Understanding the Effects of Education on Health: Evidence from China*

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Abstract

Using temporal and geographical variations in the implementation of Compulsory Schooling Laws in China, I find one additional year of schooling reduces the rates of reported fair or poor health, underweight and smoking by 2.1, 1.0 and 1.5 percentage points, respectively, and increases cognition by 0.1-0.15 standard deviation. Investigation on potential mechanisms finds that cognition on average explains 15% of the effects on self-reported health, income 7% and smoking almost zero. Furthermore, cognition explains up to 25% for the women in higher education regions while income up to 18% for men in lower education regions. These findings provide new evidence for the effects of education on health and help to reconcile the mixed findings in the literature. (*JEL* classification: I12, I21, I28)

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I. Introduction

The causal effects of education on health are of central interests among the economists. They are crucial parameters in the classical theoretical models of demand for health capital (Grossman, 1972) and the influences of childhood development on adult outcomes (Heckman, 2007, 2010; Conti et al., 2010). Moreover, quantifying to what extent that education causally impacts on health is essential to the formation and evaluation of education and health policies. This is meaningful especially in comparison to high cost of health insurance and rising healthcare spending with the uncertain or little return (Weinstein and Skinner, 2010).

However, the empirical findings on causality are mixed. For example, Lleras-Muney (2005) used state-level changes in Compulsory Schooling Laws (CSLs) in the United States as instruments for education and identified large effects of education on mortality;[1] in contrast, Clark and Roayer (2013) used two education policy reforms in the UK and found no impact on mortality. Some mixed findings are even found within the same country.[2] The inconsistent findings in the literature reflect scarce evidence on the mechanisms, which is largely due to data limitation. Since most CSLs reforms in industrial countries usually happened early and the changes were small in general, the affected cohorts were really old when surveys took place and the policies only induced small increase in education. For example, CSLs changes in Lleras-Muney (2005) happened between 1914 and 1939 and most of the states only increase or decrease smaller than two years,[3] and the two reforms in Clark and Roayer (2013) happened in 1947 and 1972 both increased the school leaving age by only one year.

Using a national representative sample from three large individual level datasets from China, this paper investigates the causal effects of education on health and explores the

[1] Identification of this effect is achieved by exploiting variation in the timing of the law changes across states over time such that different birth cohorts within each state have different compulsory schooling requirements.

[2] Fletcher (2015)revisited the case for the US and did not find evidence for causality on mortality.

[3] See the Appendix of Lleras-Muney (2005). This could be a reason why the results are not robust when state-specific time trends are added since they may absorb most of the variations.

possible mechanisms in between.[4] The unprecedented nationwide education reform initiated in 1986 made nine-year schooling (i.e. the junior high school) compulsory and sixteen the minimum school leaving age, for all the regions in the largest developing country. This education reform got great achievements: the enrollment rate for junior high school increased by 26 percentage points from 69.5% in 1986 to 95.5% in 2000, and the number of students enrolled in junior high school increased by 8.9 million.

Following the previous literature (Lleras-Muney, 2002, 2005), I first exploit the variation in the different timing of policy adoption across the provinces. Because the central government allowed the provincial governments to implement the policy separately, I construct a CSLs-eligibility indicator for the birth cohorts in the corresponding provinces. Since the timing variation across provinces is small (the gap between the earliest and latest provinces is only 5 years), I further explore the cross-sectional variation in the education's potential increase across the regions. Because all the provincial governments were required to enforce the "nine-year" compulsory schooling laws, I hypothesize that the education in the provinces with more people with less than nine-year schooling before the laws' enforcement should potentially increase more afterwards.[5] The estimates in the preferred econometric model provide sound evidence for it. Specifically, the CSLs significantly increased the schooling years by 1.1 on average; the effect is 1.6 years in the regions with lower education before (lower than median) but is 0.6 years for the rest. Consistent with the policy implementation, the effects of CSLs are also more pronounced for rural people and women.

Since the identification is based on the different timing of the laws enforcement and the heterogenous effects across regions, there are some concerns about the excludability of the constructed variables for the education reform. The first one is that the potential cohort trends across the provinces caused by other factors such as heterogeneous economic growth

[4] Since the surveys span from 1995 to 2012, I keep the 1955-1993 birth cohorts and aged between 18 and 50 at the survey to conduct this study.

[5] In practice, I calculate the proportion of individuals with fewer than 9 years schooling among the CSLs non-eligible cohorts in the local province (the mean value is 0.37 and the value ranges from 0.05 to 0.79 in the sample), and interact it with the CSL-eligibility in the regressions. Positive coefficients on this interaction provide evidence for this hypothesis.

may drive the estimation. I further control for province-specific birth cohort linear trends and it yields fairly consistent results. The second concern is that the constructed variables may pick up the effects of other reforms since China implemented a couple of policies during that period. However, exactly consistent with the "nine-year" compulsory schooling, the results show that the effects of CSLs on education only exist *if and only if* schooling years is below or equal to nine. Third, the associations of CSLs with education may reflect the "regression to the mean" rather than the actual effects because regions with lower education may increase more simply due to lower marginal cost. I conduct a placebo test for the CSL-ineligible cohorts and find no evidence for this. Finally, the more increase in education in the regions probably reflects the larger nutrition improvement because these regions probably had poorer nutrition status in the beginning. But I find the policy has no effects on height, a measure for younger adulthood nutrition status.

The estimates from the reduced forms and the Two-Stage Least-Squares (2SLS) both find pronounced effects of education on health outcomes. The 2SLS estimates show that one additional schooling year significantly reduces reported fair or poor health rate by 2 percentage points, underweight rate by 1.2 points and smoking rate by 1.5 points, respectively; one additional year of schooling also increases 0.09 standard deviation in words recalling and 0.16 standard deviation in math calculation ability. The results are robust to different model specifications. Gender-specific analysis shows that the effects of education on self-reported health and cognition are not significantly different for men and women. But the effect on underweight are larger for women, and that on smoking is larger for men; one additional year of schooling decreases 1.8 percentage points in underweight rate for women but 0.1 for men, and 2.9 points in smoking for men but 0.2 for women.

Apart from the large impacts on education, another virtue of the CSLs in China is that it is the latest documented education reform such that the data used in this study collected more detailed individual information. To shed some light on the mixed findings on effects of education in the literature, this paper further quantitively identifies the possible mechanisms in effects of education. There are some candidates to examine. For example, income is usually used as an explanation for the impact of education on health since higher education predicts higher income that enables people to purchase commodities of higher quality. For another, education also increases people's cognition so that they are able to obtain more health knowledge and know how to take care of themselves better. Yet no evidence is provided to support these hypotheses.

Therefore, I examine a couple of potential mechanisms including income, cognition and smoking. Extending the econometric framework in Cutler and Lleras-Muney (2010), I use the exogenous variations in CSLs and quantify how much these intermediate variables explain the effects of education on self-reported health, respectively. The estimates find that the cognition on average explains 15 percent of the education's effect, income 7 percent and smoking almost zero. This finding suggests that to enable the people obtain more health knowledge is an important mechanism through which education improves health.

In addition, income and cognition have differential performance for different genders and in the regions with different education level prior to the reform. Specifically, cognition explains over 20 percent of the education's effect for women and only 12 percent for men. However, income explains 7-10 percent of the effect for men but only 1-3 percent for women. Finally, the estimates also suggest that the proportion that can be explained by cognition is stable across the regions with different the education levels. But income explains much more in the regions with lower education before the education reform. Specifically, income explains about 5-10 percent in the regions with lower education (lower than median) but only 3-5 percent in the rest places.

The findings in this paper contribute to several literatures. First, this paper contributes to the famous debate between Grossman and Fuchs (Grossman, 2006) and the mixed findings in the literature. The findings provide evidence for the effectiveness of education policies in improving education and health status, and build up the literature by studying causality between education and health under the developing country and working-age population setting. In addition, this study fills in the gap in the literature by examining the potential mechanisms why the education's effects on health for the first time, which helps to explain the large heterogeneity in impact of education on health across different nations and in different periods.

The findings also build up the literature on causality of education on other outcomes such as BMI and cognition. First, the findings highlight the effects of education under a developing country setting: education increases BMI in China because it reduces the underweight rate but has no effects on obesity, while the previous literature (e.g. Brunello et al., 2013) found negative effects of education on BMI because it mostly reduces the obesity rate.[6] Second, due to education policies took place much earlier than our realization about the importance of cognition, the evidence for causal effect of education on cognition is rare in literature. Our findings thus contributes to this literature on the causal effect of education on cognition, especially for the working-age population.[7]

II. Background and Data

As emphasized in previous literature, the OLS coefficients when directly regressing the health outcomes on schooling years cannot be interpreted as causality because of endogeneity. The endogeneity may originate from many aspects, including family background, unobservable inherent ability, habit and personality like patience, and even the reversal causality that those with longer life expectancy will invest more in education (Jayachandran and Lleras-Muney, 2009). Researchers investigating the causality have been insistently searching for exogenous variations in education by the Compulsory schooling laws (CSLs) (e.g. Lleras-Muney, 2005;

[6]The reason may be that the underweight is a more serious health problem in the developing countries like China while obesity matters more for the countries in those developed ones like Europe and US.

[7]Some investigated the casual impact of education on cognition for those aged people, i.e. Banks and Mazzonna (2012) for the UK and Huang and Zhou (2013) for China. Two exceptions areAaronson and Mazumder (2011) and Carlsson et al. (2012). The former found that the construction of Rosenwald schools had significant effects on the schooling attainment and cognitive test scores of rural Southern blacks and the latter found that 180 days extra schooling increased crystallized test scores by approximately 0.2 standard deviations among the 18-years-olds adolescents in high schools in Sweden. The findings in this paper provide consistent evidence to this. Another exception is which

Clark and Roayer, 2013) to derive the causal effects of education.

2.1 Compulsory Schooling Laws in China

Following this strand of literature, this paper first explores the variations from the CSLs changes in different provinces and different time in China and then uses the exogenous variations to derive the causal effects of education.

China's Compulsory Education Laws were passed on April 12, 1986 and officially went into effect on July 1, 1986. This was the first time that China used a formal law to specify educational policies for the entire country. This law had several important features (China Ministry of Education 1986): 1) nine years of schooling became compulsory; 2) children were generally supposed to start their compulsory education at 6 years of age in principle, 3) compulsory education was free of charge; 4) it became unlawful to employ children who are in their compulsory schooling years and 5) local governments were allowed to collect education taxes to finance compulsory education (Fang et al., 2012). Different from the US and European countries increasing one or two years in the compulsory schooling years, the laws in China actually use the uniform "9 years" for the length of years of compulsory schooling no matter where it is.

Local provinces were also allowed to have different effective dates for implementing the law because the central authorities recognized that not all provinces would be ready to enforce the law immediately. But the variation in the timing is not large because most of the provinces started in 1986 or 1987, and the gap between the earliest and latest provinces is only 5 years.

Therefore, I further explore the cross-sectional variations in the enforcement of the laws. The central government planed to have different implement forces across different regions because of large inequality in education levels across regions, and thus it decided to mainly support the less-developed regions.[8] A government document "Decisions about the Education

[8]The central government divided the whole nation into three categories: 1) cities and developed regions (cities and some towns); 2) middle-level developed regions (most of the towns and part of the villages); and

System Reform" in 1985 said "the nation will try best to support the less-developed regions to reduce the illiterate rate." One direct consequence is that the CSLs have compressed the education inequality across the nation. For example, the illiterate rate for those aged over 15 in rural areas declined by 25 percentage points from 37.7% in 1982 to 11.6% in 2000 while that in urban areas only declined by 12 percentage points from 17.6% to 5.2% in the same period (Yearbooks Population Survey, 1982 and 2000). Therefore, this study explores both the temporal and geographical variations in the law enforcement to identify the effects of education.

Different from the small changes in education reforms in other countries that happened in last century, the CSLs in China got great achievements: the enrollment rate for junior high school increased by 26 percentage points from 69.5% in 1986 to 95.5% in 2000, and the number of students enrolled in junior high school increased by 8.9 million. It made China the first and the only country attaining the "nine-year compulsory schooling" goal among the nine largest developing countries.[9]

It is the first time for the largest developing country to enforce such compulsory schooling laws. It is unrealistic to require those with no formal education but aged over 10 to complete the full nine-year compulsory schooling. Those aged 12, for example, are required to go to school to receive education until they are reach 16. They can stop their education legally and go to work because they are not age-eligible any more. Thus, the laws actually defined the age-eligible children as those aged between 6 and 15, and required the minimum school leaving age being 16 rather than truly "9-year" formal education, at least in the first few years.

2.2. Data and Variables

The main sample used in this study is from CFPS, CHIPS and CHNS, three on-going and largest surveys in China. The detailed description is in Data Appendix. I keep the variables

3) least-developed regions (mainly villages).

[9] The nine countries are China, India, Indonesia, Pakistan, Bangladesh, Mexico, Brazil, Egypt and Nigeria.

consistently measured across the datasets, if possible: 1) demographic variables: gender, year of birth, *hukou* province (i.e. the province where the household was registered), and type of *hukou* (rural/urban); 2) socioeconomic variables: years of schooling and marital status; 3) health and health behavior variables.[10]

Because the compulsory schooling laws was announced and implemented in 1986, I keep those birth cohorts born after 1955 and earlier than 1993 and surveyed between 1995 and 2011, so that there are as many affected and unaffected cohorts in the sample. Furthermore, I also restrict the sample to the individuals aged over 18 because most of the respondents have completed their education by then. For simplicity, I also drop those aged over 50 because all of them are ineligible to the CSLs and the mortality rate start to increase. Similar to the procedures in previous literature (e.g. Cutler et al., 2015; Altonji et al., Forthcoming), I pooled the three datasets together. The total number of observations is over 100 thousand, making it one of the largest micro-level samples to analyze the impact of education on health so far.[11] Table 1 report the mean and standard deviations of the key variables used in the study.

[Table 1 about here]

Self-Reported Health Previous literature suggests the self-reported health is highly predictive of mortality and other objective measures of health (Idler and Benyamini, 1997), and thus this study use this measure as a major outcome for individual health outcome.[12] The measure of self-reported health is based on the answer to the question "How is your health in general?" in the three surveys, which ranges from 1 to 5: 1 for Excellent, 2 very good, 3 good,

[10]CHNS were collected in nine provinces and in almost every two years since 1989: 1989, 1991, 1993, 1995, 1997, 2000, 2004, 2006, 2009 and 2011. The CHIPs and CFPS data are sampled nationwide. But the CHIPs data used here include those collected in 1995, 2002, 2007 and 2008; the CFPS data here are those surveyed in 2010 and 2012. More details can be found in Data Appendix.

[11]Since the three different datasets were collected in different years and different provinces, I allow the systematic differences across the different datasets by including dummies for the province, survey year, data sources and all the possible interactions between the three.

[12]Although individual mortality is a more accurate and objective measure for health and has been widely used in previous literature, the sample here is much younger than those examined in previous literature, and the mortality rate for this age group is too low.

4 fair and, 5 poor. Indicator for reported fair or poor health is equal to one if the answer is 4 or 5, and zero for otherwise. Table 1 shows that 19 percent of respondents reported fair or poor health in the sample.

BMI, Underweight and Obesity BMI is also a widely used variable in the literature to depict the nutrition situation and has shown to be correlated with mortality and economic growth (Fogel, 1994). All the three surveys provide necessary information to calculate BMI,[13] and I define the underweight status as BMI being less than 18.5 and obesity as BMI greater than 30. Table 1 reports that the underweight rate is 8% while the obesity rate is only 2%,[14] indicating that the obesity problem seems not to be a big issue compared to the popular obesity in the developed areas like the US and Europe.

Smoking Due to high smoking rate in China and the close relationship between smoking and mortality (Wasserman et al., 1991), this study also examines the effects of education on smoking. In most of the surveys, respondents were asked "Do you smoke now?" or "Did you ever smoke last week?". I then code the respondents as current smokers, which equals to one if the answer to these questions is "yes", and zero if otherwise. The smoking rate is 26% for the full population and most of the smokers are men, whose smoking rate is higher than 50%, almost three times of that in the US.

Cognitive abilities Cognition refers to mental processes that involve several dimensions, including thinking part of cognition and includes memory, abstract reasoning and executive function, and the knowing part, which is the accumulation of influence from education and experience. (Hanushek and Woessmann, 2008; Lei et al., 2012; Huang and Zhou, 2013). The

[13]Height and weight are reported by respondents themselves in CHIPS and CFPS but are measured by professional nurses in CHNS. This study simply takes the BMI derived from the reported variables and that from measured variables equally. In our regressions, we controlled for the indicators for calendar year, data source and hukou provinces and all of their interactions to capture any possible systematic bias. I also drop those BMI with values being smaller than 10 or larger than 50 (less than 1 percent of the sample) because these outliers are mostly due to falsely reporting

[14]There are 12 percent of women in the sample suffering underweight, though not reported in this table.

CFPS measured the cognitive abilities by two sets of tests. For the words recalling test, interviewers read a list of ten nouns, and respondents were asked immediately to recall as many of the nouns as they could in any order. The test would stop if the respondents continuously spoke three nouns that were not in the list. The other test is about mathematical calculation ability: the respondents were asked to answer the 8 or 10 math calculation questions and the test would also terminate if the respondents answer three questions wrongly in a line. I calculate the proportion of right answers of each test and use the Z-score in each year as the cognition measures.[15]

Demographics and Education The basic demographic variables like education, gender, type of hukou (urban/rural), and year of birth (or age) are consistently collected in the surveys. For all the surveys, information about schooling years is provided.[16] Panel B of Table 1 reports the basic statistics for these variables; the people in the sample are aged 30 on average, and 33 percent of them lived in urban areas.

III. First Stage: Impact of CSLs on Education

3.1 Graphical Analysis

Because the central government allowed the provincial governments to implement the policy separately, I collected the formal official document and report initial effective year of the CSLs in each province in column 1 of Table 2 and report the the first cohort affected in column 2.[17] Note that one feature of CSLs in China is the *uniform* nine-years compulsory schooling. I hypothesize that the provinces with lower education prior to the CSLs would

[15]I do so because of different number of questions are used in the different survey years.

[16]There are some people reporting over even 25 years of schooling and I drop the 10 observations in my sample. I also use a dummy variable indicating whether the individual has at least 9 years of schooling as a robustness check.

[17]The timing of the CSLs, as shown in Table 2 , is weakly correlated with the education level of each province (Correlation coefficient = 0.2). Regressing the law effective year on the education prior to CSLs yield insignificant (p-value = 0.27) though positive coefficient. In further analysis, this study also allows the provinces to endogenously determine when to start the CSLs, finding the results are also consistent. Results are available upon request.

increase more because of the uniform 9-year compulsory schooling. So I first calculate the proportion of those with fewer than 9 years education in the birth cohorts prior to the CSLs (within 15 years) in each province and report them in column 3. It ranges from 0.05 for Beijing to 0.79 for Fujian and has a large variation, suggesting a large regional education inequality in China before the CSLs enforcement.

[Table 2 about here]

I then use graphic analysis to provide some evidence for the hypothesis. I divide the provinces by the median value of column 3 of Table 2 into high-education provinces and low-education ones. Then I regress the schooling years on the dummies of different birth cohorts relative to the CSLs eligibility for each group, with controlling for gender indicator and dummies for *hukou* province, survey year, sample source (CHNS/CFPS/CHIPS) and all of their interactions. The the reference group is the just-CSLs-eligible cohort (i.e. the birth cohorts aged 15 when CSLs started in the local province). Figure 1 reports the results. Consistent with the hypothesis, the schooling years in the low-education provinces increased about 1.6 years on average while that in the high-education provinces only increased about 0.7 years.

[Figure 1 about here]

3.2. Econometric Methodology and Empirical Results

I estimate the following equation to formally test the hypothesis:

$$Edu_{ijbt} = \alpha_0 + \alpha_1 Eligible_{bj} + \alpha_2 prop_j^{prior<9} \times Eligible_{bj} + \alpha X_{ijbt} + \delta_{sjt} + \epsilon_{it} \quad (1)$$

where the subscripts i, j, b and t denotes the individual i, province j, birth cohort b and survey year t, respectively. The dependent variable Edu_{ijbt} denotes year of schooling of individual i and $Eligible_{bj}$ denotes the CSL-eligibility for the birth cohort b in province j, which equals to one if the individual is fully eligible to the CSLs (i.e. aged 6 or below) and equals to zero if the individual is totally ineligible (i.e. aged 16 or above). Then I assume the eligibility follows the linear function in between. The results do not rely on the linear-function assumption. I also used the step function in between (i.e. every three years or five years) and find consistent results.

One potential issue here is that the *hukou* province may be not the province where they received their education. It is true but I cannot address this issue without further information since the surveys do not provide needed information. But according to the census 2005 and later waves of CHNS, the proportion for those with the province living in being not the hukou province is less than 5 percent, suggesting this may not the first order issue driving the results.

In equation (1), X_{ijbt} denotes a set of control variables, including dummies for gender, type of *hukou* (urban/rural), married, age and year of birth. δ_{sjt} denotes a set of dummies, including data sample s(CHNS/CFPS/CHIPs), province j and survey year t and all of the three interactions. Adding δ_{sjt} into the equation controls for not only the potential systematic difference existing across datasets but also the different contemporaneous conditions in each provinces.

$prop_j^{prior<9}$ denotes the proportion of people with fewer than 9 years schooling in the population born prior to the CSLs in province j (i.e. the value of column 3 in Table 2). Since it varies at province level, main effect has been absorbed by the province dummies. The coefficients of eligibility (α_1) and the interaction (α_2) are of main interest because they capture the main effect of CSLs, and differential increase in education after the CSLs between the provinces with lower and higher prior education. In practice, I interact the CSLs eligibility with the *demeaned value* of $prop_j^{prior<9}$. Thus the coefficient on eligibility(α_1) can be interpreted as the impact of CSLs on education at the mean level of prior education, which is expected to be positive. I also expect $\alpha_2 > 0$, which suggests those with lower education prior to CSLs will increase more afterwards.

Table 3 reports the OLS estimation for α_1 and α_2, with the standard errors clustered at provincial-year of birth level. Column 1 presents the results without the interaction term, showing that CSLs increase the schooling years by 1.1 years on average. Estimates in column 2 show that $\alpha_1 > 0$ and $\alpha_2 > 0$, and both of them are significant. The magnitude of the coefficient suggests that the policy-induced education increase in regions with lower education before CSLs (e.g. Fujian, Jiangxi and Gansu) would be 1.5 years more than the regions with higher education before CSLs (e.g. Beijing, Tianjin and Shanghai).

[Table 3 about here]

One potential issue is that time trends across the different regions, caused by other factors like economic growth, may drive the estimation. This issue is also relevant to Stephens and Yang (2014) because they found the results in previous literature become insignificant and wrong-signed when including region-specific linear trends. I control for province-specific birth cohort linear trends in column 3. The estimates show that the impact of CSLs is robust to including these, suggesting that the other birth cohort linear trends across different regions should not be the first order factors.

In the final column, I add the interaction between CSLs-Eligibility and square of demeaned proportion of those with fewer than 9 years education prior to CSLs into the regression in column 4. The predicted education increase are consistent across different provinces. The next section shows that the conclusions are also consistent when using the square term as an additional source of exogenous variation in schooling.

Appendix Table A1 further divides the sample by gender and by hype of *hukou* to examine the heterogeneous impact of CSLs on education. Consistent with the policy implementation, the results show that the impact of CSLs is larger for women and for the people with rural *hukou*.[18]

[18] Note that the F-tests across all columns are large enough except for the urban hukou sample, thus in the second stage results, I show the results by gender but not by type of hukou.

3.3. Evidence for Exogeneity of the CSLs

Evidence 1: Other Confounding Factors or Other Policies?

Comparison between before- and after- CSLs across the provinces captures the differential education increase across the regions. However, the timing of CSLs and the interaction may pick up the variations of other policies because China enforced a series of different policies in the 1980s. But it is difficult to list all contemporaneous policies in different regions during that period and test their correlation with timing and enforcement of the CSLs.

Instead, I directly test to what extent that CSLs increased the schooling years. Note that the education reform requires nine-year compulsory schooling for all the provinces. Therefore, the constructed variables based on the CSLs may increase the education up to and only up to nine-year schooling. However, there is no evidence that other confounding factors like local opinions towards education or other policies will increase the schooling years only to 9 years.

To test this, I construct a set of indicators for different years of schooling, use these indicators as dependent variables and conduct the regressions as equation (1). Figure 2 reports the results for these different indicators. The intervals show the heterogeneous effects from 10th to 90th percentiles of the prior-CSLs education across the regions. The coefficients are significantly positive if the schooling years does not exceed the threshold - nine. Once the schooling years is above nine, however, the impact of the policy diminished dramatically both for the main effects and the heterogeneous effects across regions. These findings suggest that the positive association between education and constructed variables in Table 3 should originate from the CSLs rather than other unobserved factors like the implementations of other policies.

[Figure 2 about here]

Evidence 2: "Regression to the Mean" and Nutrition Status?

I also conduct two sets of placebo tests to provide further evidence on excludability of the constructed CSLs variables. The first set aims to test whether the impact or associations in Table 3 are only "regression to the mean". First, I restrict the sample to those cohorts earlier than the first affected cohort (i.e. the cohorts 2-15 years earlier than the first affected cohort). And then I suppose the implement year of CSLs to 5 years before, estimate the same regressions as equation (1) and report the results in the first two columns in Table 4. The results provide no evidence that pre-trends or regressions to the mean matters much in this analysis.

[Table 4 about here]

The second set of placebo tests are conducted to test whether the impact of CSLs reflect the better nutrition of the individuals in the childhood or young adulthood. I use the individual height as an independent variable since height is proved to be a good measure for health and nutrition status of childhood and young adulthood (Thomas et al., 1991; Huang et al., 2013). If the impact of CSLs reflects the economic development or nutrition improvement, the effects should be captured in height. The estimates in the last two columns of Table 4 provide no evidence for it.

IV. Effects of Education on Health

4.1. Baseline Results

I begin the analysis by first conduct the OLS estimation for following equation as benchmark:

$$Health_i = \theta_0 + \theta_1 Edu_i + \theta X_i + \delta_{sjt} + \epsilon_i \quad (2)$$

where the dependent variable, $Health_i$, denotes the health outcome variables, which may be self-reported health, underweight, smoking or cognition, and all the other variables are the same with those in equation (1). Panel A of Table 5 reports the OLS estimates of θ_1, showing that higher education is correlated with better health in general. The sample size varies across columns because some surveys may not collect the corresponding health information. For example, the cognition tests (i.e. words recall and math calculation) are only collected by CFPS).

[Table 5 about here]

Panel B shows the reduced form results, whereas the education is replaced by the constructed CSLs variables (i.e. $Eligible_{bj}$ and $prop_j^{prior<9} \times Eligible_{bj}$) directly:

$$Health_i = \lambda_0 + \lambda_1 Eligible_{bj} + \lambda_2 prop_j^{prior<9} \times Eligible_{bj} + \lambda X_i + \delta_{sjt} + \epsilon_i \quad (3)$$

Since both $Eligible_{ij}$and $prop_j^{prior<9} \times Eligible_{ij}$ predict higher education, the signs of the coefficients in reduced form estimations should be negative for bad health and positive for better health. The estimates in Panel B provide consistent evidence for this. Finally, I then use Two-Stage Least Squares (2SLS) to estimate the effects of education on health:

$$Health_i = \beta_0 + \beta_1 \widehat{Edu}_i + \beta X_i + \delta_{sjt} + \varepsilon_i \quad (4)$$

where $\widehat{Edu}_i$ is the predicted education value of equation (1) and all the other variables are the same with those in equation (1). Panel C presents the 2SLS estimates, which are of main interest in this analysis. Due to different samples, the F-tests in the first stage (i.e. Weak Instrumental Variable Tests) and Hansen tests (Over-Identification Tests) for the instruments are reported in the bottom of each column.[19]

The 2SLS estimates are about three times larger in general. On one hand, it is possible that the effects among the compliers (i.e. the ones increase education under CSLs and do not if without the laws) are larger since the effects identified from 2SLS are local average

[19]The large F-statistics reject the hull hypothesis and provide evidence for significant first stage. This study did not report the detailed first stage for different outcomes but the results are available upon request. In general, the instruments also passed the over-identification tests, except for smoking.

treatment effects (LATE). Hence, I divide the whole sample by whether the individuals completing nine-year education and conduct OLS estimation to investigate the associations of education with the health outcomes for each group. In general, the results of Appendix Table A2 provide consistent evidence for it.[20] On the other hand, the OLS estimates may be biased to zero due to the classic measurement error in education because the values were reported by the respondents themselves, and these reported values may be wrong due to lack of awareness.

The first column in Table 5 provides estimates for self-reported fair or poor health, indicating that an additional year increase in schooling decreases the probability of reporting fair or poor health by 2 percentage points.[21] Column 2 of Panel C shows that an additional year of schooling leads to about 1.2 percentage points drop in underweight rate, suggesting education improves nutrition status. However, the results are different from the findings: in the developed regions like the US and Europe, literature usually finds negative effect of education on BMI (Kemptner et al., 2011; Brunello et al., 2013). Estimates in the next three columns of Table A4 show that education in China increases BMI but the effects only exist in the sample with lower BMI. The estimates do not provide evidence that education increases the obesity in China. These findings suggest that schooling increases BMI in developing countries through decreasing underweight proportion but decreases BMI in developed countries via reducing the obesity rate.

Column 3 in Table 5 shows the effects of education on smoking. The 2SLS estimates suggest that an additional year in schooling reduces the likelihood of smoking by 1.5 percentage points, which are consistent with the findings of De Walque (2007) and Jensen and

[20] The associations in the lower education group ($<$9 years) tend to reflect the impact of education among the "complier" group since previous analysis shows the CSLs are mainly effective in the lower education group. Consistent with the hypothesis, the coefficients in Panel A are generally larger in magnitude than those in Panel B. The only exception is the results for smoking, and the reason could be income effects.

[21] Considering the CHNS used four-point scale and the other two used five-point, I drop the CHNS sample and re-estimate the effects of schooling in column 2 of Appendix Table A3, which yield very consistent results. In the last column, I further examined the effects of schooling on reporting excellent health and the 2SLS estimates show that an additional year of schooling increases the likelihood of reporting excellent health by about 1.2 percentage points.

Lleras-Muney (2012). The last two columns examine cognition. This study provides first evidence for the causal effects of education on cognition among working age population, as the estimates in the last two columns of Table 3 suggest an additional year of schooling increases the cognition by 0.16 standard deviations for both words recalling and math calculation.[22]

The difference between reduced form and 2SLS estimates should be noteworthy. The 2SLS estimates are based on the exogeneity of CSLs and estimates the effects of education on health among compliers. The estimates do not consider the spillover effects or externalities that are documented by Acemoglu and Angrist (2001). The reduced form estimates, however, estimates the effects of CSLs implementation on health outcome directly and thus the main and spillover effects of the education reform are estimated together.

4.2. Robustness Checks

With consideration that health and behaviors may be different in men and women due to biological and cultural reasons, I conduct the gender-specific 2SLS estimation and report the results in Figure 3. The results indicate some differences between two sexes. In Figure 3a, the effects of education on self-reported health are larger in magnitude among women but the difference between two sexes is insignificant. However, the effects on underweight are larger for women and those on smoking are much larger among men. Figure 3b shows that effects are similar for men and women.

[Figure 3 about here]

Considering the CHNS are from nine provinces and combined the three samples together might put disproportionate weights on these provinces, I weight the regressions in Panel A of Appendix Table A5 by the population of the province divided by the number of observations,

[22] These findings are consistent with Carlsson et al. (2012), which which found that 180 days extra schooling increased crystallized test scores by approximately 0.2 standard deviations among the 18-years-olds adolescents in high schools in Sweden. They are also consistent with Aaronson and Mazumder (2011), which found that the construction of Rosenwald schools had significant effects on the schooling attainment and cognitive test scores of rural Southern blacks.

and it yields very consistent estimates. I also use another education measure, an indicator whether the respondent finished the junior high school, and report the results in Panel B of Appendix Table A5. The results are also consistent.

I also use the square of proportion of people with lower than 9 years education interacting with the CSLs-eligibility as another instrument to check the robustness of the results. Figure A1 show the consistent results. All the estimates show consistent estimates when adding the new variable as additional instrument, indicating that this non-linear relationship.[23]

Figures A2 present the original estimates and the ones including provincial specific linear trends, which shows that adding trends does not influence the estimates for the effects on self-reported health and cognition.[24]

Another concern about the above analysis is that the sample covers a large span of birth cohorts (i.e. 1955-1990). I test the robustness of the results by trimming the sample to those born between the birth cohorts 15 years earlier or later than the CSL-eligible birth cohort. The estimates are reported in Figure A3, showing a fairly consistent pattern in the trimmed sample.

V. Understanding the Effects of Education on Health

5.1. Econometric Framework

Due to data limitation and lack of exogenous variation, most previous studies mainly focused on whether education has causal effects on health rather than how and why. The mixed findings in literature call for studies to investigate the mechanisms through which education affects health and this section aims to shed some light on it.

To verify the mechanisms and quantitatively estimate how much the certain mechanism

[23]Panel A of Figure A1a first show the 2SLS estimates when only using CSL-eligibility as an instrument, which has a larger magnitude but a much wider confidence interval.

[24]But doing so changes the estimates in magnitude for underweight and smoking, as the effect on underweight diminish but that on smoking are strengthened. However, the estimates do not provide evidence for significant differences between the coefficients under the two setting for both outcomes given the wide confidential intervals.

can explain the effects, I follow Cutler and Lleras-Muney (2010) and estimate the following two equations:

$$Health_i = \gamma_0 + \gamma_1 Eligible_{ij} + \gamma_2 prop_j^{prior<9} \times Eligible_{ij} + \gamma X_i + \delta_{sjt} + \epsilon_i \quad (5)$$

$$Health_i = \gamma_0' + \gamma_1' Eligible_{ij} + \gamma_2' prop_j^{prior<9} \times Eligible_{ij} + \gamma' X_i + Z_i + \delta_{sjt} + \epsilon_i \quad (5')$$

where the dependent variable $Health_i$ is the main health outcome - self-reported fair or poor health, and Z_i denotes the potential intermediate variables (i.e. smoking, income or cognition). I only use the two instrumental variables directly - $Eligible_{ij}$ and $prop_j^{prior<9} \times Eligible_{ij}$ rather than the education itself because the former are more exogenous and the estimated effects capture the potential spillover effects. And the coefficients on them show the effects of the CSLs and thus reflect the education effects. Different from Cutler and Lleras-Muney (2010), however, we have two coefficients relevant to the impact of education in each equation and it should be not right to only focus on either one of them to verify the potential channels.[25] Therefore, I extend the methodology as follows:

Estimating the above two equations yields γ_1, γ_2 and γ_1', γ_2'. If $prop_j^{prior<9}$ is not demeaned, γ_1 can be interpreted as the education's effects on health in the regions where all the earlier cohorts have nine or more years schooling and $\gamma_1 + \gamma_2$ the effects where all having less than 9 years schooling. I illustrate them in Figure 4a. When the intermediate variable(s) Z_i is controlled, the two coefficients become γ_1' and γ_2'. Therefore, the explained proportion for the whole population is provided by

$$E = 1 - \mid \frac{E_1}{E_0} \mid$$

where $E_0 = \int_{\underline{p}}^{\overline{p}} (\gamma_1 + \gamma_2 p) f(p) dp$ is the weighted mean of *the original* effects and $E_1 = \int_{\underline{p}}^{\overline{p}} (\gamma_1' + \gamma_2' p) f(p) dp$ is the weighted mean of the *conditional* effects with certain intermediate

[25]In Cutler and Lleras-Muney (2010), there is only one coefficient on education, and thus the relative changes of this coefficient when adding potential intermediate variables can be interpreted as the part that can be explained by the intermediate variables.

variables controlled. And $f(p)$ is the population density function, and $\underline{p}$ and $\overline{p}$ are the lowest and highest values for p among the population, respectively.

The setting above also enables to calculate the explained part for the regions with different education level among earlier cohorts. As the Figure 4a shows, for the region with p proportion of people with less than 9 years schooling in earlier cohorts, the part that can be explained by the intermediate variable is

$$e(p) = 1 - \mid \frac{\gamma_1' + \gamma_2' p}{\gamma_1 + \gamma_2 p} \mid .$$

Therefore, estimating the equations (5) and (5') enables the estimation for $e(p)$. Plotting $e(p)$ over p presents how the explained part changes with the prior education level of local region. Because of the large heterogeneity in p across the regions, this further provides some new insights on how education affects individual health in different development stage. For example, figure 4b & c provide two different cases. In figure 4b, the explained part (the grey area) increases as the education level goes down (i.e. more people with less 9-year schooling among earlier cohorts); but in 4c, the explained part decreases as the education level goes down.[26]

5.2. Empirical Results on Mechanisms

Previous literature provides some potential mechanism candidates. The first one is income: since higher education predicts higher income, this allows the people to live in a life with higher quality like living in a house in a safer region and with better environment or having less financial pressure etc. Another is health behaviors; many papers argue that the people with higher education are less likely to have bad health behaviors like smoking and drinking heavily and thus they are in better health status. In addition, cognition is also a potential channel because better cognition help individuals to make wiser and rational choices like

[26] The difference in the two figures reflect the different changes in the estimated coefficients. Figure 4b is the case when γ_1 and γ_1' are close to each other but γ_2' is much smaller in magnitude than γ_2; but Figure 4c is the case when there is a large difference between γ_1 and γ_1' but not so much reduction in the magnitude of γ_2.

choosing proper food and taking drugs in a right way if necessary, evaluate the potential risks in life and avoid the potential danger etc. Due to data availability, this study mainly focuses on these possible mechanisms: income, smoking, as well as cognition measured by words recall and math calculation.

Table 6 reports the results for the explained proportion by the intermediate variables. Note that the results may be different in the two sexes, I also conduct the analysis by gender and report the results in different rows; since only CFPS data measures cognition, I further conduct the parallel analysis for the full and the CFPS samples separately and report the results in Panel A and Panel B. Column 1 reports the original effects.[27] The original effects are very similar in the two samples. Column 2 reports the conditional effects when the possible intermediate variable, income, is controlled for and column 3 the proportion that can be explained by it.[28] The part that can be explained by income ranges from 3.8 to 11% in the full sample and 1.3 to 7.9 in the CFPS sample. One possible reason why the estimates in CFPS is smaller is the survey years of CFPS data are 2010 and 2012, the latest two years, when the households and individuals had higher income. In addition, the part can be explained by income is consistently larger for men for both samples, which are 10.7% in the full sample and 7.9% in the CFPS sample (the statistics are 3.8% and 1.3% for women).

Columns 4 and 5 report the part that can be explained by smoking behaviors. The effects estimated in column 4 almost are unchanged compared to column 1 and the explained part ranged from -2.7% to 2.0%, implying that smoking behavior is not an important mechanism in the effects of education on health.

Note that the first few columns show very consistent results in the two panels, which suggests the feasibility to use CFPS data to calculate the part that can be explained by cognition. Column 6 reports the *conditional* effects when only cognition measured by words recall and math calculation is controlled for, and column 7 reports the reduction of magnitude

[27] Note that the original effect is calculated by $E_0 = \int_{\underline{p}}^{\overline{p}} (\gamma_1 + \gamma_2 p) f(p) dp$.

[28] The income here include both individual income and household income. Table A6 in the appendix shows that the CSLs also increased both of them.

in percent. The proportion that can be explained by cognition ranges from 12.5% for men to 22.0% for women, implying that cognition is an more important channel among women. In addition, the part that can be explained by cognition is larger than that by income, suggesting that the cognition be the most important intermediate variables examined here. These findings are also consistent with the literature that highlights the importance of cognition in economic development (Hanushek and Woessmann, 2008), decision making Frederick (2005) and health behaviors (Cutler et al., 2008; Cutler and Lleras-Muney, 2010).

As mentioned above, based on the estimated γ_1, γ_2 and γ_1', γ_2' for each possible pathway, I further plot the $e(p)$, the explained part by certain pathway, over p, the proportion of people with less than 9-year education in the local region in Figure 5. Figures 5a and b presents the proportion explained by the income over the proportion with less 9-year education among CSLs-non-eligible cohorts, by full sample and CFPS sample. The lines indicate that the proportion that can be explained by income increases in the regions wither lower prior education. The magnitude is not small: the explained part in the regions with more low education people is about 2-3 times of that in the regions with fewer lower education people. This suggests that the income potentially plays a more important role in the poorer regions. In addition, consistent with Table 6, the proportion explained by income is consistently lower in CFPS data.

Figure 5c shows the pattern for the proportion explained by cognition. Different from income, there is no significant change for the full sample or for the men, which are about 0.15 and 0.12, respectively. For the women, the proportion reduces from 0.25 to 0.20 as the proportion of lower-education people increases. Figure 5c shows the pattern when controlling for all the possible mechanisms examined here. It is easy to find that the patterns are mixed by the income effects and the cognition effect together. Specifically, the decreasing line for women is almost driven by that of cognition, and the increasing line for men is almost driven by that of income. And the level is almost shifted up by cognition because the proportion that can be explained by cognition is larger than that by income.

VI. Conclusions and Discussion

It is important to know whether and why education has causal impact on health. However, the controversial discussion in the literatures do not come to a consensus that education improves individual health but reveals the heterogeneity in education's effects across different countries. This paper uses the exogenous temporal and geographical variation in CSLs establishment in China around 1986 to identify the effects of schooling on a series of health outcomes including self-reported health, underweight, smoking and cognition.

This paper hypothesize and provides sound evidence that the CSLs increased the education of the regions with prior lower education more when the laws became effective due to the "nine-year" compulsory schooling. The 2SLS results show that one additional year of schooling decreases 2-percentage points in reporting fair or poor health, 1.1-percentage points for underweight and 1.5-percentage points for smoking. These results are robust to different model specifications. There are some heterogeneity for men and women.

This study further aims to unravel the potential mechanisms. I extend the framework in Cutler and Lleras-Muney (2010) and find that income and cognition explain the impact of education on self-reported health by 7% and 15%, separately. Gender difference exists. For example, income explains 7-10% for men but only 1-3% for women; cognition explains over 20% for women but only 12% for men. These results suggest that to make people know the health knowledge is even more important for health than income, especially for women. In addition, the results also suggest that the proportion explained by cognition is stable across regions with different education levels. But income is an important mechanism for the regions with lower education: the proportion explained by income can reach up to 20% for men in the lower education regions. These findings suggest the mixed findings in the previous literature may be due to the different effectiveness of the potential pathways and channels. However, since there is a large part that cannot be solely explained by cognition, income or health behaviors, the results also call for research in the future to further shed light on other potential mechanisms.

However, there are also a couple of pitfalls that this paper suffers. Although the CSLs are used widely in the literature to estimate the causal impact of education, this methodology is not perfect due to potential endogenous policies decisions in timing and intensity. Although the robustness checks and placebo tests suggest the validity of the instrument, I cannot rule out all the possibilities that may be correlated with the education increase and health outcomes at the same time.

In addition, this paper does not emphasize spillover effects or externalities of education. Since these externalities are probably positive (Acemoglu and Angrist, 2001), it is possible that those who still receive no formal schooling may also improve their health outcomes because of improved health of others. The 2SLS estimation in this paper may bias the results.

Finally, though this study provides some suggestive evidence on a couple of mechanisms, it is far from satisfaction. For one thing, it is still a question how much other potential mechanisms may explain the causal effects of education. For the other, it is also possible that the heterogeneity in mechanisms also exist in different countries and in different periods. Due to data limitation, I leave these questions to studies in the future for us to better understand the effects of education on health.

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Data Appendix

China Health and Nutrition Survey (CHNS)

The China Health and Nutrition Survey (CHNS) was designed to examine the effects of the health, nutrition, and family planning policies and programs implemented by national and local governments and to see how the social and economic transformation of Chinese society is affecting the health and nutritional status of its population. The survey took place over a 3-day period using a multistage, random cluster process to draw a sample of about 4400 households with a total of 26,000 individuals in nine provinces that vary substantially in geography, economic development, public resources, and health indicators. The CHNS data collection began in 1989 and has been implemented every 2-4 years since. The CHNS uses a multistage cluster sample design to survey individuals and households within 218 neighborhoods within nine provinces in China. These nine provinces contain approximately 56% of the population of China. The baseline sample was representative of each province but over time, loss-to-follow-up has occurred.

Chinese Family Panel Studies (CFPS)

The Chinese Family Panel Studies (CFPS) is by far the largest and latest comprehensive household survey with information on demographic, economic, and health aspects of households in China. It is a biennial survey and is designed to be complementary to the Panel Study of Income Dynamics (PSID) in the United States. The five main parts of the questionnaire include communities, households, household members, adults and children data. The 2010 round covered approximately 14,000 households in 25 provinces, in which 95% of the Chinese population reside. The population is divided into six subpopulation, i.e. five large provinces (Guangdong, Gansu, Liaoning, Henan, Shanghai) and the other 20 provinces. The sample was obtained by three-stage cluster sampling with unequal probabilities. The national representative final sample covers about 9,500 households and 21,760 adults.

Chinese Household Income Project Series (CHIPS)

The purpose of the Chinese Household Income Project was to measure and estimate the distribution of personal income in both rural and urban areas of the People's Republic of China. Data were collected through a series of questionnaire-based interviews conducted in rural and urban areas in 1988, 1995, 2002 and 2007. Individual respondents reported on their economic status, employment, level of education, sources of income, household composition, and household expenditures. The study was interview-based. For each year, there are three different datasets for urban, rural residents and migrants, separately. This study only uses the data for the residents. On average, each year has over 20,000 individuals in urban or rural survey. The data are coded on-site observation through face-to-face interview.

Table 1: Summary Statistics

Variables	(1) Obs	(2) Mean	(3) Std. Dev.	(4) Min	(5) Max
Panel A: Health and Health Behaviors					
Health Fair or Poor	88,971	0.19	0.39	0	1
Health Excellent	88,971	0.28	0.45	0	1
BMI	85,275	22.47	3.18	12.11	50
Underweight	85,275	0.08	0.27	0	1
Obese	85,275	0.02	0.15	0	1
Smoke	105,634	0.26	0.44	0	1
Panel B: Education and Demographics					
Years of schooling	114,647	8.86	3.91	0	23
Male	114,647	0.50	0.50	0	1
Age	114,647	32.46	9.16	18	50
Urban	114,647	0.39	0.49	0	1
Married	114647	0.54	0.55	0	1

Notes: Data source is CFPS, CHIPs and CHNS. The variables are consistently measured across the datasets. The sample is composed of the 1955-1993 birth cohorts, aged between 18 and 50, and surveyed between 1995 and 2011.

Table 2: Compulsory Schooling Laws in different provinces

Province	Law effective year	First affected birth cohort	Prop of earlier cohorts with less 9-years education
Beijing	1986	1971	0.053
Tianjin	1987	1972	0.285
Hebei	1986	1971	0.401
Shanxi	1986	1971	0.394
Liaoning	1986	1971	0.352
Jilin	1987	1972	0.487
Heilongjiang	1986	1971	0.385
Shanghai	1987	1972	0.220
Jiangsu	1987	1972	0.306
Zhejiang	1986	1971	0.249
Anhui	1987	1972	0.302
Fujian	1989	1974	0.790
Jiangxi	1986	1971	0.672
Shandong	1987	1972	0.392
Henan	1987	1972	0.358
Hubei	1987	1972	0.288
Hunan	1991	1976	0.357
Guangdong	1987	1972	0.382
Guangxi	1991	1976	0.381
Chongqing	1986	1971	0.226
Sichuan	1986	1971	0.318
Guizhou	1988	1973	0.475
Yunnan	1987	1972	0.499
Shaanxi	1988	1973	0.409
Gansu	1991	1976	0.577
Xinjiang	1988	1973	0.581

Notes: Data source is the education year books for each province.

Table 3: OLS Estimation for Impact of Compulsory Schooling Laws on Years of Schooling

	(1)	(2)	(3)	(4)
Variables	Dependent variable is Years of Schooling			
CSLs Eligibility	1.116***	1.136***	1.242***	1.012***
	(0.381)	(0.360)	(0.382)	(0.359)
Pr(less than 9-year education)		4.065***	6.124***	3.396***
* CSLs Eligibility		(0.646)	(1.445)	(0.613)
Pr(less than 9-year education)				10.83***
square * CSLs Eligibility				(2.174)
Observations	114,647	114,647	114,647	114,647
R-squared	0.243	0.245	0.249	0.245
F-statistic for all the variables	8.572	23.25	16.19	22.13
P-value for the F-test	0.003	0.000	0.000	0.000
Province-YoB Linear Trends	No	No	Yes	No

Notes: Data source is CFPS, CHIPs and CHNS. Robust standard errors in parentheses are clustered at province-year of birth level. Covariates include indicators of type of hukou (Urban/Rural), Year of birth, Age (three-year categories), Hukou Province, Survey year and all interactions of province, year and sample. The Pr(less than 9-year education) variables are demeaned value so that the coefficient on CSLs Eligibility can be interpreted as the impact where the Pr(less than 9-year education) has the mean value.

Table 4: Placebo Tests for Impacts of Compulsory Schooling Laws

	(1)	(2)	(3)	(4)
Settings	CSLs ineligible (2-15 years earlier) and suppose CSLs 5 years before		Use Height as Dep. Var.	
Dependent variable	Years of Schooling		Height (cm)	
CSLs Eligibility	0.266	0.257	0.466	0.463
	(0.622)	(0.617)	(0.447)	(0.448)
Pr(less than 9-year education)		1.415		-0.353
* CSLs Eligibility		(0.940)		(0.570)
Observations	39,511	39,510	87,137	87,137
R-squared	0.305	0.305	0.546	0.546
F-statistic for all the variables	0.183	1.185	1.086	0.728
P-value for the F-tests	0.669	0.306	0.298	0.483

Notes: Data source is CFPS, CHIPs and CHNS. Robust standard errors in parentheses are clustered at province-year of birth level. Covariates and variables definition are the same as those in Table 1.

Table 5: Effects of Education on Health

Dependent variables	(1) Health Fair or Poor (Yes = 1)	(2) Underweight (Yes = 1)	(3) Smoker (Yes = 1)	(4) Words recall Z-score	(5) Math Ability Z-Score
Mean of Dependent Var.	0.190	0.077	0.264	0.000	0.000
Panel A. OLS Estimation					
Years of Schooling	-0.00761***	0.000155	-0.00389***	0.107***	0.0834***
	(0.000448)	(0.000325)	(0.000465)	(0.00142)	(0.000843)
Observations	88,971	85,275	105,634	34,999	34,985
R-squared	0.095	0.053	0.356	0.382	0.809
Panel B. Reduced Form Estimation					
CSLs Eligibility	-0.0628***	-0.00282	-0.0713***	0.320***	0.150***
	(0.0217)	(0.0174)	(0.0208)	(0.0808)	(0.0496)
Pr(less than 9-year education) * Eligibility	-0.0759**	-0.0693**	-0.0123	0.335***	0.103
	(0.0328)	(0.0311)	(0.0358)	(0.111)	(0.0839)
Observations	88,971	85,275	105,634	34,999	34,985
R-squared	0.090	0.053	0.355	0.185	0.684
Panel C. 2SLS Estimation					
Years of Schooling	-0.0205***	-0.0115*	-0.0134*	0.158***	0.0694***
	(0.00642)	(0.00636)	(0.00723)	(0.0265)	(0.0114)
Observations	88,971	85,275	105,634	34,999	34,985
First Stage F-statistics	26.87	27.67	25.78	12.15	12.20
Over-identification P-values	0.125	0.263	0.004	0.06	0.435

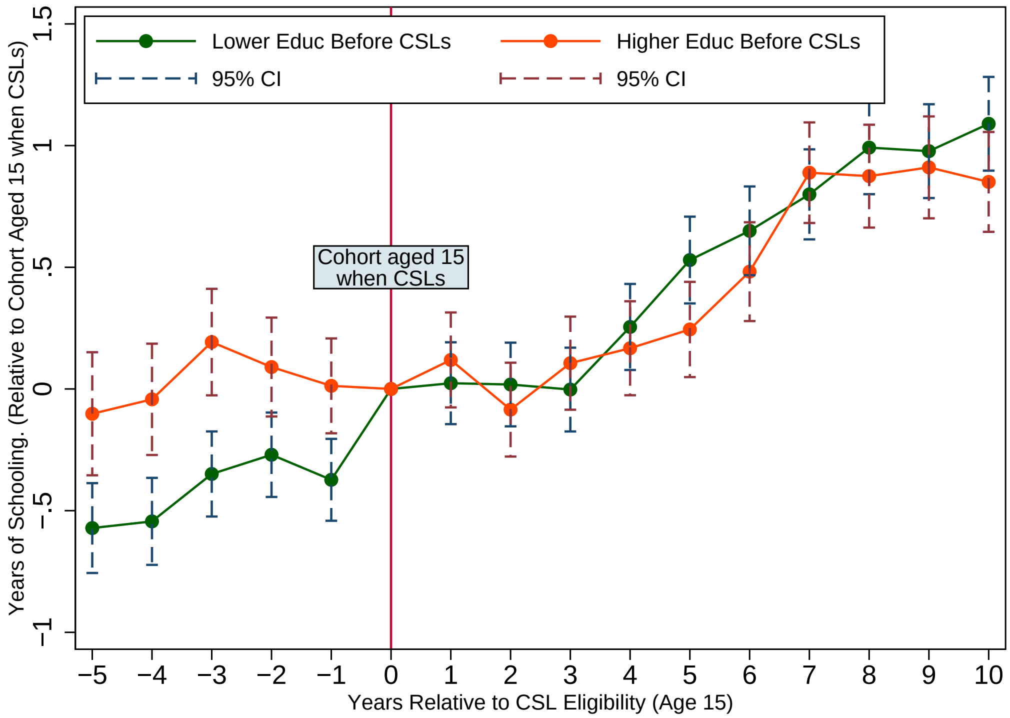
Notes: Data source is CFPS, CHIPs and CHNS. Robust standard errors in parentheses are clustered at province-year of birth level. Covariates and variables definition are the same as those in Table 1.

Table 6: The Role of Income, Smoking and Cognition in Effects of Education

Sample	(1) Original ave. effect	(2) Income controlled: Ave. effect	(3) Income controlled: Explained (%)	(4) Smoking controlled: Ave. effect	(5) Smoking controlled: Explained (%)	(6) Cognition controlled: Ave. effect	(7) Cognition controlled: Explained (%)	(8) All controlled for: Ave. effect	(9) All controlled for: Explained (%)
Panel A: Full sample									
Both genders	-0.061	-0.057	6.79	-0.061	0.26			-0.058	6.91
Male	-0.049	-0.044	10.7	-0.050	-1.68			-0.045	8.86
Female	-0.075	-0.072	3.79	-0.074	1.64			-0.071	5.27
Panel B: CFPS sample									
Both genders	-0.060	-0.057	4.32	-0.061	-0.84	-0.050	15.0	-0.051	15.0
Male	-0.069	-0.064	7.91	-0.071	-2.73	-0.061	12.5	-0.060	13.7
Female	-0.053	-0.053	1.29	-0.052	1.97	-0.042	21.0	-0.042	21.0

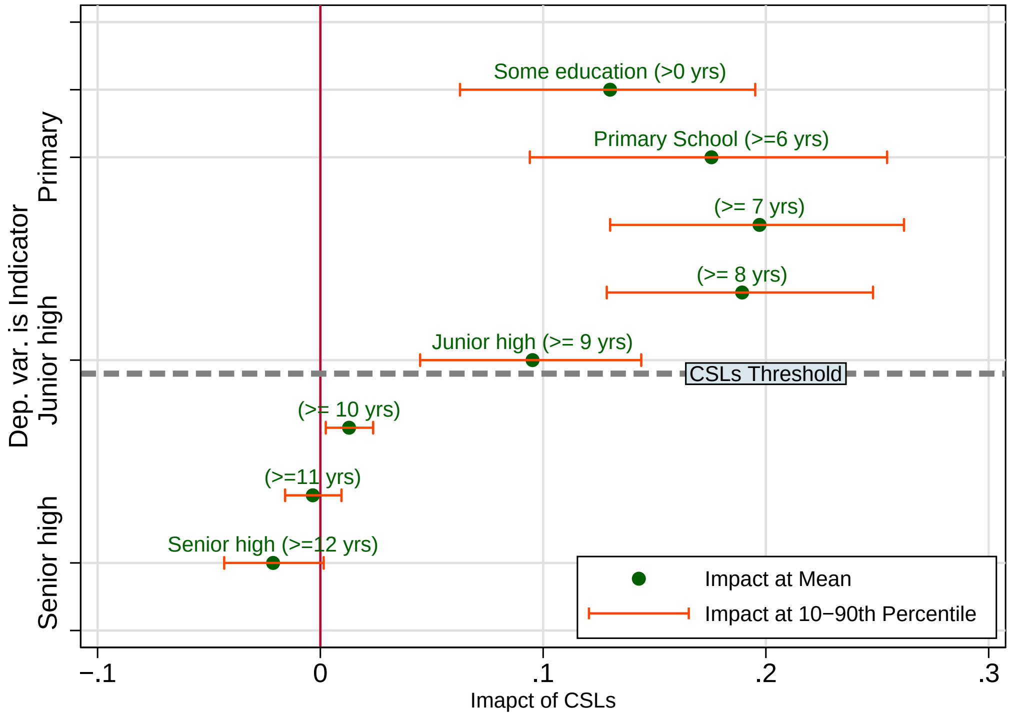
Notes: Data source is CFPS, CHIPs and CHNS. The original average effect is calculated by $E_0 = \int_{\underline{p}}^{\overline{p}} (\gamma_1 + \gamma_2 p) f(p) dp$ where the γ_1 and γ_2 are estimated through equation (5) and $f(p)$ is the population density function. The average effect when controlling for the specific intermediate variable is calculated by $E_1 = \int_{\underline{p}}^{\overline{p}} (\gamma_1' + \gamma'_2 p) f(p) dp$ where he γ_1' and γ_2' are estimated through equation (5'). The corresponding explained proportion is $1 - |\frac{E_1}{E_0}|$.

Figure 1: Years of Schooling Increase over the Time Relative to CSLs, by Education Level Before the Laws



Notes: Data source is CFPS, CHIPs and CHNS. The sample is divided by the median value of proportion of individuals with less than 9-year education prior to CSLs. For each subsample, regression is conducted to estimate how the years of schooling change over the time relative to the CSLs eligibility, with controlling for gender indicator and dummies for hukou province, survey year, sample (CHNS/CFPS/CHIPS) and all of their interactions. The reference group is the cohort *just-eligible* for the CSLs (i.e. the birth cohorts aged 15 when CSLs started in the local province) for each subsample. Both point estimation and 95% confidential intervals are reported for the coefficients on the dummies of the relative years to the CSLs eligibility.

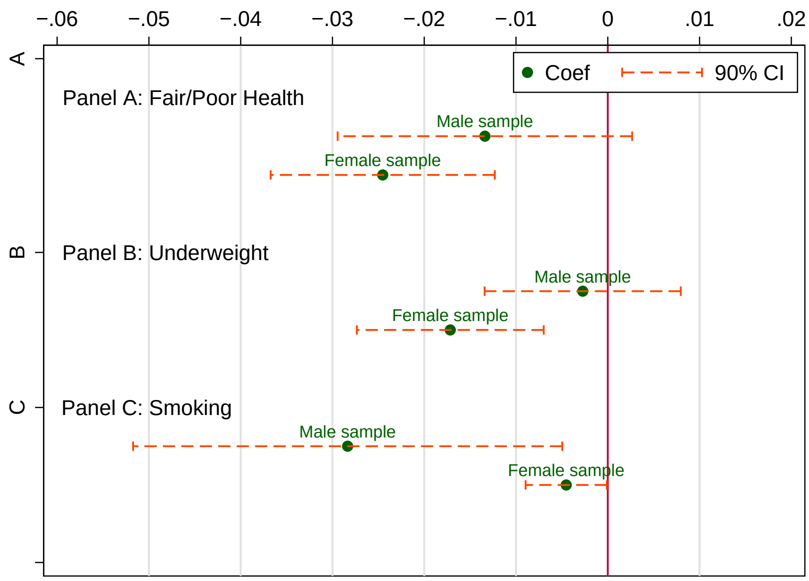
Figure 2: Impact of CSLs on Years of Schooling at Different Education Levels



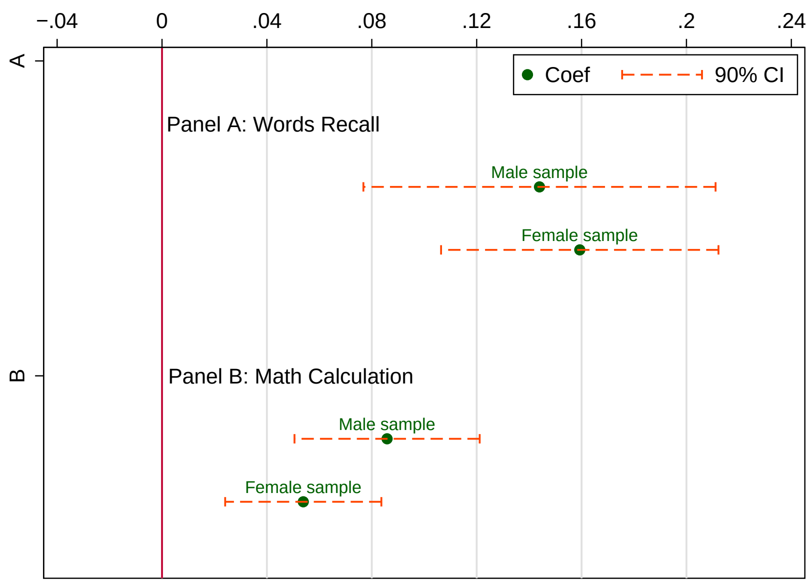
Notes: Data source is CFPS, CHIPs and CHNS. Each row reports a specific the OLS estimation when the dependent variable is the indicator for completing the corresponding years of schooling (as marked). The independent variables are described in equation (1). The points in the figure report the coefficients on CSLs-eligibility and the intervals show the impact from 10th percentile to 90th percentile of the prior education level calculated from the OLS estimates.

Figure 3: Effects of Education on Health, by Gender

a. Effect of Education on Fair/Poor Health, Underweight and Smoking



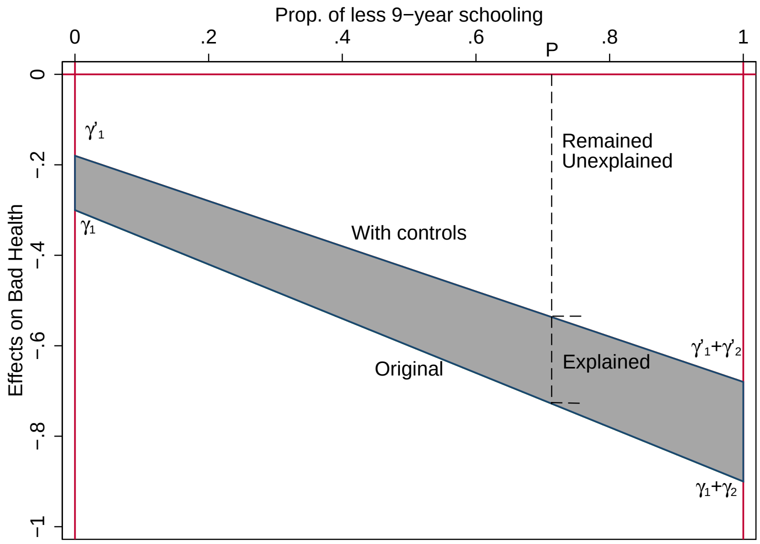
b. Effect of Education on Cognition



Notes: Data source is CFPS, CHIPs and CHNS. Gender-specific 2SLS estimation (Equation 2) is conducted for each outcome. The points show the coefficients on the years of schooling in the 2SLS estimation and the intervals are the 90% confidential intervals based on standard errors clustered at province-year of birth level.

Figure 4: Effects of Education on health and the Part Explained by Intermediate Variable

a. Explained v.s. Unexplained



b & c. More/Less can be explained against the prior education level

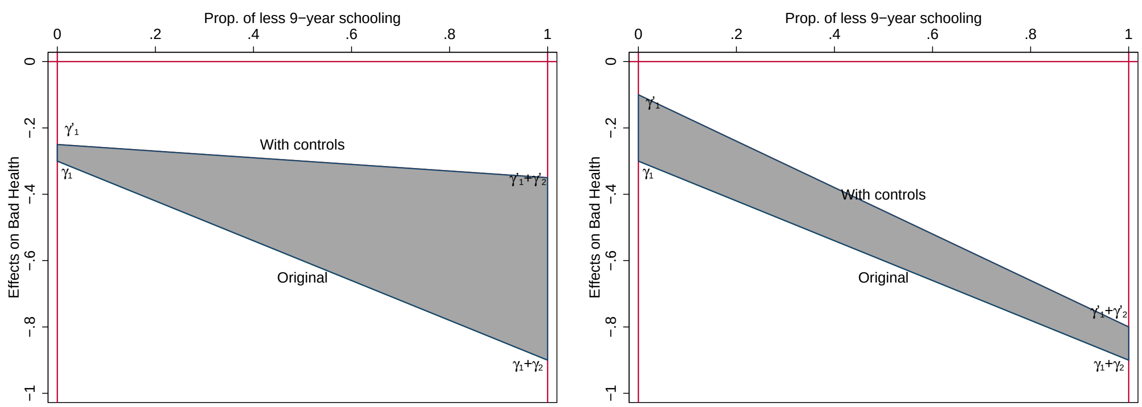
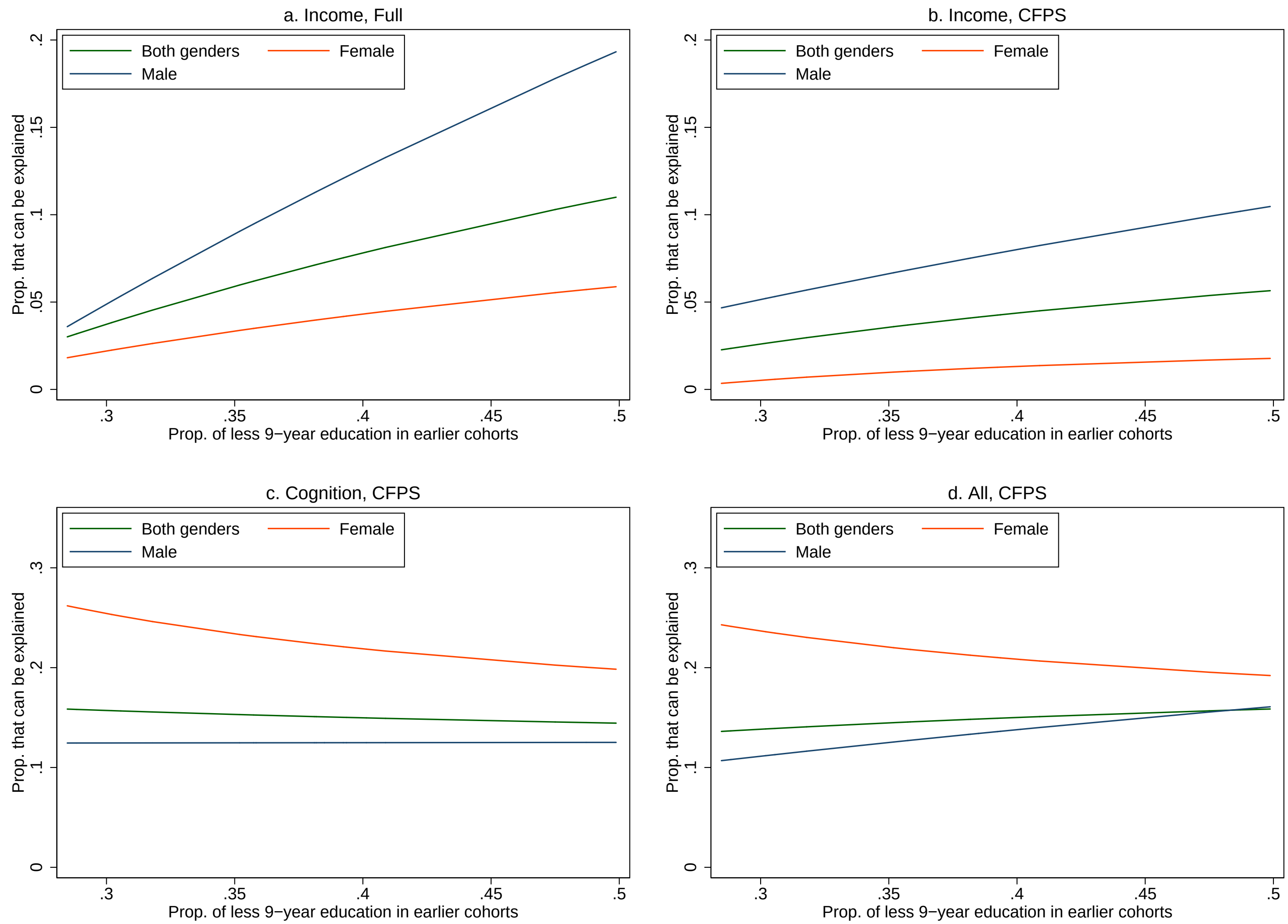


Figure 5: Explained Proportion of Education Effects, by Possible Pathway and Sample



Notes: Data source is CFPS, CHIPs and CHNS. For each specific intermediate variable and sample marked above or in the figure, I estimate the equation (5) and (5'). After estimating γ_1, γ_2 and γ'_1, γ'_2, I then plot $e(p) = 1 - |\frac{\gamma'_1 + \gamma'_2 p}{\gamma_1 + \gamma_2 p}|$ against p under each specific setting and for different samples.

Appendix Tables and Figures

Table A1. OLS Estimation for Impact of CSLs on Years of Schooling

	(1)	(2)	(3)	(4)
	Dependent variable is Years of Schooling			
	Subsamples by gender		Subsamples by Type of Hukou	
Sample	Male	Female	Urban	Rural
CSLs Eligibility	0.910**	1.229***	0.244	1.591***
	(0.416)	(0.469)	(0.497)	(0.341)
Pr(less than 9-year education)	3.173***	4.765***	2.033***	4.476***
* Eligibility	(0.699)	(0.769)	(0.781)	(0.651)
Observations	56,832	57,815	45,264	69,383
R-squared	0.201	0.288	0.196	0.265
F-statistic for all the variables	12.41	21.67	3.413	35.13
P-value for the F-test	0.000	0.000	0.038	0.000

Notes: Data source is CFPS, CHIPs and CHNS. Robust standard errors in parentheses are clustered at province-year of birth level. Covariates are the same with Table 1.

Table A2. Impact of Education on Health is Larger for the lower education group

VARIABLES	(1) Health Fair or Poor (Yes = 1)	(2) Underweight (Yes = 1)	(3) Smoker (Yes = 1)	(4) Words recall Z-score	(5) Math Ability Z-Score
Panel A: Years of Schooling <= 9 Sample					
Years of Schooling	-0.0107***	-0.00141***	0.00122*	0.111***	0.0786***
	(0.000706)	(0.000450)	(0.000658)	(0.00202)	(0.000937)
Observations	57,933	55,921	70,123	25,665	25,657
R-squared	0.113	0.045	0.395	0.301	0.764
Panel B: Years of Schooling > 9 Sample					
Years of Schooling	-0.00369**	0.000499	-0.0142***	0.0594***	0.0220***
	(0.00150)	(0.00120)	(0.00168)	(0.00432)	(0.00142)
Observations	31,038	29,354	35,511	9,334	9,328
R-squared	0.073	0.076	0.299	0.171	0.980

Notes: Data source is CFPS, CHIPs and CHNS. Robust standard errors in parentheses are clustered at province-year of birth level. Covariates are the same with Table 1. Panel A reports the results for the people with education no higher than nine-year schooling and Panel B for the people with over nine-year schooling.

Table A3. Impact of Education on Health, Robustness Checks

	(1)	(2)	(3)
Setting	Original	Drop CHNS sample	Health Excellent
Dependent variables	Health Fair or Poor (Yes = 1)	Health Fair or Poor (Yes = 1)	Health Excellent (Yes = 1)
Years of Schooling	-0.0204***	-0.0215***	0.0123*
	(0.00643)	(0.00630)	(0.00681)
Observations	88,971	69,042	88,971
First-stage F-statistics	27.24	33.54	27.24
Over-identification P-values	0.131	0.648	0.964

Table A4. Impact of Education on BMI Related Variables, Robustness Checks

	(1)	(2)	(3)	(4)	(5)
Settings	Original setting	Use Obese	BMI in the full sample	BMI < 22 sample	BMI >= 22 sample
Dependent variable	Underweight (Yes = 1)	Obese (Yes = 1)	BMI	BMI	BMI
Years of Schooling	-0.0118*	0.00112	0.132**	0.0615**	-0.0591
	(0.00626)	(0.00235)	(0.0634)	(0.0279)	(0.144)
Observations	85,275	85,275	85,275	41,246	44,029
First-stage F-statistics	28.09	28.09	28.09	45.91	5.725
Over-identification P-values	0.267	0.387	0.0724	0.218	0.0631

Notes: Data source is CFPS, CHIPs and CHNS. Robust standard errors in parentheses are clustered at province-year of birth level. Covariates are the same with Table 1.

Table A5. Impact of Education on Health, Health Behaviors and Cognition, Robustness checks

Dependent variables	(1) Health Fair or Poor (Yes = 1)	(2) Underweight (Yes = 1)	(3) Smoker (Yes = 1)	(4) Words recall Z-score	(5) Math Ability Z-Score
Panel A. 2SLS Results with weights					
Years of Schooling	-0.0172***	-0.0134**	-0.00708	0.153***	0.0757***
	(0.00583)	(0.00542)	(0.00647)	(0.0243)	(0.00944)
Observations	88,971	85,275	105,634	34,999	34,985
First-stage F-statistics	35.28	42.20	38.31	15.48	15.58
Panel B. 2SLS using completing junior high school as the key independent variable					
Junior High completion	-0.188***	-0.143***	-0.0707	1.607***	0.770***
(Yes = 1)	(0.0647)	(0.0536)	(0.0718)	(0.300)	(0.132)
Observations	88,971	85,275	105,634	34,999	34,985
First-stage F-statistics	31.67	49.20	32.63	17.76	17.75

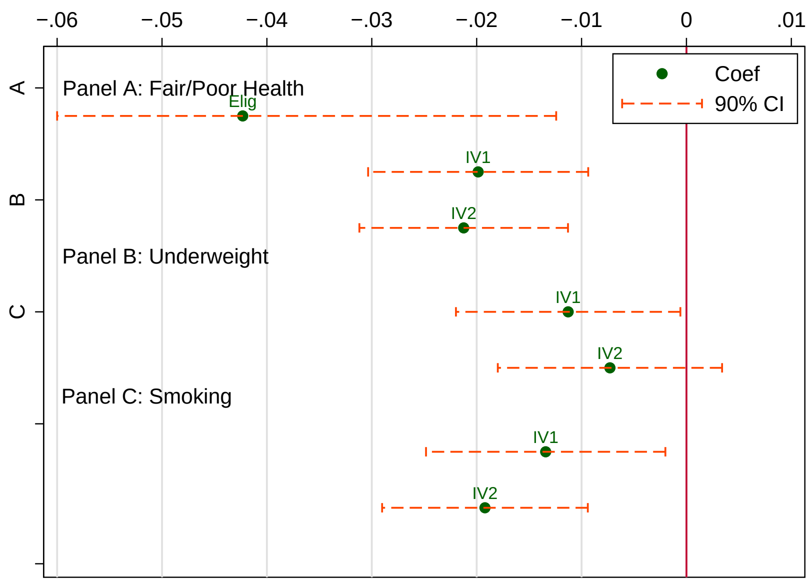
Notes: Data source is CFPS, CHIPs and CHNS. Robust standard errors in parentheses are clustered at province-year of birth level. Covariates are the same with Table 1.

Table A6. Effects of CSLs on Income

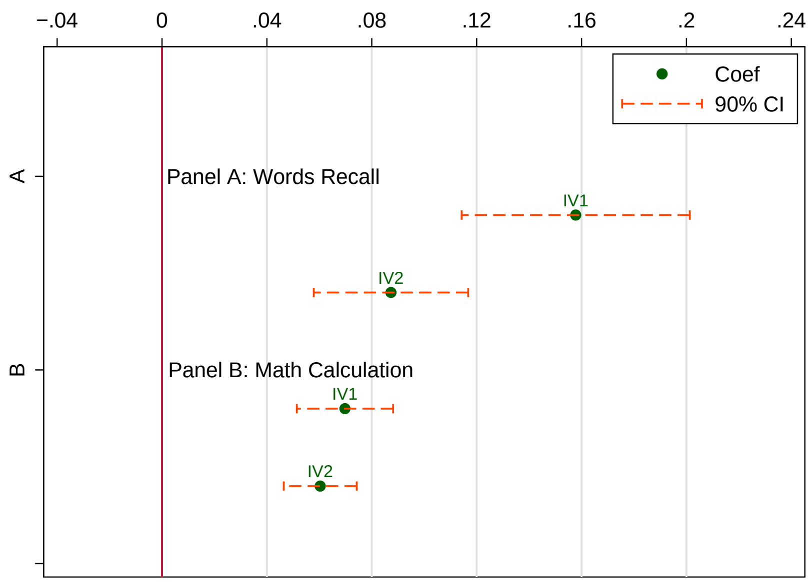
Dependent variables	(1) Log(Individual income) (Yes = 1)	(2) Log(Household income) (Yes = 1)
CSLs Eligibility	0.0642	0.170**
	(0.0941)	(0.0754)
Pr(less than 9-year education) * Eligibility	0.649***	1.080***
	(0.158)	(0.119)
Observations	64,589	87,774
R-squared	0.363	0.238

Notes: Data source is CFPS, CHIPs and CHNS. Robust standard errors in parentheses are clustered at province-year of birth level. Covariates are the same with Table 1.

Figure A1. Effects of Education on Health, by Different instruments
a. Effect of Education on Fair/Poor Health, Underweight and Smoking



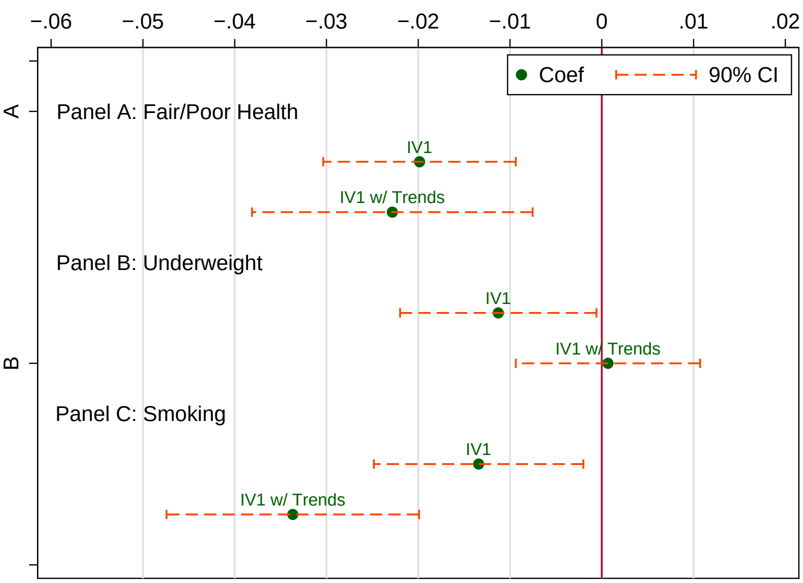
b. Effect of Education on Cognition



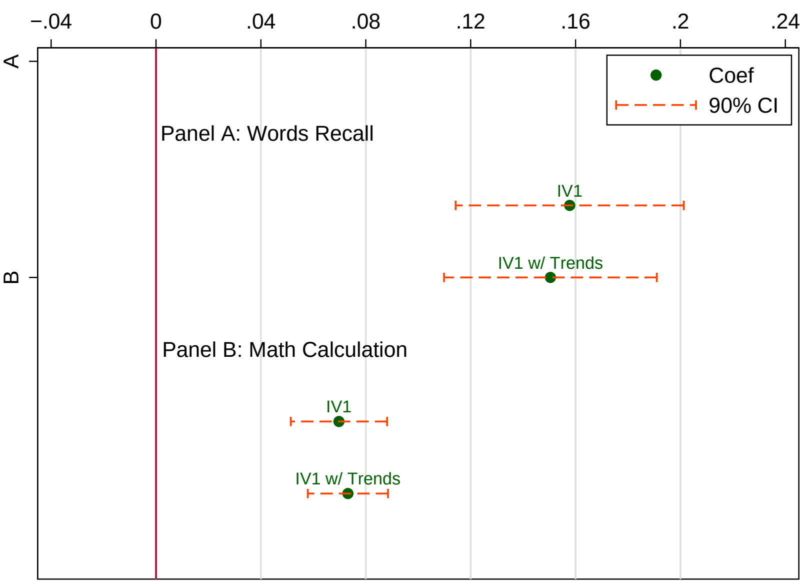
Notes: Data source is CFPS, CHIPs and CHNS. Two-Stage Least Squares estimation is conducted for each outcome using two sets of different instruments.
IV1: $Eligible_{ij}$ and $prop_j^{prior<9} \times Eligible_{ij}$;
IV2: $Eligible_{ij}$, $prop_j^{prior<9} \times Eligible_{ij}$ and $prop_j^{prior<9} square \times Eligible_{ij}$.

Figure A2. Effects of Education on Health, with Provincial-Cohort linear trends or not

a. Effect of Education on Fair/Poor Health, Underweight and Smoking



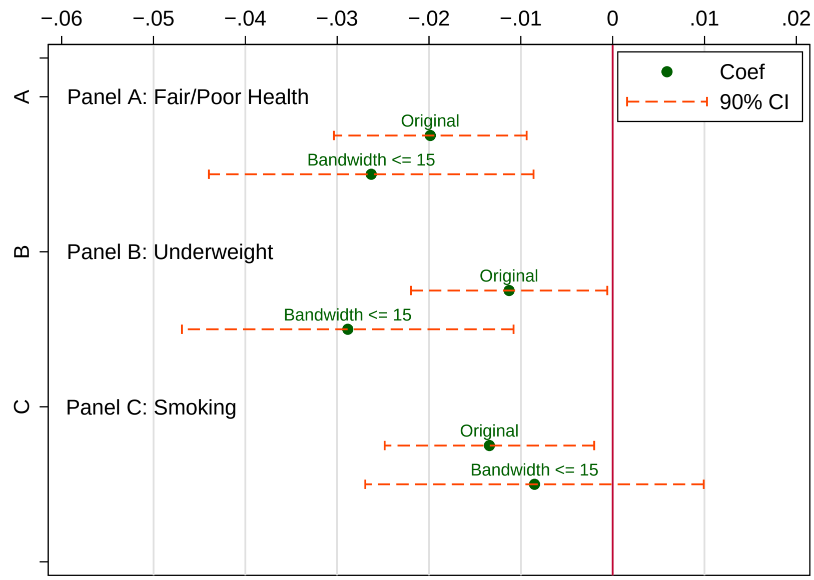
b. Effect of Education on Cognition



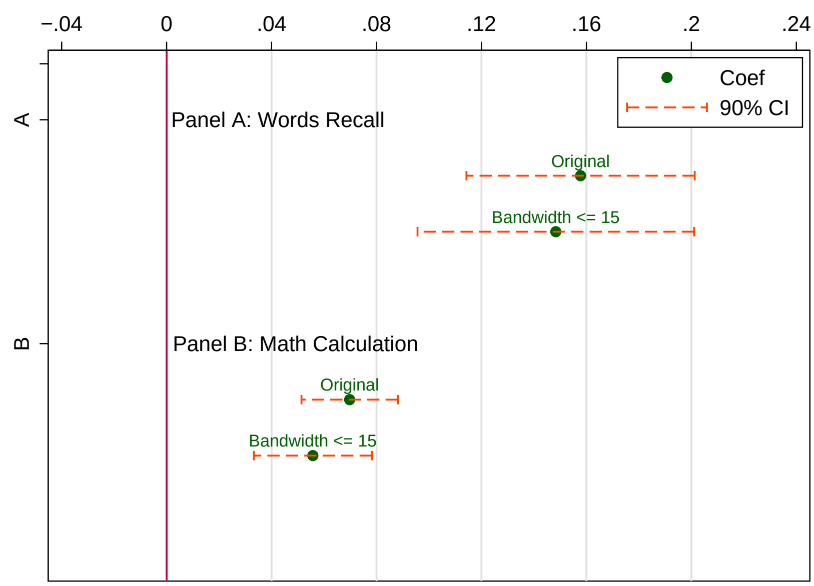
Notes: Data source is CFPS, CHIPs and CHNS. Two-Stage Least Squares estimation (Equation 4) is conducted the different settings. The results marked "IV1" are original 2SLS results using $Eligible_{ij}$ and $prop_j^{prior<9} \times Eligible_{ij}$ as instruments. The results with "w/ trends" are the 2SLS adding the provincial specific linear trends in birth cohorts.

Figure A3. Effects of Education on Health, in Full and Trimmed samples

a. Effect of Education on Fair/Poor Health, Underweight and Smoking



b. Effect of Education on Cognition



Notes: Data source is CFPS, CHIPs and CHNS. Two-Stage Least Squares estimation (Equation 4) is conducted the different settings. The results marked "IV1" are original 2SLS results using $Eligible_{ij}$ and $prop_j^{prior<9} \times Eligible_{ij}$ as instruments. The results with "Bandwidth $<=$ 15" are the 2SLS estimates using the sample between the birth cohorts 15 years earlier and later than the cohort just affected.