns have been rising rapidly during the 1990s to reach approximately the same level as those in developed countries in the early 2000s (Heckman, 2003; Zhang et al., 2005; Wang et al., 2007). Zhang et al. (2005) estimate 14 consecutive years (1988–2001) of returns to education using the Urban Household Survey. They find a dramatic increase in the returns to education, from only 4% in 1988 to 10.2% in 2001.

However, what the above studies have estimated is the correlation between years of schooling and earnings, not the causality. In this paper, we propose a schooling reform as an exogenous shock to estimate the returns to education in urban China. With the progress of the Cultural Revolution since 1966, the schooling years in most middle schools (both junior high and senior high schools) were reduced from three years to two years. After the end of the Cultural Revolution, the Chinese government gradually restored the years of schooling to three years. More importantly, the timelines varied across counties. We use the back-and-forth changes between two- and three-year middle school educational systems from the mid-1960s to the mid-1980s. To our knowledge, our paper is the first to document this reform. We looked through the local gazetteers to find the exact years of policy changes for 162 counties in the China Family Panel Study (CFPS). The instrumental variable estimation suggests that the returns to education are 12.7% for both monthly wage and disposable income.

Some recent studies also attempted to estimate the returns to education in China using other exogenous shocks, such as the Cultural Revolution and the Compulsory Education Law. Giles et al. (2015) use the Cultural Revolution from 1966 to 1976, which disrupted the education system in urban areas, as an arguably exogenous variation to estimate the returns to education. They estimate a return to college education of 37.1%. One concern is whether exposure to the Cultural Revolution is a good instrument for educational attainment. The exclusion restriction, based on the premise that the Cultural Revolution does not affect personal earnings through channels other than education, is unlikely to hold. The impacts of the Cultural Revolution are not restricted to the education system. For example, previous research finds evidence that exposure to the Cultural Revolution has a had direct impact on income (Yang and Li, 2011) and health (Abeysinghe and Gu, 2010; Islam et al., 2015). Fang et al. (2012) use China's enforcement of the Compulsory Education Law since 1986. The law mandated a minimum nine years of education. Using the provincial difference in the dates of effective implementation of the Compulsory Education Law, they find that the law increased overall education years in China by about 0.8 years. The instrumental variable estimation shows that the returns to education are approximately 20%. However, by the time the Compulsory Education Law was enforced, urban children generally had received at least junior high-level education.[2] As a result, the Compulsory Education Law cannot provide a robust estimate to the returns to education in urban China.

We contribute to the literature in three ways. First, the paper uses the changes in the duration of middle school as an alternative exogenous variation. Therefore, the paper provides a new insight into estimating returns to education in urban China using a new instrumental variable. Second, to our knowledge, our paper is the first that uses geographic variations at the county level to estimate the returns to education in China. China is a large country. There can be large geographical and economic variations within the province. Being able to control for county dummies helps to overcome the unobserved county characteristics that can affect both education and earnings. Finally, the paper tries to uncover the channels by which an extra year of schooling boosts income in an instrumental variable framework. Although previous research has broadly discussed possible education channels, they are estimated using an ordinary least square (OLS) framework and are therefore subject to the challenge of endogeneity.

The remainder of this article is organized as follows. Section 2 introduces the institutional background. Section 3 details the data used in this paper. Section 4 presents the econometric models, and Section 5 presents this paper's main result. Section 6 discusses the exclusion restriction of the instrument and further explores the underlying mechanisms. Section 7 presents conclusion.

2. Background

2.1. Three-to-two years changes since the late 1960s

The duration of China's middle school education underwent dramatic changes from 1949 to the 1990s. Before the outbreak of the Cultural Revolution in 1966, the school system in China mainly followed the Soviet Union's system: Students normally studied six years in the primary school, three years in the junior high school, and three years in the senior high school, commonly known as the 6-3-3 system. Beginning in 1964, the government started to reduce years of schooling, claiming that the reform would reduce the burden on students and improve the overall quality of education. In July 1968, Chairman Zedong Mao advocated "shortening years of schooling and revolutionizing the education." Since then, durations at almost all levels of schooling were reduced by one year. During this period, the primary-junior-senior years of schooling were reduced from 6-3-3 years to 5-2-2 years (Hannum, 1999).

2.2. Two-to-three years changes since the late 1970s

After the end of Cultural Revolution in 1976, junior and senior high schools gradually restored the required amount of schooling to three years. A series of reforms in the education system took place from the late 1970s to early 1980s. In August 1977, Xiaoping Deng hosted a forum to discuss the science and education policies. The forum decided to resume the college entrance examination, which had been suspended for 11 years since the outbreak of the Cultural Revolution, and to reform the education system. In

[2] From 2000 census of population, Giles et al. (2015) show that over 95% of cohorts born after 1960 in urban China finish at least junior high school as opposed to a number of 60% in rural China.

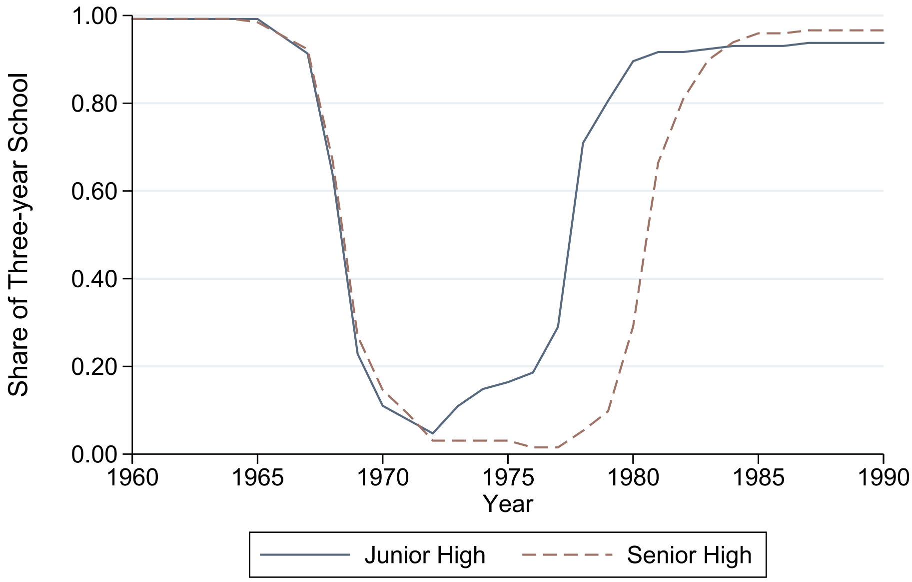

Fig. 1. Evolutions of policies.

February 1978, the Ministry of Education issued its "Tentative Ten-Year Teaching Plan for Full-time Primary and Secondary School." Required years of primary school, junior high school, and senior high school were set to five, three, and two years, respectively. As a consequence, the length of junior high school in most areas started to increase by one year in 1978. In December 1980, the Central Committee of the Chinese Communist Party and the State Council issued decisions that the required years of senior high school education should be extended to three years. In April 1981, the Ministry of Education issued its Tentative Six-Year Teaching Plan for Full-time Key Secondary School and Amendments to Tentative Five-Year Teaching Plan for Full-time Secondary School. In these two documents, the Ministry of Education increased the duration of senior high school to three years and allowed local education authorities to design their own implementation plans. The soft deadline for completing the reform was 1985.

As a result of these party- and state-level decisions, local education authorities could now make their own timetables for reforming secondary schools. Factors affecting the timetable included the number of qualified middle school teachers, the availability of suitable school premises, the number of current and incoming students. Individuals had no control over the timing of the reform. Choosing in which county to receive secondary education was extremely difficult because of China's household registration system, which greatly restricted people's ability to migrate internally. Therefore, the heterogeneity in the timing of school reform years serves as a good source of exogenous variation in years of schooling.

3. Data and variables

We combined two data sets for our empirical analysis. The first data set is a county-level data set that contains all relevant information about the changes in the duration of middle schools. The second data set is the 2010 wave of the China Family Panel Study (CFPS), containing personal information including personal earnings and educational attainment.

3.1. County-level policy data

We collected detailed information on the policy changes regarding the duration of middle school education years from the local gazetteers (*xianzhi*). These are book-length volumes of local history documenting the major events that have ever occurred in the county and are often regarded as the encyclopedia of a locality. Local gazetteers contain information on all aspects of the county, including history, demography, geography, education, transportation, and economy. We compiled a data set that contains information on the years when county-level education authorities (1) cut years of junior and senior high school education from three to two years and (2) started and finished restoring years of middle school education from two years to three years. We managed to find such information for 142 out of 162 counties in the CFPS sample.

Fig. 1 plots the share of counties adopting three-year middle schools. Fig. 2 plots the distributions of years when counties started a two-year system and years when counties started to restore the three-year system. All the CFPS counties maintained a three-year system until 1965. Between 1968 and 1970, almost all counties transitioned from a three-year system to a two-year system. The process occurred simultaneously for junior and senior high school.

In our sample, the first county-level restoration of three-year junior high school education took place in 1971. From 1977 to 1979,

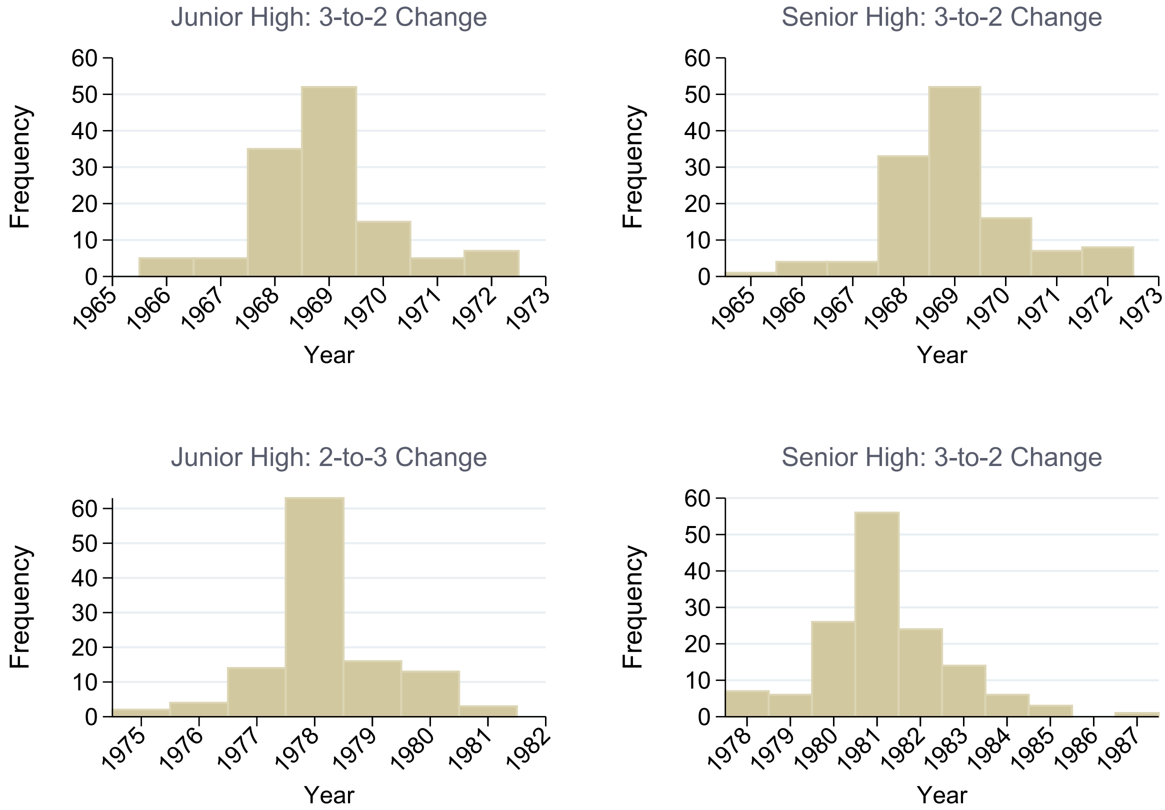

Fig. 2. Distribution of reform years.

three-year junior high school was restored in most counties (68%, 93 out of 135).[3] Senior high schools experienced the change later. The first three-year senior high school restoration began in 1977. From 1980 to 1982, most counties returned to three years of senior high school (73.9%, 105 out of 142). The last observed year of restoration is 1987 for both junior and senior high school.

3.2. CFPS data

As for the individual-level data, we use data from the 2010 wave[4] of China Family Panel Studies (CFPS), launched by the Institute of Social Science Survey of Peking University, China. CFPS covers 25 of 34 provinces or municipalities in China. The CFPS sample is drawn through a multi-stage probability with implicit stratification. Each subsample in the CFPS study is drawn through three stages: county (or equivalent), then village (or equivalent), then household. The 2010 wave of CFPS interviewed almost 15,000 families and almost 30,000 individuals within these families for an approximate response rate of 79%.

CFPS covers a wide range of socioeconomic-related questions. Two key variables of interest for this study are education and income. CFPS contains a detailed set of questions about education history. It does not directly ask individuals how many total years of schooling they have received. Instead, it separates the education history into different stages, from primary school to doctoral degree. For each stage, CFPS asks the exact year when individuals leave school and how many years they have spent in school. This approach helps to reduce the measurement errors in years of education, which would cause an underestimation of returns to education in an OLS setting (Ashenfelter and Krueger, 1994).[5] More importantly, we can ascertain from CFPS the exact number of years the interviewee spent in middle schools. Another important piece of information for which CFPS is often the only source is the type of education individuals have received. CFPS can distinguish whether people receive their education in a full-time school or in an adult

[3] There are seven counties which we cannot find information about junior high school reform.

[4] Currently, four waves of CPFS are publicly available: 2010, 2012, 2014, and 2016. We use only the wave of 2010 for two reasons. First, CFPS is a panel survey tracking a group of households. Because our identification relies on cohort-by-county variations, extra waves do not introduce new variations. Second, the way that CFPS asking about income changes drastically across waves, making it difficult to construct comparable income measures across waves.

[5] However, one type of measurement errors remains: grade repetition. While we know how many years a person spent in each stage of education, we do not know how many years may have been repeated. The measurement errors in the dependent variable can be resolved through an instrumental variable approach, along with the endogeneity issue. In Appendix A, we discuss the measurement errors in years of education in greater detail.

school. In this study, we exclude education received in an adult school because the policies changing the required years of schooling does not affect it and because it is substantially different from a full-time school. For example, a person may receive a junior high education at a full-time middle school but does not proceed to senior high school after graduation. After a couple of years of work, this person receives a senior high education at a school for adults. In this case, we would still treat the person as a junior high graduate.

As for income, we cover both monthly labor income and total income. Labor income comprises wage income, bonus income, and income from a part-time job. Total income also contains transfer income from relatives or government, which further takes into account the income redistribution effect. CFPS also includes information about interviewee's parents, regardless whether they live in the same household as the interviewee. The main parental information for which we control in this paper includes parental education (whether they receive at least a junior high school education), the parents' age at the time of the interviewee's birth, and political status (whether the parents are party members or not).

We further restrict our sample to working-age individuals (from age 30 to age 60)[6] with urban *hukou* (also known as the household registration status), and we have complete information on education and income. We exclude from the sample those who are currently retired or are students. To mitigate the influence of measurement errors and extreme values, we drop the sample whose labor income or total income belongs to the top 1%. We ended up with a CFPS sample of 3,181.

Column 1 in Table 1 reports the summary statistics of the total CFPS sample. They average 44.9 years, with 52.8% being male. The majority of the sample is Han ethnic; only 4.0% are ethnic minorities. On average, people receive 9.9 years of education. In this CFPS sample, 9.1% receive some college education. For 45.0%, the highest level of education attained is senior high school, and for 45.6%, the highest level of education attained is junior high school. These three numbers add up to almost 100.0%, which implies few individuals in our data have not finished junior high school in urban China. In 2010, the average monthly personal income was 2,481 RMB. Labor income accounts for the most of this amount (2,324 RMB, or 93.7% of total income).

We impose three additional requirements to generate the main sample for our analysis: (1) Individuals must have positive disposable income, (2) individuals must have complete information about their parents, and (3) individuals must have graduated from (academic) senior high school. The reasons for imposing the first two restrictions are obvious. We use the logarithm of income as the dependent variable; therefore, we automatically exclude observations with non-positive income. Moreover, family background is an important proxy for unobservable personal characteristics. Therefore, we control for family background in all our regressions. As a result, we exclude observations with missing family background information from the regressions. We focus on senior high graduates because they have more policy-generated variations in years of education. Because changes in both junior and senior high school can potentially affect them. The third requirement also excludes people who graduated from upper-level vocational streams. After the Cultural Revolution, the vocational stream of China's secondary education system went through enormous changes beyond the length of schooling.[7] Column 2 in Table 1 reports the summary statistics of our main sample of analysis.

3.3. Constructing the policy variable

The key instrumental variable used in this paper is the policy-indicated years of middle school education (junior plus senior). We construct this instrument by combining the CFPS data and information from the local gazetteers. Total years of middle school education can be broken down into years in junior high school and years in senior high school. According to local policies, there are three possibilities for schooling: four years (2 + 2), five years (3 + 2 or 2 + 3), and six years (3 + 3). Value 2 is assigned if an individual enters junior or senior high school under the two-year system; otherwise, a value of 3 is assigned.[8]

Ideally, we should use the county when a person received his or her secondary education. Due to data restriction, we use the current residence county instead.[9] Therefore, cross-county migration may be a potential issue.[10] In Appendix A, we show that cross-county migration is essentially the measurement errors in the instrumental variable, which does not bias the instrumental variable (IV) estimation as long as the timing of the reform is not systematically related to some county characteristics. In Section 5, we also explicitly discuss the robustness of our main results when counties starting the reform in different years have heterogeneous trends.

[6] Because new education policies in China typically take effect in September, we add one year to an individual who is born in or later than September.

[7] Prior to the Cultural Revolution, China's secondary education featured a dual-stream system: academic and vocational. The two streams merged during the Cultural Revolution, and vocational and technical schools were shut down for the first six years amid this turmoil (Tsang, 2000). The vocational and technical schools slowly re-emerged in the late 1970s and early 1980s, expanding rapidly after 1985, at which time the government announced structural reforms in education and gave a strong push to vocational education (Pepper, 1990). We thank one referee for making this point.

[8] Here is a concrete example. One county changed to a two-year junior high school system in 1968 and reverted to a three-year system in 1978. The two shifting years were 1968 and 1981 for senior high school. Person A entered junior high school in 1977 and therefore would enter senior high school in 1979 under a two-year regime, graduating in 1981. He received four years of middle school education. Person B entered junior high school in 1978, one year after person A did. Under a three-year regime, he would enter the senior high school in 1981 and spend three years there. The instrument employs a value of 4 for person A and a value of 6 for person B.

[9] The closest information in CFPS is the interviewee's residence county at the age of 12. However, researchers only have access to the province-level data for this variable.

[10] Within-county rural-urban migration barely affects our estimation because a vast majority of the counties in the sample (139 out of 142) had the same reform schedule for rural and urban schools.

Table 1
Summary statistics.

Sample	(1) All	(2) Main sample
Age	44.912	42.569
	(8.304)	(7.369)
Male	0.528	0.613
	(0.499)	(0.487)
Minority	0.040	0.042
	(0.197)	(0.201)
Junior High Graduate or Equivalent	0.456	
	(0.498)	
Senior High Graduate or Equivalent	0.450	0.735
	(0.498)	(0.442)
Some College or Above	0.091	0.265
	(0.287)	(0.442)
Years of Education	9.944	12.209
	(2.253)	(2.229)
Wage Income (Thousand Yuan)	2.324	2.617
	(2.165)	(1.992)
Total Income (Thousand Yuan)	2.481	2.795
	(2.696)	(2.839)
Observation	3181	785

Notes: Data from China Family Panel Study 2010. Standard deviation in the parentheses.

4. Empirical strategy

Our identification strategy exploits the county-level policy variations that cause the total duration of secondary education to be either four, five, or six years. More specifically, we proceed as follows. First, we start from a standard Mincerian regression and assume the logarithm of individual earnings to be a function of years of education, age, and other explanatory variables:

$$\ln Y_{cpi} = \alpha_0 + f(\text{Age}_{cpi}) + \alpha_1 \text{Edu}_{cpi} + \boldsymbol{\alpha}_2 \mathbf{X}_{cpi} + \Sigma_{p=1}^{P} \alpha_{p3} \text{County}_p + \varepsilon_{cpi}. \quad (1)$$

Here subscript c stands for a birth cohort and subscript p stands for a county. $\ln Y_{cpi}$ represents the natural logarithm of individual earnings. $f(\text{Age}_{cpi})$ is a function of age. We use a quadratic form together with a set of five-year age dummies. Edu_{cpi} is the years of completed schooling. $\mathbf{X}_{cpi}$ is a vector of control variables. The control variables include gender, minority, and parental background (education, age at interviewee's birth, and political status). County_p are the county dummies. α_1 is the return to an extra year of schooling, which is of primary interest in this equation. However, it is well known that the estimate of α_1 in Eq. (1) is subject to endogeneity bias, which may originate from numerous aspects, including but not restricted to measurement error, unobservable family background, inherent ability, and personality. The bias can be either upward or downward depending on its source.

To overcome the possible endogeneity of education, we use the changes in the duration of secondary education as an exogenous variation. An appropriate instrumental variable should be correlated with years of education but should not affect personal earnings through channels other than education. We focus on senior high graduates or above in our instrumental variable estimates. The first stage proceeds as follows:

$$\text{Edu}_{cpi} = \beta_0 + g_1(\text{Age}_{cpi}) + \beta_1 \text{Edu\_Policy}_{cp} + \boldsymbol{\beta}_2 \mathbf{X}_{cpi} + \Sigma_{p=1}^{P} \beta_{p3} \text{County}_p + \varepsilon_{cpi}. \quad (2)$$

Edu_Policy_{cp} is the default middle school years of education generated from local policies, which is defined in the previous section. Here β_1 captures the effect of middle school duration on the actual years of education. Our hypothesis is that $0 < \beta_1 < 1$. If the reforms in all counties were completed immediately and there were no enforcement issues, β_1 should equal to one. In practice, however, the coefficient should fall below one because reforms may proceed gradually and not all schools strictly follow the local policy.

Correspondingly, we set the second-stage estimation as follows:

$$\ln Y_{cpi} = \theta_0 + g_2(\text{Age}_{cpi}) + \theta_1 \widehat{\text{Edu}}_{cpi} + \boldsymbol{\theta}_2 \mathbf{X}_{cpi} + \Sigma_{p=1}^{P} \theta_{p3} \text{County}_p + \varepsilon_{cpi}, \quad (3)$$

where $\widehat{\text{Edu}}_{cpi}$ is the predicted years of education from the first stage. θ_3 is the instrumental variable estimates of the returns to education in urban China.

5. Estimation results

5.1. Main results

Table 2 reports the main result of this paper. Panel A reports the ordinary least square estimates. Panel B and Panel C report the first and second stages of the instrumental variable estimates, respectively. Our OLS estimates show that the returns to education are

Table 2
Least square regression and instrumental variable regression of returns to education.

	(1)	(2)	(3)	(4)
Dependent variable:	Log (Monthly wage)		Log (Monthly income)	
	Panel A: OLS estimates			
Years of Education	0.081***		0.086***	
	(0.011)		(0.011)	
Observations	758		785	
R-Squared	0.374		0.405	
	Panel B: IV Estimates - First stage			
Predicted (Junior + Senior) Years	0.599***		0.596***	
	(0.149)		(0.148)	
Predicted Senior Years		0.658**		0.702**
		(0.281)		(0.275)
Predicted Junior Years		0.550**		0.506**
		(0.249)		(0.246)
Observations	758	758	785	785
R-Squared	0.418	0.418	0.410	0.410
F-value of Instruments	16.17	8.098	16.23	8.207
	Panel C: IV Estimates - Second stage			
Years of Education	0.127*	0.130*	0.127*	0.136*
	(0.077)	(0.077)	(0.071)	(0.073)
Observations	758	758	785	785
P-value of overidentification test		0.636		0.350

Note: * significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors are in parentheses. Other control variables include: gender, minority dummy, age, age squared, 5-year-age group dummies, county dummies and parental background.

8.1% for wage income and 8.6% for total income. Our estimates are close to other papers' estimation of returns to education in China after 2000.

Panel B of Table 2 reports the first stage results, which show how schooling reform affects average years of education. If the policy-generated duration of middle school education is increased by one year, the actual years of education are raised by about 0.6 years, as suggested in columns 1 and 3.[11] The effect is statistically significant, with F statistics being over 16. Columns 2 and 4 separate the duration into that of junior high level and senior high level education. Senior high school reforms have larger effects on years of education than do junior high education reforms.

Panel C reports the instrumental variable estimates of the returns to education in urban China. IV estimation shows an estimate of 12.7% for both labor income and total income. In urban China, wage income is the dominant source of income, as Table 1 suggests. Therefore, we expect that the estimated returns to total income are close to those of wage income. IV estimates using changes in middle school education scheme as exogenous variable are higher than our OLS estimates and lower than the IV estimates using China's Compulsory Education Law (Fang et al., 2012). The overidentification tests in columns 2 and 4 suggest that the returns of a third year in junior high school is comparable to those of a third year in senior high school.

Our IV estimation is larger than the OLS estimation. At least three possibilities can explain the gap. The first possibility is the measurement error. In a twin study in the United States, Ashenfelter and Krueger (1994) estimate that the measurement errors account for 17%–26% of the variation in education. The magnitude of the measurement errors can be even larger in China because the education system has been going through rapid changes. People have difficulty recalling their exact years of schooling. The second possibility is a selection effect. An important share of our sample graduated shortly after China's Reform and Opening-Up Policy. It is possible that people with greater business talent are more likely to leave school early and set up their own businesses. The selection would bias the OLS estimates downward. A final possibility is local average treatment effect. Because the natural experiment we exploit is the additional third year of junior and senior high school. It is possible that the returns of that specific year are higher than those of the other years. In China, most students go to college through the college entrance exam (*gao kao*). Preparing for the exam is one of the major tasks for middle school students. When senior high education is extended to three years, students have more time to prepare for the exam and therefore are more likely to gain admission to college.

Note that our estimates of returns to education do not separately identify human capital effects (Becker, 1994) and signaling effects (Spence, 1973). On one hand, one more year of middle school education can teach students more knowledge and raise their human capital. On the other hand, longer duration also implies greater costs for attending middle school. This may alter students' decision to attend school and therefore change the ability pool at each stage of education. Our approach identifies the aggregate effects when middle school education years switch between a three-year system and a two-year system. Identifying whether the effects come from human capital or signaling requires further study.

[11] This number is slightly different between columns 1 and 3 because column 3 contains 27 more observations, who have zero wage but positive income.

5.2. Exogeneity of the reform

Our identification strategy relies on the assumption that the timing of the schooling reform is exogenous. Is it possible that each county endogenously determined the timing of the reform? The reduction of middle school durations from three years to two years was a result of the Cultural Revolution and therefore was not determined by the county government. However, each county set its own timetable for restoration of the three-year system. If counties with better fiscal conditions were more capable of early restoration,[12] the increase in years of schooling would have occurred sooner regardless of the reform.

If some unobserved local conditions were at play, we expect that those conditions would apply similarly to junior high and senior high schools and would be persistent across time. Therefore, we carried out two sets of placebo tests to address concerns regarding the endogenous timing of the policy changes. The first set tested whether a change in the duration of senior high school education would affect the years of education of those who do not enter senior high school. The second set of placebo tests moved the reform year back and forth by several years. If some county-level characteristics endogenously determined the timing of the reform, those characteristics were likely to be persistent over time. As a result, the effects should still show up in the placebo tests. Table 3 reports the results of placebo tests. Column 1 confirms that the sample who did not enter senior high school are not affected by the changes in the duration of senior high education. Columns 2 throughout 5 show that the first stage did not show any sign of statistical significance if we change the timing of the reform.

5.3. Common-trend assumption

Typical studies exploiting the geographic variations in the timing of the reform, including ours, assume common geographic trends in the factors affecting different cohorts. Stephens and Yang (2014) have re-examined the compulsory education law in the United States and point out that the common-trend assumption may not hold in the United States because of differential changes across states during this period.[13] Stephens and Yang (2014) find that the sign of instrumental variable estimate switches from positive to negative after adding interactions with regional dummies. The same concern applies to China. It is well known that after the Reform and Opening-Up Policy, coastal areas have developed at faster rates than inland areas. Therefore, the question is whether this geographical variation in development affects our estimation.

We have investigated three types of possible heterogeneous trends. First, not all counties began to restore the three-year system at the same time. Counties that were more eager to raise the residents' education level may have initiated the process earlier and entered a faster track of increasing average schooling years. Second, school reforms take time. Not all counties could restore three-year middle school at one time. In our CFPS sample, 142 counties have information on the year in which they started to restore three-year senior high school. Forty of them explicitly record the ending year. It took most counties two to four years to complete the process, with an average length of 3.1 years. Much less information is available for the restoration of three-year junior high school.[14] Possibly, counties that could complete the process in a short period were in better fiscal condition. These counties may also have managed to restore their education system sooner after the end of the Cultural Revolution. Finally, China is a large country with wide geographic variations. After the 1978 Reform and Opening-Up Policy, the coastal area experienced more rapid development than did the middle and western areas. The differential trends across regions may have resulted in estimation bias, as in a similar case in the United States (Stephens and Yang, 2014).

To examine the possible effects of the heterogeneous trends described above, we divided counties accordingly and interacted with cohort linear trends. As for heterogeneity in three-year middle school restoration years, we divided the sample by whether the reform took place before or after 1982 (66.07% versus 33.93%) for senior high school and before or after 1979 (84.32% versus 15.68%) for junior high school. As for the heterogeneity in the duration of reform, we divided the sample into three categories: counties with no reform ending year information, counties in which reform took three years or less, and counties in which reform took longer than three years (75.81%, 16.80%, and 7.39% for senior high school; 91.94%, 6.83%, and 1.23% for junior high school). As for geographic heterogeneity, we divided the sample into: eastern (46.25%), middle (38.52%), and western (15.23%). Table 4 reports the instrumental variable estimation, in which we added into the regression the types of counties that interacted with cohort trends. Additionally controlling for these variables barely affects our estimation. After controlling for various possible heterogeneous trends, we find that the IV estimates of the returns to education vary between 12.9% and 14.2%, which are close to our baseline estimation in Table 2.

6. Discussion

This section is devoted to answering two questions. First, are other factors associated with the changes of middle-school duration that have direct impact on people's earning ability? This question is related to the exclusion restriction of the instrumental variable—the policy changes should affect income through and only through years of education. We explore three possible confounders: the Cultural Revolution, household budget, and quality of education. The second question is: what are the channels of the returns to education?

[12] Because most Chinese middle schools are public, supporting an extra year of schooling can be costly for the local government.

[13] For example, previous research finds evidence that the school quality in the southern United States experienced more dramatic improvement than in the northern United States (Card and Krueger, 1992; Bleakley, 2007; Aaronson and Mazumder, 2011).

[14] Only 14 out of 135 counties officially recorded when the restoration ended. The average length was 2.5 years.

Table 3
Placebo tests (Dependent variable: Years of education).

	(1)	(2)	(3)	(4)	(5)
Education group	Below senior high	Senior high graduates or above			
Reform year		Move back 5 years		Move forward 5 years	
Predicted (Junior + Senior) Years		0.204		0.031	
		(0.186)		(0.142)	
Predicted Senior Years	0.078		0.039		0.17
	(0.129)		(0.343)		(0.244)
Predicted Junior Years			0.363		-0.096
			(0.292)		(0.204)
Observations	810	785	785	785	785
R-Squared	0.311	0.396	0.397	0.396	0.396

Note: * significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors are in parentheses. Other control variables include: gender, minority dummy, age, age squared, 5-year-age group dummies, county dummies and parental background.

Table 4
Robustness check of heterogeneous cohort trends.

	(1)	(2)	(3)	(4)
	Panel A: IV Estimates - First stage			
Predicted (Junior + Senior) Years	0.587***	0.535***	0.621***	0.554***
	(0.147)	(0.148)	(0.149)	(0.148)
Observations	785	785	785	785
R-Squared	0.411	0.417	0.424	0.433
First Stage F-value	15.95	13.10	17.31	13.92
	Panel B: IV Estimates - Second stage (Wage)			
Years of Education	0.134*	0.139	0.129*	0.142*
	(0.075)	(0.087)	(0.074)	(0.082)
Observations	758	758	758	758
	Panel C: IV Estimates - Second stage (Income)			
Years of Education	0.127*	0.137*	0.133*	0.132*
	(0.072)	(0.081)	(0.069)	(0.077)
Observations	785	785	785	785
Late Reform Dummy × Trend	X			X
Reform Duration Dummies × Trend		X		X
Regional Dummies × Trend			X	X

Note: * significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors are in parentheses. Other control variables include: gender, minority dummy, age, age squared, 5-year-age group dummies, county dummies and parental background. a. Late reform is defined as the cities restored three-year senior high school after 1981 (1978 for junior high school). b. Reform duration dummies include a dummy variable indicating the duration information is missing and a dummy variable indicating the reform took more than three years. c. Regional dummies include three dummies including eastern, middle and western.

6.1. Influence of the cultural revolution

As the background section describes, China's historical changes to the duration of middle school are closely related to the Cultural Revolution, which may affect people's education in many ways. For example, roughly 17.7 million urban youths were resettled to rural areas from 1967 to 1978 (Gu, 2009). This event is known as the "sent-down-youth movement" or "going up to the mountains and down to the villages" (*shangshan xiaxiang*). Those urban youths went to the country-side and were "re-educated by the poor farmers"[15] instead of pursuing further education. The Cultural Revolution affected not only the number of students educated, but also the education that they received. At the beginning of the Cultural Revolution, teachers were not allowed to follow the standard curriculum, and students were expected to learn farming and manual labor. The standard curriculum was gradually resumed after 1972 (Meng and Gregory, 2002). One natural question is—could Cultural Revolution (1966–1976) contaminate our results?

Note that our identification exploits not only temporal variation but also spatial variation. The years of transitioning between two- and three-year systems differed across counties. Unless those changes were systematic with regard to the local intensity of the Cultural Revolution, our identification remains valid. To further assure our identification's robustness in relation to the Cultural Revolution, we tried two different approaches, as Table 5 shows. The first column additionally controls for the overlapping years between individuals' middle school ages (12–18) and the decade of the Cultural Revolution (1966–1976). The second column excludes from the sample the group most affected by the Cultural Revolution—the group of sent-down-youth. The estimated returns

[15] "It is very necessary for the urban educated youth to go to the countryside to be re-educated by the poor farmers!", said Chairman Mao in the December 12, 1968, issue of *People's Daily*.

Table 5
Examine possible influence from the cultural revolution.

	(1)	(2)
Panel A: IV Estimates - First stage		
Predicted (Junior + Senior) Years	0.571***	0.643***
	(0.151)	(0.154)
Observations	785	759
R-Squared	0.411	0.408
First Stage F-value	14.36	17.45
Panel B: IV Estimates - Second stage (Wage)		
Years of Education	0.139*	0.140*
	(0.082)	(0.077)
Observations	758	733
Panel C: IV Estimates - Second stage (Income)		
Years of Education	0.112	0.105
	(0.075)	(0.067)
Observations	785	759
Controlling for Exposure to 1966 – 1976 Period	X	
Drop Sent-Down-Youth Sample		X

Note: * significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors are in parentheses. Other control variables include: gender, minority dummy, age, age squared, 5-year-age group dummies, county dummies and parental background. a. "Exposure to 1966 – 1976 Period" refers to the personal overlap years between middle-school ages (age 12 – 18) and the Cultural Revolution period (year 1966 – 1976). b. "Sent-Down-Youth Sample" refers to the sample who self-report to be sent down during the movement.

vary from 10.5% to 14.0%, which are not far away from our baseline estimation in Table 2.

6.2. Affordability of another year of schooling

How does an extra year of required middle school education affect household decisions? When middle school duration was restored to three years, the extra year of education was not "free." From the households' perspective, the extra year involved both direct schooling costs (for example, tuition) and indirect opportunity costs, including the forgone income that the students could have earned if they had entered the labor market instead.

Although we cannot observe the tuition and household income in the time when people in our sample were attending middle school, we find suggestive evidence that costs of attending middle school has not been a major concern. The dropout rates are extremely low in urban China. We observe no junior high school dropouts and few senior high school dropouts in our data. Among the 785 students who have attended senior high school, 751 graduated with a diploma, suggesting that the dropout rate was at most 4.3%. Moreover, if staying in middle school for one more year became a burden for the household, we expect that family background would have had important impacts on our estimation because students in families with higher socioeconomic status would have been less affected by the extra costs. In Table 6, we re-ran the estimations without controlling for any parental information. By comparing the results in Table 6 with those in Table 2, we find that information about the family background barely affected the IV estimation.

6.3. Quality of education

Both issues mentioned above raise the concern of the quality of education. It is possible that the education reform has not simply prolonged middle school education by one year. Rather, it could also have changed the quality of school through changes in textbooks and curriculums. Moreover, extending the duration was not easy from the schools' perspective. One more grade of students was projected to increase the schools' size by about 50%. This would require significantly more teachers and school premises. If schools were to fail to accommodate these changes properly, it could lead to a reduction in the quality of education.

Admittedly, measuring schooling quality is a challenging task. Although we have no direct information on the quality of education, we have tried an alternative approach. We estimate using the following equation:

$$\ln Y_{cpi} = \theta_0 + g_2(\text{Age}_{cpi}) + \theta_1 \text{Edu}_{cpi} + \theta_2 \text{Edu\_Policy}_{cp} + \boldsymbol{\theta}_3 \mathbf{X}_{cpi} + \Sigma_{p=1}^{P} \theta_{p4} \text{County}_p + \varepsilon_{cpi}. \quad (4)$$

In short, we add the instrumental variable to the Mincerian equation. The equation tests whether the changes in policy have direct effects on the earnings through mechanisms other than years of education. If the policy changes also affect the quality of education, which is supposed to have an important effect on income, the estimator θ_2 should remain significant even if we control for the quantity of education. Table 7 reports the results. We find that if we control for years of education, the coefficients of the policy indicators remain small and statistically insignificant. To conclude, we find no evidence that the changes in the education scheme had a direct impact on personal earnings through channels other than years of education.

Table 6
OLS and IV regression of returns to education without controls for parental background.

Dependent variable:	(1) Log (monthly wage)	(2)	(3) Log (monthly income)	(4)
	Panel A: IV Estimates - First stage			
Predicted (Junior + Senior) Years	0.612***		0.605***	
	(0.148)		(0.146)	
Predicted Senior Years		0.640**		0.689**
		(0.277)		(0.271)
Predicted Junior Years		0.588**		0.534**
		(0.249)		(0.246)
Observations	758	758	785	785
R-Squared	0.414	0.414	0.408	0.408
F-value of Instruments	17.15	8.569	17.19	8.653
	Panel B: IV Estimates - Second stage			
Years of Education	0.136*	0.138*	0.133*	0.142**
	(0.075)	(0.076)	(0.070)	(0.072)
Observations	758	758	785	785
P-value of overidentification test		0.550		0.317

Note: * significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors are in parentheses. Other control variables include: gender, minority dummy, age, age squared, 5-year-age group dummies and county dummies.

Table 7
Does the reform affect the quality of education?.

Dependent variable:	(1) Log (monthly wage)	(2) Log (monthly income)
Years of Education	0.080***	0.085***
	(0.012)	(0.011)
Predicted (Junior + Senior) Years	0.028	0.025
	(0.050)	(0.048)
Observations	758	785
R-Squared	0.375	0.405

Note: * significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors are in parentheses. Other control variables include: gender, minority dummy, age, age squared, 5-year-age group dummies, county dummies and parental background.

6.4. Mechanism

We have so far provided evidence that increased years of schooling have a causal impact on one's earnings. However, the mechanism through which education affects income remains unclear. In this subsection, we discuss four possible channels: work in better institutions and higher-paid industries, higher chances of attending college and study in better majors, improved cognitive ability, and health status.

First, higher education improves the prospect of finding a better job in a better industry. For instance, annual wage income in a foreign-owned enterprise is about three times as high as that in a collective enterprise (55,919 RMB versus 18,455 RMB). Wage income in the financial sector is about ten times the amount in the agricultural sector (47,528 RMB versus 4,969 RMB). We control for institution dummies and industry dummies to probe this channel. Institution dummies include 13 dummy variables indicating the type of the institutions in which people work (e.g., state-owned enterprise, private firms). Industry dummies include 19 dummy variables that indicate the industries in which people work (e.g., mining, transportation).

Second, more years of schooling can improve students' performance on the college entrance exam and increase the probabilities of college admission. They can also choose better majors in college. This channel is especially important in China because the third year of senior high school is typically devoted to preparing for the exam. The first column of Table 8 reveals that people who spend a longer time in middle schools are more likely to go to college. To understand the importance of this channel, we control for a dummy variable indicating whether people attend college or university and 11 dummies indicating the majors people study in college (economics, history, and law, for example).

Third, education can improve people's cognitive ability. Existing studies have provided evidence that more years of schooling causally improve the cognitive abilities measured in various aspects (Falch and Sandgren Massih, 2011; Banks and Mazzonna, 2012; Carlsson et al., 2015). The 2010 wave of CFPS data include two sets of questions testing people's cognitive abilities. The first set is the word recognition test. Interviewees are shown 34 words from simple to difficult and are asked to pronounce the words correctly. The

Table 8
Possible benefits of an additional year of schooling.

	(1)	(2)	(3)	(4)	(5)
		Senior high & above		Senior high only	
Dependent variable:	Normal college	Word test	Math test	Word test	Math test
Predicted (Junior + Senior) Years	0.070**	0.519*	0.072	0.587*	0.117
	(0.032)	(0.284)	(0.124)	(0.332)	(0.123)
Observations	785	785	785	577	577

Note: * significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors are in parentheses. Other control variables include: gender, minority dummy, age, age squared, 5-year-age group dummies, county dummies and parental background.

Table 9
Possible mechanisms of returns to education.

	Panel A: IV Estimates - Second stage (Wage)					
Years of Education	0.127*	0.116	0.059	0.099	0.127*	0.075
	(0.076)	(0.078)	(0.101)	(0.079)	(0.077)	(0.106)
Observations	758	745	758	758	758	745
	Panel B: IV Estimates - Second stage (Income)					
Years of Education	0.127*	0.136*	0.059	0.099	0.136*	0.107
	(0.072)	(0.079)	(0.097)	(0.076)	(0.074)	(0.111)
Observations	785	772	785	785	785	772
Job Institutions and Industries		X				X
College & University Majors			X			X
Math & Word Tests				X		X
Health Status Indicators					X	X

Note: * significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors are in parentheses. Other control variables include: gender, minority dummy, age, age squared, 5-year-age group dummies, county dummies and parental background.

second set is the mathematical test. Interviewees need to solve 24 math questions from simple to difficult.[16] Column 2 of Table 8 provides suggestive evidence that one more year of middle school duration increases people's word test score by 0.519 question, or about one third of the standard deviation. Such an effect cannot be explained by a higher chance of going to college. Column 4 shows that the positive effect on word test scores remains even among people who do not go to a university.

Finally, empirical research also finds that education may lead to a better health (Cutler and Lleras-Muney, 2006; Grossman, 2006; Oreopoulos and Salvanes, 2011) and which, in turn, can result in higher income (Strauss and Thomas, 1998; Smith, 1999). To understand the role of health status, we control dummy variables indicating self-rated health (from 1 = very healthy to 5 = very unhealthy), variables indicating whether people feel uncomfortable in the past two weeks, whether a patient has been diagnosed with a chronic disease in the past six months, and a given person has undergone hospitalization in the past year.

Table 9 reveals the relative importance of each mechanism. The results suggest that returns to education can be explained mostly by selecting a better college major in college and obtaining a better job afterward. If we control for college majors, the coefficient falls from 0.127 to 0.059. The second important factor is cognitive abilities. The coefficients would fall from 0.127 to 0.099 if the regression includes variables representing cognitive abilities. Job industries and health status, in contrast, do not play important roles in explaining returns to education.

7. Conclusion

In this study, we empirically estimate the returns to education in urban China using the changes in the duration of middle school education as exogenous shocks. The duration was reduced from three years to two years in the late 1960s and have been gradually restored to three years since the late 1970s. We found the exact years of the policy changes of 142 counties in the CFPS sample from local gazetteers. We exploited both time variation and geographical variation. We estimated that a one-year increase in the duration of middle school education as a result of policy changes has increased actual years of education by about 0.6 years.

There are three important takeaways from this study. The first takeaway is that the estimated returns to education in urban China are 12.7% for both wage income and for total income. The second takeaway is that researchers should be cautious when inferring years of education from the highest level of education. Many influential surveys in China, such as the population census and the

[16] The math problems include the following: addition, subtraction, multiplication, division, exponent, logarithm, trigonometric, permutation, and combination.

Urban Household Survey, only ask about the highest level of education rather than about total years of education. When estimating returns to education using those surveys, researchers often assign a specific value of years to each degree.[17] Our study highlights such an approach can be problematic, especially if the sample of interest received their education from the late 1960s to the early 1980s. This is because the duration of education in each stage varies across time and regions. The final takeaway is that we present a new source of exogenous increase in education from the supply side. Previous studies exploited the compulsory education law (Harmon and Walker, 1995; Oreopoulos, 2006; Devereux and Hart, 2010; Fang et al., 2012), school construction programs (Duflo, 2001), and college expansion (Walker and Zhu, 2008; Devereux and Fan 2011). Because the IV approach estimates the local average treatment effect (LATE), different exogenous shocks identify the educational returns in different levels for different populations. The schooling reform in our paper targets China's more highly educated urban population. This strategy can also be applied to estimating non-pecuniary returns, such as health, cognitive abilities, and non-cognitive skills.[18]

It is noteworthy that our paper does not take into account the general equilibrium effect and the spillover effect. Understanding the general equilibrium effect of an extra year of education can be complicated. An extra year may affect the supply side because the cost of attending school becomes higher. As a result, it may reduce the supply of senior high graduates. On the demand side, employers will realize that people with senior high school diploma are better educated than before. The effect of general equilibrium requires further research. As for the spillover effect, previous research (Acemoglu and Angrist, 2001, for example) suggests that the externality of human capital is likely to be positive. If that is the case, people who receive a two-year middle school education can also benefit from the extension of middle school duration. Therefore, our estimation provides a lower-bound estimate with the existence of spillovers.

Appendix A. Possible measurement errors in years of education and the instrumental variable

This appendix discusses several issues in defining our key endogenous variable (years of education) and instrumental variable (policy-indicated years of middle school education).

A1. Measurement errors in years of education: Grade repetition

If a student failed in the previous year, he or she may be asked to repeat the same grade. In the data, we only know how many years a person spent in each stage of education; we do not know how many years may have been repeated. Inferring grade repetition from years spent in school is also problematic. The default length of schooling varies across counties and years according to local policies. For example, a third year of middle school indicates grade repetition in the two-year regime, but not in the three-year regime.

Although we cannot distinguish grade repetition from standard years of education, we claim that grade repetition does not affect our IV estimation. The reason is simple: grade repetition is essentially the measurement error in the independent variable—a problem that will be resolved by using an instrumental variable. Nevertheless, in Table A1, we re-run our main regressions using capped years of education instead—primary education terms are capped at six years, junior high and senior high education are capped at three years, etc. By comparing Table A1 with Table 2, we note that the OLS returns in Table A1 are slightly larger than those in Table 2 (0.089 versus 0.081); this should be expected, because capped years of education have arguably smaller measurement errors, but the IV results remain almost identical (0.128 versus 0.127). In short, grade repetition is unlikely to affect our main results.

Table A1
Least square regression and instrumental variable regression of returns to education (using capped years of education instead).

	(1)	(2)	(3)	(4)
Dependent variable:	Log (monthly wage)		Log (monthly income)	
	Panel A: OLS estimates			
Years of Education	0.089***		0.088***	
	(0.011)		(0.011)	
Observations	758		785	
R-Squared	0.378		0.404	
	Panel B: IV Estimates - First stage			
Predicted (Junior + Senior) Years	0.593***		0.590***	
	(0.143)		(0.143)	
Predicted Senior Years		0.662**		0.694***
		(0.258)		(0.253)
Predicted Junior Years		0.534**		0.502**
		(0.226)		(0.223)

(*continued on next page*)

[17] Generally, people who only finish primary education are supposed to receive six years of education and people with a junior high degree are expected to receive nine.

[18] In China's context, Huang (2015) estimates the causal impact of education on health and Chen et al. (2018) estimate the non-pecuniary returns of education in non-cognitive skills. Both studies use China's Compulsory Education Law as an exogenous shock and therefore implicitly target the rural population.

Table A1 (*continued*)

Dependent variable:	(1) Log (monthly wage)	(2)	(3) Log (monthly income)	(4)
Observations	758	758	785	785
R-Squared	0.413	0.414	0.405	0.405
F-value of Instruments	17.25	8.643	17.03	8.591
	Panel C: IV Estimates - Second stage			
Years of Education	0.128*	0.131*	0.128*	0.137*
	(0.077)	(0.078)	(0.072)	(0.073)
Observations	758	758	785	785
P-value of overidentification test		0.648		0.349

Note: * significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors are in parentheses. Other control variables include: gender, minority dummy, age, age squared, 5-year-age group dummies, county dummies and parental background.

A2. Measurement errors in the instrumental variable: Cross-county migration

Because of data limitations, we use the policy history information in the current residence county (denote as Z) instead of the more ideal information in the education-receiving county (denote as Z^*). Cross-county migration essentially generates measurement errors in the instrumental variable $\epsilon = Z - Z^*$. Econometrically speaking, the existence of ϵ does not invalidate the IV approach—the validity of instrumental variable rests on the relevance restriction (Z should be correlated with the endogenous variable) and the exclusion restriction (Z does not affect the outcome variable through the error term in the second stage). The validity does not involve precise measurement of the instrumental variable. Our first-stage results in Table 2 prove the relevance restriction under the existence of measurement errors in Z. Therefore, whether cross-county migration invalidates our IV approach depends on the exclusion restriction—does the measurement error ($Z - Z^*$) directly affect the outcome variable? In the context of our study, this question can be asked in another way—are years of policy changes systematically related with local characteristics that directly affect earnings?

Because the sample in CFPS is too small to give convincing county-level estimates, we compute the county characteristics using a 20% sample of the 2005 1% inter-decennial population census (henceforth mini-census 2005) and match it to the CFPS data. The reason we employ mini-census 2005 is that it is the only census that contains income measures, to the best of our knowledge. We first compute a set of statistics representing a county's economic and demographic situation: its share of minority/urban/migrant/young (age 0–19)/ old (age > 60) population. Then we compute a set of education and income measures for the population that corresponds to our main CFPS sample—the urban population age 30 to 60, excluding students and retirees.[19] The measures include: the share of college/senior high/junior high graduates, the share of the working population, and average income last month. Table A2 estimates whether the years of policy changes (3-to-2 change; 2-to-3 change; years in the two-year system) are associated with the 10 county characteristics described above.[20] Among 60 coefficients presented in Table A2, 8.33% (5 out of 60) show statistical significance at 10%, and we see no systematic patterns among these. Moreover, the tests of joint significance of those 10 variables yield large p-values, the maximum being 0.425. In summary, we find no evidence that the timing of the education reform is related with county-level characteristics.

Table A2
Regressing policy variables on county-level characteristics.

	(1) Junior high	(2)	(3)	(4) Senior high	(5)	(6)
Dependent variable:	Year of 3-to-2 change	Year of 3-to-2 change	Years in 2-year system	Year of 3-to-2 change	Year of 3-to-2 change	Years in 2-year system
Share of Minority	-1.602	0.557	2.698	-1.906	1.437	3.344*
	(1.920)	(2.440)	(3.204)	(1.227)	(1.151)	(1.868)
Share of Urban Hukou	-0.265	-0.587	-0.393	0.174	0.006	0.175
	(1.007)	(1.015)	(1.466)	(1.063)	(0.740)	(1.398)
Share of Migrants	3.357	0.445	-3.444	-3.590	1.357	4.519
	(2.392)	(2.662)	(4.551)	(3.252)	(2.348)	(2.905)
Share of Age 0–19	-0.727	0.633	0.706	-8.577*	1.906	12.454*
	(3.757)	(4.974)	(7.361)	(4.804)	(4.469)	(6.368)
Share of Age > 60	0.147	0.509	-0.451	-14.554	-8.373	6.883

(*continued on next page*)

[19] The sample of mini-census 2005 has an average of 455 observations per county under this restriction.

[20] The number of counties in Table A2 are smaller than our main regression because we are unable to match some counties from mini-census 2005 to CFPS 2010.

Table A2 (*continued*)

Dependent variable:	(1) Junior high Year of 3-to-2 change	(2) Year of 3-to-2 change	(3) Years in 2-year system	(4) Senior high Year of 3-to-2 change	(5) Year of 3-to-2 change	(6) Years in 2-year system
	(5.694)	(7.273)	(10.885)	(9.094)	(7.530)	(8.332)
Share of College or Above	-1.860	-1.438	-0.235	-1.264	0.029	1.823
(Main Sample)	(2.739)	(2.588)	(3.649)	(2.695)	(1.820)	(4.192)
Share of Senior High	-2.842	0.513	1.286	-0.534	-0.518	0.385
(Main Sample)	(3.012)	(2.372)	(4.260)	(3.349)	(1.903)	(4.858)
Share of Junior High	-4.165*	-0.144	2.552	-2.105	-0.358	2.462
(Main Sample)	(2.374)	(2.771)	(3.713)	(2.687)	(1.781)	(4.247)
Share of Working	0.028	2.326	2.824	1.803	-0.181	-2.227
(Main Sample)	(1.715)	(2.117)	(2.809)	(1.932)	(1.684)	(3.042)
Average Income, Thousand Yuan	-1.924	-0.395	1.953	-2.123*	-0.410	1.847
(Main Sample)	(1.289)	(1.110)	(2.055)	(1.215)	(0.586)	(1.434)
Observations	115	123	106	117	130	112
R-Squared	0.255	0.378	0.401	0.297	0.263	0.297
Test of Joint Significance (*p*-value)	0.481	0.625	0.743	0.515	0.705	0.425

Note: * significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors are in parentheses. Province dummies are controlled for.

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