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

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Abstract

Using temporal and geographical variation in the implementation of compulsory schooling laws in China, I show that education significantly reduces the rates of reported fair or poor health, underweight, and smoking, and enhances cognition. Investigating the mechanisms finds that cognition and income only explain 15 percent and 7 percent of the effects on self-reported health. Spillovers from increased education of other people in the local region could explain over 25 percent. These findings present new evidence for the causal effects of education on health and help to reconcile the mixed findings in the literature. (*JEL* classification: I12, I21, I28)

Keywords: Education, Health, Mechanism

*Email: weihuang@fas.harvard.edu. I thank Amitabh Chandra, Raj Chetty, David Cutler, Richard Freeman, Edward Glaeser, Claudia Goldin, Nathan Hendren, Gordon Liu, Lawrence Katz and Adriana Lleras-Muney for their constructive comments and suggestions. I also thank the participants of Harvard China Seminar, Harvard Labor Lunch, North America China Economic Society Meeting and Seminars in Chinese Academy of Social Sciences, China Center for Economic Research and East China Normal University for their helpful suggestions. I am also grateful for the financial support from the Cheng Yan Family Research Grant from Department of Economics at Harvard and Jeanne Block Memorial Fun Award from IQSS. All errors are mine.

I. Introduction

The causal effects of education on health are of central interest to the economists. These effects 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 the extent to which education causally affects on health is essential to the formation and evaluation of education and health policies.

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 Royer (2013) used two education policy reforms in the United Kingdom and found no impact on mortality. The effects of education on mortality have also been found in the Netherlands (van Kippersluis et al., 2011) and Germany (Kemptner et al., 2011) but not in France (Albouy and Lequien, 2009) or Sweden (Lager and Torssander, 2012).[2] The inconsistent findings in the literature reflect scarce evidence on the mechanisms, which is largely due to data limitation. Since most education 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, the education reforms in Lleras-Muney (2005) happened between 1914 and 1939 and in most of the states the changes in minimum school-

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

[2] Some mixed findings are even found within the same country; Fletcher (2015) revisited the case for the United States and did not find evidence for causality on mortality.

leaving age were less than two years.[3] And the two reforms in Clark and Royer (2013) happened in 1947 and 1972, both increasing the minimum school-leaving age by only one year.

To shed light on the causal effects of education and the mixed findings in the literature, this study explores the compulsory schooling laws (CSLs) in China to investigate the causal effects of education on health and explores the possible mechanisms. The unprecedented nationwide education reform initiated in 1986 made nine-year schooling (i.e., up to the junior high school) compulsory and 16 years the minimum school-leaving age for all the regions in the largest developing country.[4] This education reform resulted in great achievements: the enrollment rate for junior high school increased by 26 percentage points, from 69.5 percent in 1986 to 95.5 percent 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 five years in the sample), I further explore the cross-sectional variation in the potential increase in education across the regions. Because *all* the provincial governments were required to enforce the "nine-year" compulsory schooling laws, the years of education in the provinces

[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.

[4] The surveys span from 1995 to 2012 and the CSLs started in 1986, so I keep the 1955-1993 birth cohorts and aged between 18 and 50 at the survey to conduct this study.

with more people with less than nine years of schooling before the enforcement of the law should potentially increase more after the law was enforced.[5] The estimates provide sound evidence for this. The CSLs significantly increased the schooling by 1.1 years on average (i.e., 12 percent of the mean value); the effect is 1.6 years in the regions with lower education before (lower than median) but is only 0.6 years for the rest (i.e., 19 percent of the mean value in the lower education regions and 7.1 percent of the mean in the higher education regions). Compared to the exogenous shocks in the previous literature, the effects of the CSLs in China are much larger in magnitude, both in absolute and relative scales.[6]

Since the identification is based on the different timing of the enforcement of the laws and the heterogenous effects across regions, there are some concerns about the identification. First, the potential cohort trends across the provinces caused by other factors, such as heterogeneous economic growth, may drive the estimates. I further control for province-specific birth cohort linear trends, and this yields fairly consistent results. Second, 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* the number of years of schooling is less than 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 probably because of lower

[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.

[6] For example, Clark and Royer (2013) found that the both education reforms in the UK increased years of schooling by 0.3-0.5 with mean values of years of education around 15-16. Thus, both reforms increased education by 1.9-3.3 percent.

marginal cost. I conduct a placebo test for the CSL-ineligible cohorts and find no evidence for this. Finally, greater increase in education in the regions probably reflects the larger improvement in nutrition, because these regions probably had poorer nutrition status in the beginning. But I find the policy has no effects on height, which is a widely used measure for nutrition status of younger adulthood (Thomas et al., 1991; Deaton, 2003).

The estimates from the reduced forms and the two-stage least squares (2SLS) both find pronounced effects of education on health outcomes. Specifically, the 2SLS estimates show that one additional year of schooling leads to 2-percentage points decrease in reporting fair/poor health (10 percent of the mean), 1.1-percentage points decrease in the rate of underweight (14 percent of the mean), and 1.3-percentage points decrease in the rate of smoking (5 percent of the mean).

Apart from the remarkable increase in education, another virtue of using the variations in the CSLs in China is that they happened much later (i.e., 1986-1991 in the sample) than the reforms examined in the literature. Thanks to the series of surveys conducted since the 1990s in China, I can use detailed individual information collected in the micro-level data sets to provide some *quantitative* evidence on several candidate mechanisms. For example, income is usually used as an explanation for the impact of education on health because richer people can afford healthier foods since higher education predicts higher income.[7] Another one is that education increases people's cognition, so that they are able to obtain more health knowledge and know how to take care of themselves better. The final one could be the exter-

[7]Higher incomes increase the demand for better health, but they affect health in other ways as well. For example, richer people can also afford more cigarettes; higher wage also means the higher opportunity cost of time: because many health inputs require time (such as exercise or doctor visits or cooking).

nalities or spillover effects of education. For example, increased education of the population over all by the CSLs would improve the health behaviors in general and generates better sanitary conditions, and thus lead to different health outcomes.

Therefore, I examine the above three mechanisms. The estimates show that income and cognition only explain a small proportion of the effects of CSLs on self-reported health; income explains 7 percent and cognition explains 15 percent. However, the empirical results suggest a more important role of the externalities of education, especially among those with lower education. Among those received no formal education, the empirical estimates also suggest a better health among those CSLs-eligible cohorts than that among the CSLs non-eligible cohorts. A conservative calculation suggest the externalities explain over 25 percent of the effects of the CSLs.[8] In addition, the roles of income, cognition, and externalities are different for different health measures. When underweight is the outcome, empirical results suggests a much more important role of income (i.e., income explains 20-30 percent of the effects of CSLs on underweight), but a less important role of spillover effect (i.e., the empirical estimates provide no evidence for this). For the smoking behaviors, however, spillover effect is a more important mechanism, while income and cognition together explain less than 10 percent.

The findings in this paper contribute to several strands of literature. First, the findings provide evidence of the effectiveness of education policies in improving education and health status, and build up the literature by studying causality between education and health for the working-age population in a developing coun-

[8]This is a little bit different from the "peer effects" documented in the literature (e.g., Jensen and Lleras-Muney, 2012). The externalities or spillover effects here emphasize that the people around have higher education caused by the CSLs would improve individual own health even though there is no increased in own education.

try. Second, the findings about BMI and cognition are consistent with the results in Cutler and Lleras-Muney (2012),[9] Aaronson and Mazumder (2011) and Carlsson et al. (2012).[10] Finally, this study fills a gap in the literature by examining the potential mechanisms through which education affects health, which helps to explain the large heterogeneity in the impact of education on health across different nations and in different periods.

II. Background and Data

2.1 Compulsory Schooling Laws in China

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 : 1) nine years of schooling became compulsory; 2) children were generally supposed to start their compulsory education at six years of age in principle, 3) compulsory education was free of charge in principle; 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 United States and European countries which

[9]First, the findings highlight the effects of education in a developing country: 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. 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.

[10]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 cognition 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.

increased the compulsory schooling by one or two years , the laws in China actually use the uniform "nine 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, and the gap between the earliest and latest provinces is only 5 years in our sample.[11] Therefore, I further explore the cross-sectional variations in the enforcement of the laws. The central government planned to have different levels of implementation across different regions because of large inequality in education levels across regions, and thus it decided to mainly support the less-developed regions. A government document, "Decisions about the Education 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 educational inequality across the nation. For example, the illiterate rate for those over age 15 years in rural areas declined by 25 percentage points, from 37.7 percent in 1982 to 11.6 percent in 2000, while that in urban areas only declined by 12 percentage points, from 17.6 percent to 5.2 percent in the same period (Yearbooks Population Survey, 1982 and 2000). Therefore, this study explores both the temporal and geographical variations in the enforcement of the law to identify the effects of education. Sections 3 and 4 provide empirical evidence.

The CSLs in China produced great achievements: the enrollment rate for junior high school increased by 26 percentage points, from 69.5 percent in 1986 to 95.5

[11]Note that our sample covered 26 provinces in China. The latest two provinces are Hainan and Tibet, whose CSLs starting year are 1992 and 1994. But these two are not covered in our sample.

percent in 2000, and the number of students enrolled in junior high school increased by 8.9 million. The CSLs made China the first and only country attaining the "nine-year compulsory schooling" goal among the nine largest developing countries.[12]

It was the first time for the largest developing country to enforce such compulsory schooling laws. It would be unrealistic to require those over age 10 years with no formal education but to complete the full nine-year compulsory schooling because they are legal to work at age 16. Those aged 12, for example, are required to go to school to receive education until they are reach age 16 years. They can stop their education legally and go to work because they are no longer age-eligible. Thus, the laws actually defined the age-eligible children as those between ages 6 and 15 years, and required the minimum school-leaving age to be 16 rather than truly "9-year" formal education, at least for the first few cohorts.

2.2. Data and Variables

The main sample used in this study is from the Chinese Family Panel Studies (CFPS), Chinese Household Income Project Series (CHIPs), and China Health and Nutrition Survey (CHNS), three ongoing and largest surveys in China. The Data Appendix provides a detailed description for each of them. I keep the variables consistently measured across the data sets, 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.[13]

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

[13]CHNS was collected in nine provinces and almost every two years since 1989: 1989, 1991, 1993, 1995, 1997, 2000, 2004, 2006, 2009, and 2011. The CHIPs and CFPS data are sampled

Because the CSLs were 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 almost as many affected as unaffected cohorts in the sample. Furthermore, I restrict the sample to individuals over age 18 years because most of the respondents have completed their education by then. For simplicity, I also drop those over age 50 years because all of them are ineligible to the CSLs and the mortality rate start to increase. I pooled the samples from three data sets together, and the total number of observations is more than 100,000, making it one of the largest micro-level samples to analyze the impact of education on health so far.[14] Table 1 reports the mean and standard deviation of the key variables used in the study.

[Table 1 about here]

Self-reported health and reported fair/poor health Previous literature suggests that self-reported health is highly predictive of mortality and other objective measures of health (Idler and Benyamini, 1997), and thus this study uses this measure as a major individual health outcome.[15] The measure of self-reported health is based on the answer to the question "How is your health in general?" in the three surveys, with the response ranging from 1 to 5: 1 for excellent, 2 very good, 3 good, 4 fair and, 5 poor. Indicator for reported fair or poor health is equal to one if the answer

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 the Data Appendix.

[14]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.

[15]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.

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 individuals' nutritional situation and has shown to be correlated with mortality and economic growth (Fogel, 1994; Cutler et al., 2003). All three surveys provide the information needed for calculating BMI,[16] and I define 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 percent and the obesity rate is only 2 percent,[17] indicating that the obesity problem seems not to be a big issue compared to the popular obesity in the developed areas like the United States and Europe.

Smoking Because of the high smoking rate in China and the close relationship between smoking and mortality (Wasserman et al., 1991; Cutler and Lleras-Muney, 2010), 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 smoke last week?" I then code the respondents as current smokers, which equals one if the answer to these questions is "yes," and zero if otherwise. The smoking rate is 26 percent for the full population and most of the smokers are men, whose smoking rate is higher than 50 percent, almost three times of that in the United States.

[16]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

[17]In the sample, 12 percent of the women are underweight, although this is not reported in this table.

Cognitive abilities Cognition refers to mental processes that involve several dimensions, including the thinking part of cognition, which 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). The CFPS measured cognitive abilities by two sets of tests. For the words recall test, interviewers read a list of 10 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 mentioned three nouns that were not in the list. The other test is about mathematical calculation ability: the respondents were asked to answer 8 or 10 math calculation questions and the test would also terminate if the respondents answered three questions in a row incorrectly. Because of different number of questions are used in the different survey years, I calculate the proportion of correct answers for each test and use the Z-score in each year as the cognition measures.

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

III. Graphical Analysis

Because the central government allowed the provincial governments to implement the policy separately, I collected the formal official documents in each province and report the initial year in which the CSLs were effective in each province in column 1 of Table 2, and report the first cohort affected in column 2.[18] Figures 1 a-f graphically show the CSLs enforcement across different provinces over time. Almost all the provinces enforced CSLs within the 1986-1991 period.[19]

An important feature of CSLs in China is the *uniform* nine-years compulsory schooling. I thus hypothesize that the increase in years of education in provinces with lower education prior to the CSLs be greater after the CSLs enforcement. So I first calculate the proportion of those with fewer than nine years education in the birth cohorts prior to the CSLs (within 15 years) in each province, as reported in column 3. It ranges from 0.05 for Beijing to 0.79 for Fujian and has a large variation, suggesting a large regional inequality in education in China before the enforcement of the CSLs. Figure 2a plots the values geographically.

[Table 2 and Figure 2 about here]

I divide the provinces by the median value of column 3 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, controlling for gender, *hukou* province, survey year, sample source (CHNS/CFPS/CHIPS) and all of their interactions. The the reference group is the *just-eligible* cohort (i.e., the birth cohorts aged 15 the CSLs became effective in the local province). Figure 2b reports the point estimates and the corresponding confidence intervals for each

[18] 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 year when the law became effective on the education level prior to the CSLs yields an insignificant (p-value = 0.27) though positive coefficient. In further analysis, this study also allows the provinces to determine endogenously when to start the CSLs, finding the results are also consistent. The results are available upon request.

[19] There are only two provinces in mainland China which did not start the CSLs in 1991, Hainan and Tibet. These two provinces are not surveyed in the three data sets.

birth cohort (i.e., from those born 4 years earlier than the reference cohort to those born 14 years later than the reference cohort). These birth cohorts cover those totally *non-eligible* ones (i.e., age sixteen years or older when CSLs enforcement), those *partially-eligible* ones (i.e., age between seven and fifteen years when CSLs enforcement), and those *fully-eligible* ones (i.e., age six years or younger when CSLs enforcement). Initially, there is more years of schooling among those non-eligible cohorts in higher-education regions. However, the difference is much narrowed among the partially-eligible cohorts, and is even reversed among the fully-eligible cohorts. The years of schooling in the low-education provinces increased about 1.6 on average, while that in in the high-education provinces only increased about 0.7.

Figure 2c reports the results of parallel analysis when the dependent variable is self-reported health (i.e., the value ranges from 1 to 5, and the higher value indicates unhealthier status). The figure shows that the relative levels and cohort trends in self-reported health (compared to the reference group in each sample) among non-eligible cohorts are similar in the two groups; however, self-reported health improved more from the non-eligible cohorts to the fully-eligible cohorts in the regions with lower education prior to the CSLs enforcement. Therefore, Figure 2b and 2c together provide some evidence for the causal effects of education on self-reported health. The following sections further provide further evidence by conducting regression analysis.

IV. First Stage: Impact of CSLs on Education

4.1. Econometric Methodology

I estimate the following equation to to test the hypothesis formally:

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

The subscripts i, j, b, and t denote the individual i, province j, birth cohort b, and survey year t, respectively. The dependent variable Edu_{ijbt} denotes years of schooling of individual i, and $Eligible_{bj}$ denotes the CSL-eligibility for birth cohort b in province j, which equals one if the individual is fully-eligible for the CSLs and equals zero if the individual is non-eligible. Then I assume the eligibility follows a linear function in between ages six and sixteen years. The results do not rely on the linear-function assumption. I also used a step function (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 education. But this may not be a first-order issue driving the results: the proportion of individuals whose *hukou* province is the same with their birth province is more than 93 percent for the same cohorts, according to the author's calculation based on the 2005 census.

X_{ijbt} denotes a set of control variables, including dummies for gender, type of *hukou* (urban/rural), married status (married or not), 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 three interactions. Adding δ_{sjt} into the equation controls for not only the potential systematic difference existing across data sets but also the different contemporaneous conditions in each province.

$prop_j^{prior<9}$ denotes the proportion of people with fewer than nine years schooling in the population born prior to the CSLs in province j (i.e., the value in column 3 in Table 2). Since the proportion varies at the province level, the main effect would be 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 the CSLs, and the differential increase in education after the CSLs between the provinces with lower and higher prior education. In practice, I interact the CSL-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 the CSLs will have a greater increase in years of education after the enforcement of CSLs.

4.2. Empirical Results

Table 3 reports the OLS estimation for α_1 and α_2, with the standard errors clustered at the province-year of birth level. Column 1 presents the results without the interaction term, showing that CSLs increase the years of schooling by 1.1 years on average. The 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 increase in years of education in regions with lower education before the CSLs (e.g. Fujian, Jiangxi and Gansu) would be 1.5 years more than the regions with higher education before the 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), who found the results in previous literature become insignificant and wrong-signed when region-specific linear trends are included. I thus control for province-specific birth cohort linear trends in column 3. The estimates show that the impact of the CSLs is robust to including these, suggesting that the other birth cohort linear trends across different regions should not be the first-order factors.

Appendix Table A1 further divides the sample by gender and hype of *hukou* to examine the heterogeneous impact of the 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*.

4.3. Evidence of Exogeneity of the CSLs

Evidence 1: Other Confounding Factors or Other Policies?

Comparison between before and after CSLs across the provinces captures the differential increase in years of education across the regions. However, the timing of the CSLs and the interaction may pick up variations in other policies, because China experienced a series of different reforms in the 1980s. But it seems to be unrealistic to list all contemporaneous policies in different regions during that period and test their correlation with the timing and enforcement of the CSLs. Instead, I directly test to what extent that CSLs increased the years of schooling . The education reform requires nine years of compulsory schooling for all the provinces. Therefore, the constructed variables based on the CSLs may increase the years of education up to and only up to nine years. However, there is no evidence that other confounding factors, such as local opinions toward education or other policies, would increase the years of schooling only up to nine 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 in equation (1). Because the effects of CSLs are depicted by the coefficients α_1 and α_2 together, I use the estimated coefficient in each regression to calculate the impact of CSLs on education at the mean level of prior education, and those at 10th and 90th percentile level of prior education. The points in Figure 3 reports the impact of CSLs on education when the prior education equals to the mean value of all the provinces. For each dependent variable, left end of the interval is the effect of CSLs when prior education is at the 10th percentile; while the right end indicates that when prior education is at the 90th percentile. The wider the intervals are, the larger heterogenous effects of CSLs have across the regions. When the years of schooling do not exceed the threshold of nine, the points are obviously positive and the range is wide. Once the years of schooling are greater than, however, the impact of the policy diminished dramatically both for the main effects (the points are much closer to zero and are not significant) and the heterogeneous effects across regions (the intervals are much narrower). These findings suggest that the positive association between education and the constructed variables in Table 3 should originate from the CSLs rather than from other unobserved factors like the implementation of other policies or reforms.

[Figure 3 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 CSL 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 year of implementation of the CSLs was five years earlier, 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 matter much in this analysis.

[Table 4 about here]

The second set of placebo tests are conducted to test whether the impacts of the CSLs reflect better nutrition in individuals in childhood or young adulthood. I use height as an independent variable since height is proved to be a good measure for health and nutritional status in childhood and young adulthood (Thomas et al., 1991; Deaton, 2003; Currie and Vogl, 2013). If the impact of the CSLs reflects the improvement in nutrition , the effects should be captured in height. The estimates in the last two columns of Table 4 provide no evidence of this.

V. Effects of Education on Health

5.1. Baseline Results

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

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

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 as those in equation (1). Panel A in 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]

Following previous literature, I conduct 2SLS estimation:

$$Health_i = \beta_0 + \beta_1 \widehat{Edu}_i + \beta X_i + \delta_{sjt} + \varepsilon_i \quad (3)$$

$\widehat{Edu}_i$ is the predicted education value of equation (1) and all the other variables are the same as those in equation (1). Panel B presents the results. Because of the 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 at the bottom of each column. The large F-statistics reject the null hypothesis and provide evidence of a significant first stage for all the columns. 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.

The 2SLS estimates are about three times larger in general. It is possible that the effects among the compliers (i.e., those with increased education under the CSLs and not without the laws) are larger because the effects identified from the 2SLS are local average treatment effects (LATE). Table A2 provides some evidence for this.[20] In addition, the OLS estimates may be biased to zero because of the classic measurement error in education, because the values were reported by the respondents themselves, and these reported values may be inaccurate.

The first column in Table 5 provides estimates for self-reported fair or poor health, indicating that an additional one year of schooling decreases the probability of reporting fair/poor health by 2 percentage points.[21] Since there were 19 percent of individuals in the sample reporting fair/poor health, the 2SLS estimates suggest one additional year of schooling reduce the reporting fair/poor health by 10 percent. Column 2 in Panel C shows that an additional year of schooling leads to a drop of about 1.2 percentage points in the underweight rate (14 percent of the mean), suggesting that education improves nutritional status.[22] Column 3 shows the effects

[20]The associations in the lower education group (less than nine 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. Hence, I divide the whole sample by whether the individuals completed nine years of education and conduct OLS estimation to investigate the associations of education with the health outcomes for each group. In general, the results in Appendix Table A2 provide consistent evidence for this. 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 a four-point scale and the other two used a five-point scale, I drop the CHNS sample and re-estimate the effects of schooling in column 2 in Appendix Table A3, which yields very consistent results. In the last column, I further examine 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.

[22]However, the results are different from the findings in developed regions like the United States and Europe. Both Kemptner et al. (2011) and Brunello et al. (2013) found that education has a negative effect of education on BMI. The estimates in the next three columns in Table A4 show that education in China increases BMI but the effects only exist in the sample with lower BMI, and do not provide evidence that education increases the rate of obesity in China. These findings suggest that schooling increases BMI in developing countries through decreasing the underweight proportion but decreases BMI in developed countries via reducing the obesity rate. This finding is consistent with Cutler and Lleras-Muney (2012).

of education on smoking. Consistent with the findings in Jensen and Lleras-Muney (2012), the 2SLS estimates suggest that an additional year of schooling reduces the likelihood of smoking by 1.3 percentage points (5 percent of the mean). The last two columns examine cognition. The estimates in the last two columns in Table 3 suggest that an additional year of schooling increases cognition by 0.09 standard deviation for word recall and 0.16 for math calculation.[23]

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

$$Health_i = \lambda_0 + \lambda_1 Eligible_{bj} + \lambda_2 prop_j^{prior<9} \times Eligible_{bj} + \lambda X_i + \delta_{sjt} + \epsilon_i \quad (4)$$

Since both $Eligible_{bj}$ and $prop_j^{prior<9} \times Eligible_{bj}$ predict higher education, the signs of the coefficients in the reduced form estimations should be negative for poor health and positive for better health. The estimates in Panel B provide consistent evidence of this.

The difference between the reduced form and 2SLS estimates is noteworthy. The 2SLS estimates are based on the exogeneity of the CSLs and estimates the effects of education on health among compliers. However, the 2SLS estimates do not consider the spillover effects or externalities of education. The reduced form estimates, however, estimate the effects of CSLs implementation on health outcomes *directly*, and thus the effects of individual education and effects of the *average* education of the population are mixed together.

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

5.2. Robustness Checks

Considering that health and behaviors may be different in men and women because of biological and cultural reasons, I conduct gender-specific reduced form and 2SLS estimation, and then report the results in Table A5 and Figure A1, respectively. In general, the results provide evidence for the effects of CSLs or education on self-reported health and cognition for both genders. But the effects on underweight are significant only for women and those on smoking are significant only for men. It makes sense in China because women has a much higher underweight rate (the underweight is 12 percent for women but is less than 3 percent for men) while men has a much higher smoking rate (the smoking rate for men is over 50 percent but for women is less than 3 percent).

Since the CHNS was collected from nine provinces and combining the three samples together might put disproportionate weights on these provinces, I weight the regressions in Panel A in Appendix Table A6 by the population of the province divided by the number of observations, 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 A6. The results are also consistent.

Figures A2 presents the original estimates and the ones including province specific linear trends. The figure shows that adding trends does not influence the estimates of the effects on self-reported health and cognition.[24] Another concern about

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

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 just-eligible birth cohort. The estimates are reported in Figure A2, showing a fairly consistent pattern in the trimmed sample.

VI. Understanding the Effects of Education on Health

As suggested in Cutler and Lleras-Muney (2012), studies of the effect of education on health will need to understand the pathways that link the two because this would improve our understanding of the education-health link substantially. On one hand, the evidence on mechanisms is somewhat weaker than the evidence on causality, since researchers often have to make assumptions about what constitutes a mechanism, which partly due to the data limitation. On the other hand, the mixed findings in the literature call for studies to investigate the mechanisms through which education affects health. This section aims to shed some light on this issue.

Theoretical foundations for a causal effect of education on health were first provided by the seminal work of Grossman (1972). Current studies such as Cutler and Lleras-Muney (2012) provide some potential mechanism candidates.[25] Due to data limitation, this study examines three possible pathways, including income, cognition and spillover effects. The first two are intermediate variables at individual

[25]Cutler and Lleras-Muney (2012) classified the pathways of the effect of education on health into four categories. First one is labor market outcomes since higher education yields higher income and safer occupation etc. Second one is the "technology" parameter, such as better use of information. Third one is that education could change the 'taste' for a longer, healthier life, (i.e., the utility function could be changed). Final one is peer effects, which means that people with higher education would be more connected to those with higher education and thus are more likely to develop better health behaviors and have better health.

level. Since higher education predicts higher income as Table A7 suggests, this allows people with higher education can have a higher quality, such as living in a house in a safer region and with better environment or having less financial pressure, etc. Higher cognition induced by higher education, as shown above, helps people to get useful information more efficiently and make wiser and more rational choices like choosing proper food, taking drugs in the right way if necessary, evaluating the potential risks in life, and avoiding the potential danger, etc. I also investigate the spillover effects or externalities of education (Borjas, 1995; Ludwig et al., 2012; Wantchekon et al., 2015).[26] For example, increase in education could decrease the smoking rate overall, which would in turn increase the indoor air quality and improve sanitary conditions. In addition, it is also possible that those without any formal education may follow the others with higher education, and they are likely to get more useful suggestions when asking other people around.[27]

6.1. Income and Cognition as Mechanisms

To quantify the possible mechanisms, I follow Cutler and Lleras-Muney (2010) and estimate the following two equations:

[26] However, the literature does not reach a consensus about the peer effect or the externalities of human capital, which partly depends on what the outcome is. For example, Borjas (1995) found the average skills of the ethnic group in the parent's generation had some effects on the individual skills; Ludwig et al. (2012) found moving to a better neighborhood leads to long-term (10- to 15-year) improvements in adult physical and mental health and subjective well-being. However, Ciccone and Peri (2006) and Acemoglu and Angrist (2001) do not find evidence for externalities for human capital on individual return.

[27] It should be noted that the spillover or externalities here are similar to the "peer effects" documented in the literature such as Jensen and Lleras-Muney (2012) because both of them refer to the effects from people around. But they are different: the peer effects of education usually mean that people with higher education would be more connected to those with higher education and thus are more likely to develop better health behaviors and have better health. But the externalities or spillover effects here emphasize that the people around have higher education caused by the CSLs would improve individual own health even though there is no increased in own education.

$$Health_i = \gamma_0 + \gamma_1 Eligible_{bj} + \gamma X_i + \delta_{sjt} + \epsilon_i \quad (5)$$

$$Health_i = \gamma_0' + \gamma_1' Eligible_{bj} + \gamma' X_i + Z_i + \delta_{sjt} + \epsilon_i \quad (5')$$

the dependent variable $Health_i$ is the main health outcome, which can be reported fair/poor health, underweight and smoking. All the other variables have the same definition as those in equation (2). I only use $Eligible_{bj}$ directly here because it captures the average effects of CSLs on the health and thus include both the direct effect of increased own education and the indirect effect of increased education of others in the local region. The estimated effects of CSLs have taken into account of the potential spillover effects. Z_i denotes the potential intermediate variables (i.e., income, cognition or both). Following the methodology in Cutler and Lleras-Muney (2010), I interrupt the change in the magnitude of coefficient on $Eligible_{bj}$ as the part that can be explained by the intermediate variable Z_i (i.e., the explained proportion equals $1 - |\frac{\gamma_1'}{\gamma_1}|$).

Panel A in Table 6 reports the results for the proportions explained by the possible intermediate variables when the dependent variable is self-reported fair/poor health. I conduct the analysis by gender with consideration that the effects may differ in between; since only CFPS data measure cognition, I also conduct a parallel analysis for the full and CFPS samples separately. Column 1 reports the original effects of the CSLs. Column 2 reports the conditional effects when income is controlled for and column 3 reports the corresponding proportion that can be explained by income.[28] The part that can be explained by income is 9.9 percent for men and 3.6 percent for women in the full sample, and 7.1 percent for men and 1.2 percent

[28] Income here includes both individual income and household income. Table A6 in the appendix shows that the CSLs also increased both.

for women in the CFPS sample. One possible reason why the estimates with the CFPS data are smaller is the survey years of the CFPS data are 2010 and 2012, the latest two years in the full sample, when the households and individuals had higher income in general. In addition, the part can be explained by income is consistently larger for men for both samples.

[Table 6 about here]

Consistent results of two samples in the first few columns suggest the feasibility of using CFPS data to calculate the part explained by cognition. Column 6 reports the *conditional* effects when only cognition measured by word recall and math calculation is controlled for, and column 7 reports the reduction of magnitude in percent. The proportion that can be explained by cognition is 12.5 percent for men to 23.0 percent for women, implying that cognition is a more important channel among women. In addition, the part that can be explained by cognition is larger than that by income, suggesting that cognition is the most important intermediate variable examined here. These findings are also consistent with the literature that highlights the importance of cognition (e.g., Hanushek and Woessmann (2008),Aaronson and Mazumder (2011) and Carlsson et al. (2012)).

Panel B and Panel C reports the results for underweight among women and smoking among men, respectively. I only keep men or women for these specific outcomes because of no significant effect of CSLs on underweight among men and on smoking among women as shown in Table A5 and Figure A1. The results show that income is an important mechanism to explain the effects of education on underweight since it explains 20-30 percent. But cognition is not since it only explains

7 percent. For the smoking behaviors among men, both income and cognition only explain a small proportion.

Appendix B takes into account of the differential effects of CSLs across the regions by adding the interaction between education level prior to the CSLs and CSL-eligibility, which yields very consistent results reported in Table A8.

6.2. Spillover Effects or Externality of Education on Health

The above analysis suggests a small proportion of the effects of education on self-reported health and smoking that can be explained by the individual intermediates such as income and cognition. For self-reported health, around 80 percent of the effects cannot be explained. The natural question is what is the most important factor that may explain the effects of education. As mentioned above, the potential spillover effects may be an important candidate. To provide some evidence of the externalities, I first use the sample composed of those with all education levels, and conduct a reduced form estimation (i.e., equation 5) to quantify the effect of CSLs-eligibility on self-reported health in Panel A. The estimates in all the columns show that CSL-eligibility improves health.

[Table 7 about here]

Then I restrict to the sample to those without any formal education to conduct the same regression in Panel B.[29] Because the education is unchanged for those

[29] Age-eligible children may not go to school due to several reasons. First, primary schools in the local regions may not have been built up yet because it takes time to catch up. Second, in some remote villages, the children may not go to school and the punishment of the laws cannot be enforced because the administrative department may not even have the case because most of the administrative departments were located in urban regions. Third, the CSLs cut the tuition but not abandon the fee. Many primary schools still collect different kinds of fees and there are some poor people still not going to school due to the cost.

receiving no formal education, if the individuals without formal education before and after CSLs are comparable, the estimated effects would be only caused by the externalities or education of others. But the condition may not hold because those who had no formal education after CSLs may be more adversely selected. In this case, however, the spillover effects are expected to be underestimated. If CSLs-eligibility is associated with better health in this specific group, it would provide some evidence for spillover effect; if not, it does not mean that there is no spillover effect at all. The estimates here present some evidence for spillover effects for self-reported health and smoking, but not for underweight. Specifically, among those without formal education, the CSLs fully-eligible cohorts have better self-reported health and lower smoking rate, and the magnitude is even two to three times larger than the average effects reported in Panel A.

[Figure 4 about here]

In Panel C, I conduct the parallel analysis for the sample of those with more than nine-years schooling because Figure 3 implies that CSLs did not affect the received education among them. The results show that CSLs do not have any significant effects on health among these people, suggesting little spillover or external effects of CSLs for them. Therefore, these findings suggest that these results provide some evidence of externalities of education, but the externalities mainly exist for those with lower education.

The proportion which can be explained by the externalities would be quantitatively important. However, it is really difficult to accurately estimate this proportion without introducing any additional assumptions. But the above estimates enables a back-of-the-envelope calculation which only takes into account of the spill-over effects among those without any formal education. For example, take the self-reported health as an example. Suppose the estimated coefficients are estimated spill-over effects, and only consider those without any formal education, then my calculation suggests that the proportion could be over 27 percent in full sample, and 36 percent and 22 percent for men and women, respectively. The suggestive evidence shows that the large increase in education caused by the CSLs may have large spillover effect on self-reported health among the population, especially those with lower education. Based on the conservative estimates, the explained proportion is fairly high compared to that explained by the individual intermediates.

VII. Conclusions and Discussion

It is important to know whether and why education has a causal impact on health. However, the controversial discussion in the literatures has not come to a consensus that education improves individual health, but reveals the heterogeneity in the effects of education across different countries. This paper uses the exogenous temporal and geographical variation in the establishment of CSLs in China around 1986 to identify the effects of schooling on a series of health outcomes and shed some light on the possible mechanisms.

First stage results suggest that the CSLs significantly increased the education by 1.1 years in China on average. Because of the uniformly "nine-year" compulsory schooling years across all the regions, the results also suggest the policy-included increase in education is significantly larger in the regions with lower education prior to the CSLs were enforced. These variations caused by the CSLs provide valid estimates for the effects of CSLs on health outcomes. In the next, both the reduced form and 2SLS estimates provide sound evidence for the improved health status by the CSLs and the induced higher education. Specifically, the 2SLS estimates show that one additional year of schooling leads to 2-percentage points decrease in reporting fair/poor health (10 percent of the mean), 1.1-percentage points decrease in the rate of underweight (14 percent of the mean), and 1.3-percentage points decrease in the rate of smoking (5 percent of the mean).

The next part of this study aims to unravel the potential mechanisms. I use the framework in Cutler and Lleras-Muney (2010) and examine the potential roles of income, cognition and externalities in effects of education on health. The estimates suggest that income and cognition explain the impact of education on self-reported health by 7 percent and 15 percent, separately. These results suggest helping people to obtain knowledge about health is even more important for health than income. However, the empirical results suggest a more important role of the externalities of education in the effects of education on self-reported health, especially among those with lower education; a conservative calculation suggests the externalities explain over 25 percent. However, the results are different for various dependent variables. For example, income explains the effects on underweight by over 20-30 percent but only explains 5 percent of the effects on smoking. The results also suggest externalities may be important to explain the effects on smoking while hardly explain the effects on underweight.

Although this study provides some suggestive evidence on a couple of mechanisms, it is far from satisfactory. For one thing, it is still a question how much the spillover can explain the effects of education exactly. Further, it is also possible that the heterogeneity in mechanisms exists in different countries and in different periods. Due to data limitations, I leave these questions to future studies that will help us to gain a better understanding of the effects of education on health.

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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	22.5	3.18	12.1	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	32.5	9.16	18	50
Urban	114,647	0.39	0.49	0	1
Married	114,647	0.54	0.55	0	1

Notes: Data source is CFPS, CHIPs and CHNS. The variables are measured consistently across the data sets. 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 by Province

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 are from the education yearbooks for each province.

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

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

Notes: Data source is CFPS, CHIPs and CHNS. Robust standard errors in parentheses are clustered at the 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 de-meaned 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 the province-year of birth level. Covariates and variable definitions are the same as those in Table 3.

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. 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
Panel C. 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	-0.0759**	-0.0693**	-0.0123	0.335***	0.103
education) * Eligibility	(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

Notes: Data source is CFPS, CHIPs and CHNS. Robust standard errors in parentheses are clustered at the province-year of birth level. Covariates and variable definitions are the same as those in Table 3.

Table 6: The Role of Income and Cognition in Effects of Education on Health outcomes

Sample	(1) Original ave. effect	(2) Income controlled: Ave. effect	(3) Income controlled: Explained (%)	(4) Cognition controlled: Ave. effect	(5) Cognition controlled: Explained (%)	(6) Both controlled for: Ave. effect	(7) Both controlled for: Explained (%)
Panel A: Reported Fair/Poor Health							
Both genders in full sample	-0.061	-0.057	6.06				
Men in full sample	-0.048	-0.043	9.87				
Women in full sample	-0.074	-0.072	3.59				
Both genders in CFPS	-0.057	-0.055	3.93	-0.048	16.4	-0.048	16.4
Men in CFPS	-0.068	-0.063	7.10	-0.059	12.5	-0.057	15.8
Women in CFPS	-0.049	-0.049	1.22	-0.038	23.0	-0.039	20.1
Panel B: Underweight							
Women in full sample	-0.011	-0.007	30.7				
Women in CFPS	-0.018	-0.014	20.0	-0.017	7.49	-0.013	25.2
Panel C: Smoking							
Men in full sample	-0.070	-0.066	5.13				
Men in CFPS	-0.199	-0.198	0.86	-0.186	6.67	-0.185	6.96

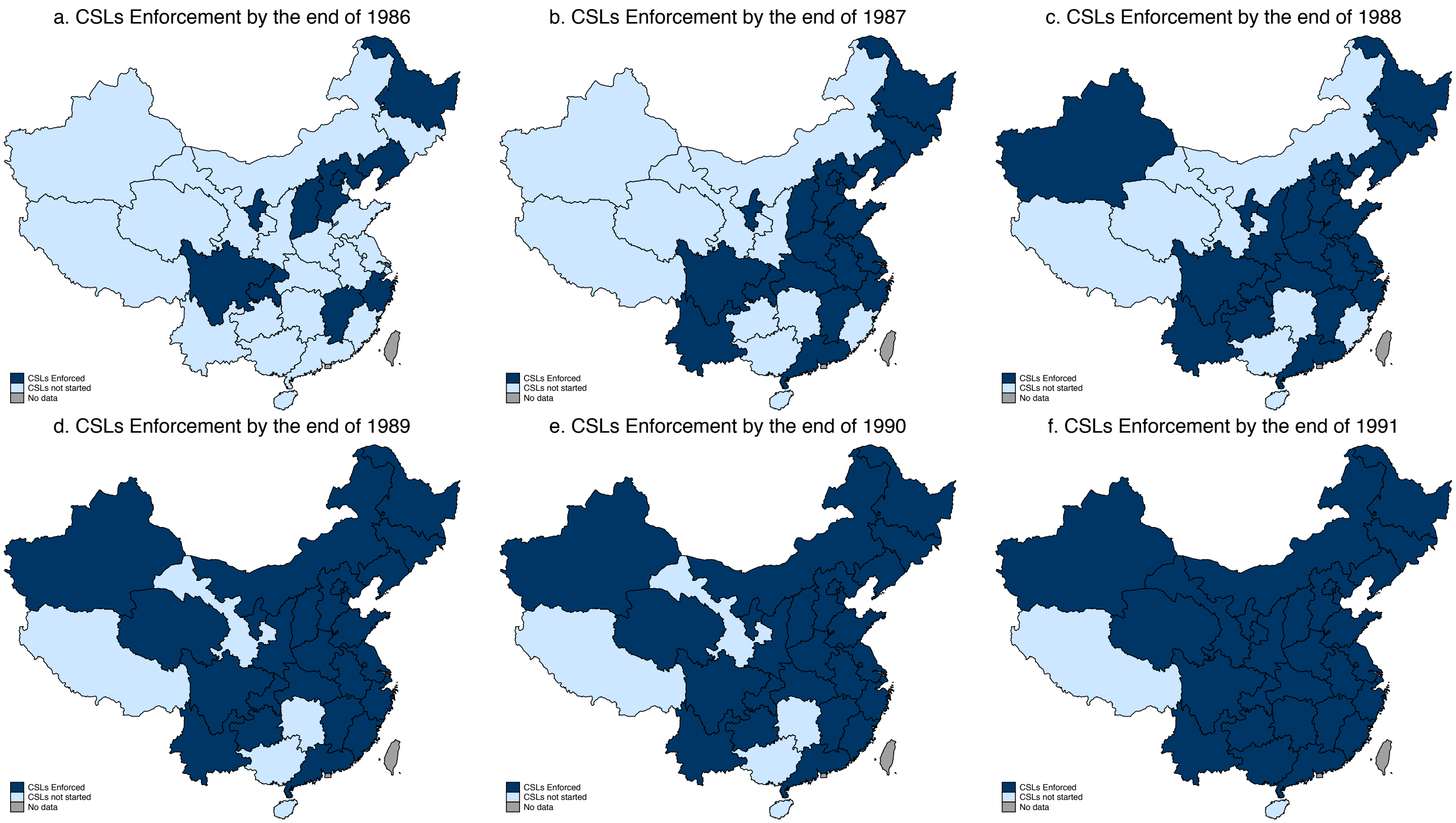
Notes: Data source is CFPS, CHIPs and CHNS. The original average effect is estimated γ_1 in equation (5). The average effect when controlling for the specific intermediate variable is estimated γ_1' in equation (5'). The corresponding explained proportion is $1 - |\frac{\gamma_1'}{\gamma_1}|$. Because the effects of CSLs on underweight and smoking are only identified among women and men, respectively, this table only examines the corresponding subsample.

Table 7: Spillover effects of CSLs on Self-reported Health, Underweight and Smoking

	(1)	(2)	(3)	(4)	(5)
Dependent variable	Self-reported fair/poor health (Yes = 1)			Underweight (Yes = 1)	Smoking (Yes = 1)
Samples	Full	Male	Female	Female	Male
Panel A: Full sample					
CSLs Eligibility	-0.0607***	-0.0483*	-0.0743**	-0.0106	-0.0698*
	(0.0219)	(0.0263)	(0.0324)	(0.0298)	(0.0391)
Observations	88,971	43,929	45,042	43,516	56,832
R-squared	0.092	0.074	0.104	0.062	0.133
Panel B: People without any formal education					
CSLs Eligibility	-0.167**	-0.275**	-0.110	0.0377	-0.221*
	(0.0705)	(0.124)	(0.0812)	(0.0655)	(0.127)
Observations	8,563	2,901	5,662	5,374	2,962
R-squared	0.120	0.147	0.124	0.080	0.262
Panel C: People with more than nine-year schooling					
CSLs Eligibility	0.0106	0.0551	-0.0431	-0.00442	0.00552
	(0.0360)	(0.0411)	(0.0595)	(0.0612)	(0.0656)
Observations	31,038	16,375	14,663	13,933	21,746
R-squared	0.075	0.069	0.089	0.078	0.165

Notes: Data source is CFPS, CHIPs and CHNS. Robust standard errors in parentheses are clustered at the province-year of birth level. Covariates and variable definitions are the same as those in Table 3. Because the effects of CSLs on underweight and smoking are only identified among women and men, respectively, this table thus examines the potential spillover effects only for men when the dependent variable is smoking and only for women when dependent variable is underweight.

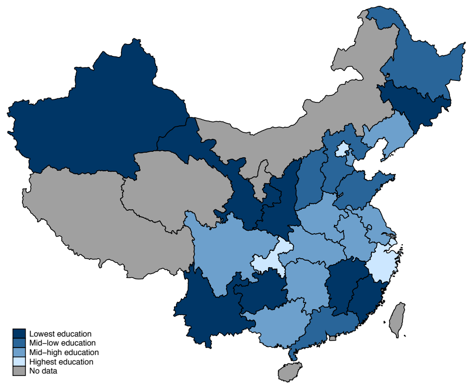
Figure 1: CSLs Enforcement in Different Provinces over Time



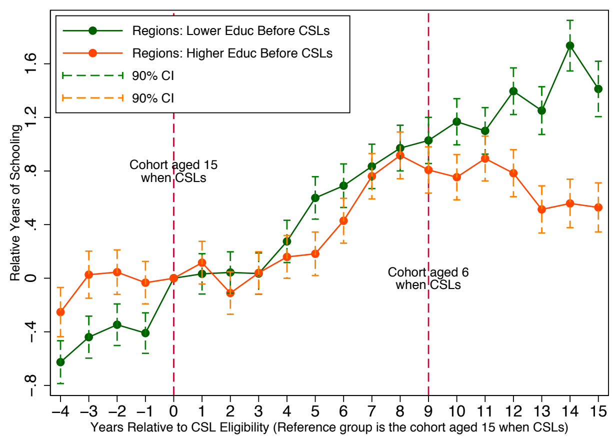
Notes: Data source is the education year books for each province. Every figure shows the CSLs enforcement across China at the end of each corresponding year. Two regions not starting CSLs in 1991 are Hainan and Tibet, which are not included in the sample. The data on Taiwan are missing.

Figure 2: Lower Prior Education, More Improvement in Education and Health after CSLs

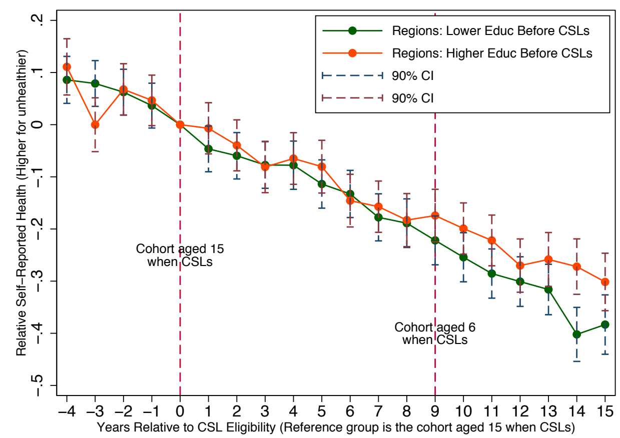
(a) Geographical Distribution of Education Levels before the Laws



(b) Increased Education over Birth Cohorts, by Local Education Level among Earlier Cohorts

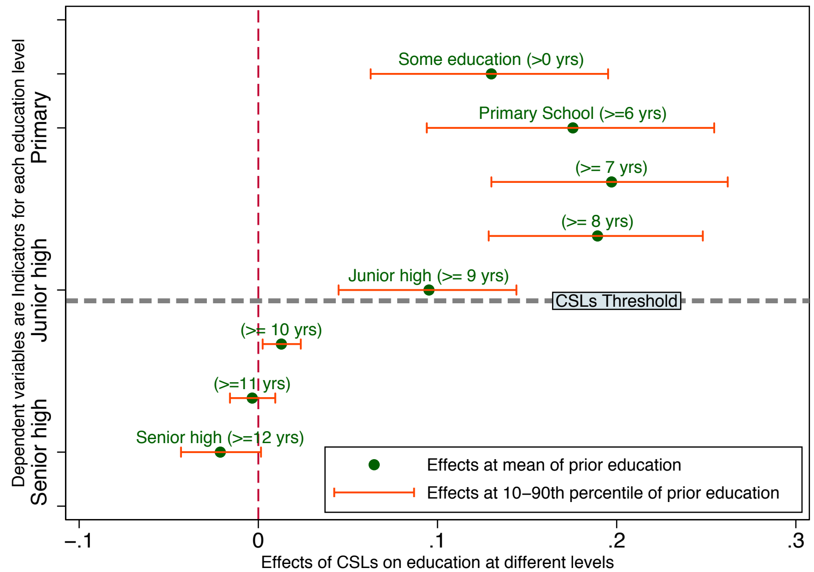


(c) Improvement in Health over Birth Cohorts, by Local Education Level among Earlier Cohorts



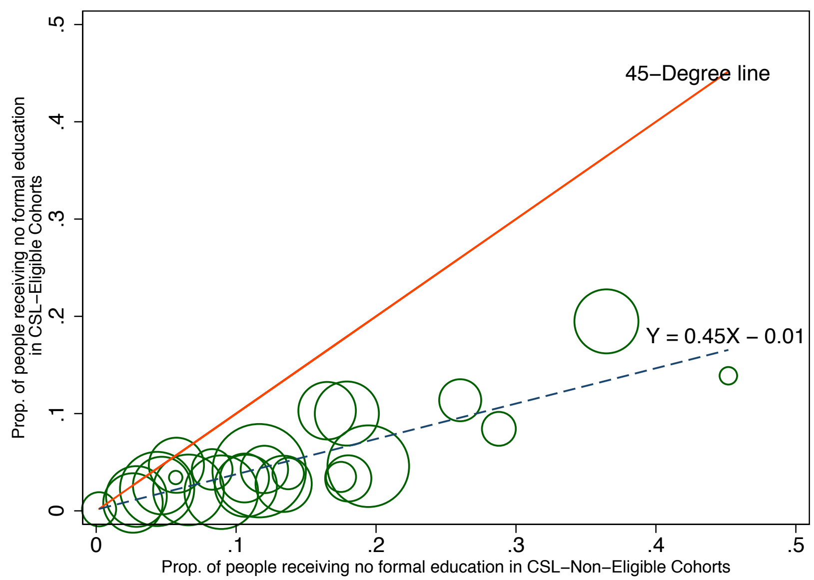
Note: Data source is CFPS, CHIPs and CHNS. Figure 2a categorizes the values in column 3 of Table 2 into four groups and plotted them geographically. For Figures 2b and 2c, I divide the sample by the median value of the proportion of people with less than 9-year education prior to the CSLs, then conduct regressions to estimate how the years of schooling or self-reported health change over birth cohorts relative to CSLs eligibility for each subsample, controlling for gender and dummies for *hukou* province, survey year, sample and all of their interactions. The reference group is the *just-eligible* cohort for the CSLs for each subsample.

Figure 3: Impact of CSLs on Years of Schooling at Different Education Levels



Notes: Data source is CFPS, CHIPs and CHNS. Each row reports a specific 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 the 10th to 90th percentile of the prior education level based on the OLS estimates.

Figure 4: Proportion of Individuals receiving no formal education
CSL-eligible cohorts v.s. CSL-non-eligible cohorts



Notes: Data source is CFPS, CHIPs and CHNS. The proportions of people receiving no formal education are calculated respectively for the CSL-non-eligible and CSL-eligible cohorts within each province. The figure plots the proportions among non-eligible cohorts (X-axis) against those among eligible cohorts in the same province (Y-axis). The size of the circles reflects the sample size.