Education on the Cheap: The Long-run Effects of a Free Compulsory Education Reform in Rural China*

Yun Xiao†[1], Li Li‡[2] and Liqiu Zhao§[3]

[1]Lee Kuan Yew School of Public Policy, National University of Singapore, Singapore 259772

[2]The Institute of Water Policy, Lee Kuan Yew School of Public Policy, National University of Singapore, Singapore 259772

[3]School of Labor and Human Resources, Renmin University of China, China 100872

Abstract

This paper evaluates the long-run effects of a free compulsory education reform in rural China on individuals' educational attainment, cognitive achievement and health. We exploit the cross-province variation in the roll-out of the reform and apply a difference-in-difference strategy to identify the causal effects of the reform on the outcomes. We find that the reform exposure, measured by the number of semesters that an individual is supposed to be enrolled in compulsory education for free, is positively associated with individuals' educational attainment and cognitive achievement measured by math test scores in early adulthood. The reform effects become greater in the longer term. Moreover, the reform effect on educational attainment is stronger for individuals with less-educated fathers.

Keywords: Free compulsory education reform; Human capital; Long-run effects; Rural China

---

*We would like to thank the helpful comments from Fei Wang, Zhong Zhao and seminar participants at Renmin University of China. Liqiu Zhao acknowledges financial support from the Natural Science Foundation of China (grant no. 71403286) and the Center for Labor Economics and Public Policy at Zhejiang University. The remaining errors are all ours.

†E-mail address: sppxiao@nus.edu.sg

‡E-mail address: sppll@nus.edu.sg

§Corresponding author. Tel.: +86 10 82502209; Fax: +86 10 62513427. E-mail address: liqiuzh@gmail.com (L. Zhao)

1 Introduction

Over the past decades, developing countries have made considerable progress in increasing the school enrollment and achievements of children and youth. School subsidy programs, in the forms of providing conditional cash transfers, educational vouchers or subsidies for other educational costs, are widely recognized as effective ways to increase enrollment and reduce dropout rates. A large number of studies have evaluated the effectiveness of these school subsidy programs, the majority of which assess the effects on outcomes measured in the short run (see Behrman (2010), Glewwe and Kremer (2006) and Orazem and King (2007) for extensive reviews of this literature). However, the evaluations of the long-term program effects, whose importance is emphasized in Behrman et al. (2011), are rather limited.[1]

China, the largest developing country in the world with approximately half of the population living in less developed rural areas, launched a free compulsory education reform in rural areas in 2006 to reduce the costs of compulsory education for rural households and increase enrollment. Under the reform, all rural students that are enrolled in primary or junior high schools are exempted from paying tuition and miscellaneous fees. The free compulsory education reform is essentially a school subsidy program, where eligible students are provided with a school subsidy that covers the tuition and miscellaneous fees. Like the other school subsidy programs, the reform has been found to raise short-run school enrollment rates, especially at junior high school level (Chyi and Zhou, 2014; Shi, 2016). However, the magnitude of the effect is small, which may be attributable to the already high enrollment rates of the compulsory education in rural China prior to the reform (see Figure 1) (Shi, 2016). Some of the most important impacts of the education reform, including the long-run impacts on completed years of schooling and labor market outcomes, has yet to be investigated.

This paper investigates the long-run impacts of the free compulsory education reform on individuals' various forms of human capital, including education, cognitive achievement and health measured in early adulthood. The effects of this reform and other similar school subsidy programs on educational attainment have already been

[1]Molina-Millan et al. (2016) review the evaluations of the long-term effects of conditional cash transfer programs in Latin American.

stressed in previous studies. Meanwhile, the income effect from the reform may induce other forms of human capital investment besides formal education (Shi, 2012), and hence improve cognitive achievement and health. Education, cognitive achievement and health are important determinants of lifetime well-being and earnings (Card, 1999; Case et al., 2002; Heckman et al., 2006). The investigation of different types of human capital, therefore, provides us a better understanding of the consequences of the free compulsory education reform.

The reform was first launched in rural areas in 13 provinces and 2 direct-controlled municipalities (municipalities hereafter), and then gradually expanded to the entire rural areas. We exploit the cross-province variation in the phase-in of the reform and apply a difference-in-differences framework where the treatment varies by province of residence and birth date. More specifically, the treatment, *i.e.*, the duration of reform exposure, is the number of semesters that an individual is supposed to be eligible for the free compulsory education during ages 6-15.

Using a sample of individuals born between 1988 and 1993 in rural areas from China Family Panel Studies (CFPS), we find that longer reform exposure during ages 6-15 leads to better educational attainment and cognitive achievement measured by math test scores. More specifically, one additional semester of reform effects increases the probability of being enrolled in school by 8.4 percentage points and years of schooling by 0.22 years in 2010. The results also suggest that the reform effects become greater in the longer term, *i.e.*, one additional semester of exposure increases the probability of high school graduation by 7.9 percentage points and completed years of schooling by 0.56 years in 2014. With respect to cognitive achievement and health, one additional semester of reform exposure increases math test scores by 0.119 standard deviations (SDs), and if anything, word test scores by 0.074 SDs and self-reported health by 0.121 SDs. The results survive a variety of placebo tests and robustness checks. Furthermore, the reform effects on educational attainment are stronger for individuals with less-educated fathers, implying that the reform tends to increase the intergenerational educational mobility and reduce the regional inequality in education in rural China.

This paper contributes to the knowledge of the short- and long-term impacts of school subsidy programs that reduce the private costs of education. Although positive impacts of school subsidy programs on outcomes measured in the short run have been documented by a large number of studies (see Behrman (2010) for a recent review of this literature), little is known about the long-term effects of these programs. Baez and Camacho (2011), one of the few studies that investigate the the long-run effects of Colombia's national conditional cash transfer (CCT) program *Familias en Acción*, find a positive impact on the probability of completing secondary school. Behrman et al. (2011) find positive long-term impacts of a school subsidy program in Mexico (*Progresa*) on schooling and labor market outcomes. Two recent studies by Barham et al. (2013) and Barrera-Osorio et al. (2015) investigate the long-term effects of CCT programs in Nicaragua and Columbia, respectively, on educational outcomes. Our paper contributes to this line of literature by examining the long-run effects of reductions in the private costs of compulsory education on individual's schooling, cognitive achievement and health.

This study also adds to the literature on identifying the causal effects of educational reforms on human capital development in three ways. First, tuition-free education usually comes as a part of the compulsory schooling law in developed and developing countries. As a result, previous studies that estimate the impact of the compulsory schooling law on educational attainment can not explicitly distinguish the impact of free education from that of mandating education (see *e.g.*, Angrist and Krueger, 1991; Meghir and Palme, 2005; Grepin and Bharadwaj, 2015; Güneş, 2015). The compulsory education law in China has been effective since the mid-1980s, long before the free compulsory education reform. This allows us to separate the effect of the free compulsory education reform from the enforcement of the compulsory education law. Second, educational reforms are usually implemented nationwide simultaneously. Consequently, the evaluations of the policy impact relying on before-and-after comparisons are likely to be confounded by systematical differences across cohorts. The free compulsory education reform in rural China was implemented at different times across provinces, which enables us to identify the causal impact of the reform on human capital with a difference-in-differences approach. Finally, most existing studies rely on a binary variable indicating program participation to estimate the program effects, ignoring the potential heterogeneity in impact that is related to the timing and duration of program exposure (King and Behrman, 2009). This study applies the duration of program exposure, i.e., the number of semesters that an individual is supposed to be exposed to the free compulsory education during ages 6-15, which can better capture the heterogeneity in program effects that varies by the amount of exposure.

The rest of the paper is organized as follows. Section 2 introduces the free compulsory education reform in rural China. Section 3 presents a conceptual framework for the analysis. Section 4 describes the data and outlines the empirical strategy used in this paper. The results are presented and discussed in Section 5. Section 6 concludes.

2 The free compulsory education reform in rural China

The Law on 9-Year Compulsory Education, which took effect on July 1, 1986, entitles school-age children the right to receive at least 9 years of education (6-year primary education and 3-year junior high education). Although the bill authorized tuition-free education for the 9-year compulsory education (Article 10), the law was never strictly enforced. On the contrary, the financial burden of compulsory education was substantially borne by the households, especially after the financial reform of basic education in 1985.

Before 1985, the central government took financial responsibilities for basic education and local governments with low revenue could be subsidized by the central government. The financial reform in 1985 caused two major changes in the financing structure of education. The first is the financial decentralization, where local governments have taken full responsibility for the provision and financing of basic education. In rural areas, governments at the county-, town- and village-level were responsible for senior high, junior high, and primary schools, respectively. The second major change is the diversification of financial sources. The governments in rural areas could either broaden the base for the government's collection of revenue for education, for example, education levies, or broaden and intensify non-government resource mobilization at the school level, such as social contributions to education and school fees. The financial burden of education that was passed down to rural households (*e.g.*, social contributions to education, school fees, etc.) is likely to be a major deterrent to enrollment for rural children (Connelly and Zheng, 2003).

Prior to the free compulsory education reform in 2006, two educational reforms were implemented to alleviate the financial burden of education for poor, rural households and promote basic education. The first is the "tuition control" reform in 2001, which required that the tuition fees for attending public primary schools in rural areas should not exceed 160 yuan (approximately US$23.5) per student per year, and that for attending junior high schools should not exceed 260 yuan (approximately US$38.5) (Chyi and Zhou, 2014). The second is the "Two Exemption and One Subsidy" (TEOS) reform launched in 2003. The main objective of the TEOS reform is to reduce educational costs for students from extremely poor households, by providing a school subsidy package that includes tuition fee exemption, free textbooks and living subsidies.[2] In the spring of 2005, in addition to the extremely poor students, all primary and junior high school students from national designated poor counties were entitled to receive the subsidy package. [3]

The government initiated the free compulsory education reform on a nationwide basis in 2006. As a part of the reform, all rural students are entitled to receive tuition fee exemptions, and eligible students are provided with free textbooks and living subsidies.[4] In practice the free compulsory education reform mainly benefits those students that were not eligible for school subsidies under the TEOS reform by exempting their tuition and miscellaneous fees, because eligible students have been entitled to free textbooks and living subsidies since the TEOS reform. The reform at the time was also known as the New Security System for Rural Compulsory Education. The new system emphasizes the joint responsibility of central and local governments in financing compulsory education. The central government contributes more share in less developed region. Specifically, the central government covers 80% of the funding in the western provinces, many of which are among the poorest, and 60% in the central provinces. For municipalities and the eastern provinces, the percentage of the funding covered by the

[2]According to government documents, the policy gives the highest priority to orphans and students from households living in absolute poverty, followed by students from low-income families.

[3]There are total 592 national designated poor counties, which account for one-third of all counties in China. The number and percentage of students that were covered by the TEOS reform varied across regions. For example, in Shandong, a relatively rich province, only 53,000 students that account for 6.5% of all rural students in the province received school subsidies in 2005. In Guizhou, one of the poorest provinces, approximately 1.86 million students that represent one quarter of all rural students were covered by the TEOS reform in 2005.

[4]The eligibility criteria is similar to the one applied in the TEOS reform.

central government is determined by the local government's fiscal capacity.

The state council made the roll-out plan of the reform. All western provinces are required to implement the reform no later than the spring of 2006, and the other provinces no later than the spring of 2007. If richer eastern provinces and municipalities were able to afford the costs of the reform on their own, they could implement it at any time no later than the spring of 2007. As a result, all primary and junior high school students in rural areas of 2 municipalities and 11 provinces, most of which are poor, western provinces of China, have been exempted from paying tuition fees since the spring of 2006. The relatively rich municipalities and provinces, including Tianjin, Shanghai, Fujian, Jiangsu, Zhejiang and Guangdong, implemented the reform earlier than the required date set by the central government. By the fall of 2007, all primary and junior high school students in rural China have been eligible for the free compulsory education. Table 1 and Figure 2 show the actual roll-out of the free compulsory education reform in rural China.

3 Conceptual framework

Economic theory predicts that lowering the private costs of education leads to increased school enrollment due to both price and income effects. However, the positive effects of school subsidy programs on contemporaneous school enrollment in other developing countries may not apply to the free compulsory education reform in rural China, where primary and junior high school enrollment rates were already high before the reform. According to the 20% sample of the 2005 1% National Population Sample Survey of China, the school enrollment rate in rural China is 98.8% for 6-12 year-old and 84.7% for 13-16 year-old. Not surprisingly, Shi (2016) finds that the reform has no significant impact on the enrollment of 9-12 year-old children, and a significant yet small impact for 13-16 year-old children: a 10 percent reduction in the educational fee increases the probability of enrollment by 0.3 percentage points.

For most of the children who would be sent to school even in the absence of tuition fee exemptions, the reform should have minimal impact on their primary or junior high school enrollment. However, the cash transfers from the reform increase households' disposable income that can be spent on other goods, such as non-required educational investment, food and clothes. As a result, the reform may not only improve children's educational attainment but also cognitive achievement and health, all of which are vital to the lifetime earnings of the adults. Even in the most extreme case where the reform only increases the investment in education, the reform could have the second-order effects on cognitive achievement and health, through the causal relation running from more education to better cognitive achievement (Carlsson et al., 2015) and health (Silles, 2009). This paper, therefore, provides a more complete picture of the reform effects on human capital.

The reform effect on human capital development in the long run is mainly driven by the income effect from the reform. Our treatment measure, *i.e.*, the duration of reform exposure, can better reflect the income effect than a dummy variable indicating program participation. With a longer exposure to the reform, households could receive more cash transfers and invest more in the beneficiary child, leading to an improvement in human capital. In addition to income effect, the reform may also affect children's human capital through other channels that are less dependent on the duration of reform exposure, such as peer effects, changing social norms toward education and changing the quality of education due to increasing enrollment (Behrman et al., 2011).

4 Data and empirical strategy

4.1 Data

The data used in this study are from the China Family Panel Studies (CFPS) survey, which was launched in 2010 by the Institute of Social Science Survey (ISSS) of Peking University in China (Xie, 2012). The CFPS is a nationally representative, longitudinal survey of Chinese communities, families, and individuals, and contains data on 14,960 household and 42,590 individuals in 2010 baseline survey. Two follow-up surveys were conducted in 2012 and 2014, respectively. The CFPS sample covers 25 provinces/municipalities/autonomous regions (excluding Hong Kong, Macao, Taiwan, Xinjiang, Tibet, Qinghai, Inner Mongolia, Ningxia and Hainan), representing 95% of the Chinese population.

Human Capital. The human capital of an individual in this study includes schooling attainment, cognitive achievement and health. Specifically, we have four measures of schooling attainment, i.e., school enrollment status in 2010, years of schooling in 2010, high school graduation status in 2014 and completed years of schooling in 2014. The information on schooling attainment in 2014 is extracted from the family roster data of CFPS 2014, which is available for any family members regardless of the presence at the time of survey.[5] The CFPS administered a math test and a word recognition test for each individual aged 10 or above in 2010. The test scores are used to measure the individual's cognitive achievement. Health is measured by self-reported health status in 2010. Although math and word recognition test scores and self-reported health status are available in the 2014 survey, the information is only available for individuals who were present at the time of survey in 2014. To avoid potential selection issues caused by large sample attrition between 2010 and 2014, we estimate the impacts of the reform on cognitive test scores and health in 2010.[6]

Reform Exposure. The treatment measure, *i.e.*, duration of reform exposure, is measured by the number of semesters (*Semester*) that an individual is supposed to be exposed to the free compulsory education during ages 6-15. In China, each academic year begins on September 1^{st} and ends in July the next year. Spring semester, *i.e.*, the second semester of an academic year, begins on March 1^{st} the next year. According to the school enrollment policy, a child should enroll in primary school after celebrating the 6^{th} birthday and therefore is supposed to finish 9-year compulsory education at age 15. For example, a child born between September 1993 and August 1994 should enroll in primary school in September 2000 and graduate from junior high school in July 2009. Thus, the number of semesters that an individual is supposed to be exposed to the free

[5]Because the survey only asked about the highest degree attained in 2014, the variable on completed years of schooling is coded in a way that dropouts are treated as never attending the corresponding level of education. For example, if the highest level of education attained is junior high school degree, no matter whether the individual has ever attended senior high school, the number of completed years of schooling is 9.

[6]Among the 1,525 individuals in the sample, only 776 individuals have non-missing cognitive test scores and self-reported health status in the 2014 survey. Even with the attrited sample, we find similar results on impacts of the reform on cognitive achievement and health reported in 2014. Results are available from the authors upon request.

compulsory education is jointly determined by the effective date of the reform in the province and his/her birth date.[7] Suppose that the effective date of the reform in the province of residence is March 2006, the number of semesters that the individual in the above example is supposed to be exposed to the reform is seven.

The amount of exposure to the free compulsory education may suffer from measurement error problem if individuals do not enroll in primary school at the supposed age (grade-for-age) and/or finish primary school education in exact 6 years. This may cause problems only when the grade-for-age status or the length of primary school education is systematically related to the effective date of the reform as well as the outcome variables. For example, children in provinces where the reform was implemented earlier may be more likely to delay primary school enrollment, and the delayed enrollment may lead to lower educational attainment. To check this possibility, we regress individuals' grade-for-age status and the number of years spent on primary school education on the treatment variable *Semester*. No significant correlation is detected between the two outcomes and *Semester* in Appendix Table A.1, suggesting that the measurement error in the treatment variable is unlikely to bias our estimates. Additionally, the measurement error problem may arise if individuals deliberately migrate to provinces where the reform was implemented earlier for more semesters of the free compulsory education. In the sample, we find that more than 99% of rural residents born between 1991 and 1993, *i.e.*, the cohorts that may have been affected by the reform, report that they were living in the same province at age 12 as the one they were living in 2010. Although the incidence of cross-province migration is rare, we exclude the individuals who lived in a different province in 2010 from the one at age 12 in the analysis to avoid any potential biases.

Regression Sample. The analysis is restricted to rural residents born between 1988 and 1993 and interviewed in CFPS 2010.[8] Table 2 shows the distribution of *Semester* by birth year. Individuals born in 1988 and 1989 were not affected by the reform, and therefore are the control group in our quasi-experimental setting. Additionally, to

[7]The effective date of the reform in each province, as shown in Figure 2, is extracted from a variety of sources, most of which are official announcements by provincial governments (see *e.g.*, Hubei Provincial Department of Education (2007)).

[8]While significant sample attrition exists in CFPS for these cohorts prior to 2010, we find that it is unrelated to the treatment variable as shown in Appendix Table A.2.

minimize the possibility that an individual in the sample was still enrolled in junior high school in 2010, we only include those born in 1993 or before.[9] Our sample for the main analysis is composed of 1,525 rural residents born between 1988 and 1993.

Table 3 shows the summary statistics of variables used in the analysis.[10] Test scores and self-reported health are standardized with a mean of zero and a standard deviation of unity within each cohort. Information on exogenous individual and household characteristics are taken from the CFPS 2010. Provincial characteristics used in the analysis are extracted from the 20% sample of the 2005 1% National Population Sample Survey of China and China Statistical Year Books (National Bureau of Statistics of China, 2005, 2006).

4.2 Empirical strategy

In this study, we exploit the cross-province variation in the phase-in of the reform and apply a difference-in-differences (DID) strategy to identify the causal impact of the reform on the outcomes. The empirical specification is given by the following equation:

$$Y_{ipt} = \phi Semester_{ipt} + X_{i,2010}\beta + \delta_p + \gamma_t + \theta_p * t + \gamma_p * t^2 + \pi_t W_p + \omega_m + \epsilon_{ipt}, \qquad (1)$$

where Y_{ipt} is the outcome of an individual i born in year t and attending primary and junior high schools in province p. Reform exposure, $Semester_{ipt}$, is the number of semesters that an individual is supposed to be eligible for the free compulsory education. As such, ϕ is the parameter of interest that quantifies the effect of one additional semester of reform exposure on the outcome Y_{ipt}. Note that we employ an intention-to-treat (ITT) analysis here, as we use the treatment that individuals were supposed to receive rather than the treatment actually received. $X_{i,2010}$ is a vector of individual and household characteristics in 2010 that may be related to the outcomes, including gender, the number of siblings, household size, both parents' years of schooling and household

[9]For example, children born between September and December in 1994 were supposed to graduate from junior high school in July 2010. As the survey period of CFPS 2010 was from April to September, these children could still be enrolled in junior high school at the time of interview.

[10]Some observations have missing values on parents' years of schooling or the number of siblings. Following Deming (2009), we account for the missing data by imputing the mean value of the sample, and include a dummy variable for imputed responses in the regressions.

wealth index.[11]

Province fixed effects δ_p are included to control for time-invariant observed and unobserved characteristics of provinces that may be related to both the effective date of the reform and the outcomes. Birth year fixed effects γ_t account for cohort trends or shocks in the outcomes, which are common to all provinces. In addition to birth cohort fixed effects, in our preferred specification, $\theta_p * t$ and $\gamma_p * t^2$ are included to control for province-specific linear and quadratic cohort trends in the outcomes. To allow the cohort trend in outcome variables to vary with provincial characteristics in the initial period, we include the interactions of provincial characteristics in 2005 denoted by W_p and a vector of birth-year-specific coefficients π_t. The vector of provincial characteristics includes the fraction of rural residents, the fraction of high school graduates among the 25-55 years-olds, per capita educational expenditure of rural households as a share of total expenditure, school enrollment rate among 6-15 year-old rural children, gross domestic product (GDP), total population, total government revenue and expenditure, and government expenditure on education as a share of GDP. To account for the implementation of the TEOS in national designated poor counties in 2005, we also control for the interactions between the proportion of poor counties in each province and birth year dummies.

As previously discussed, the treatment status is jointly determined by the effective date of the reform and the birth date of individuals. For any individuals born in the same year in a given province, their treatment status depends on their birth months. Individuals born between September and December shall have two more semesters of exposure than those born between January and August. Therefore, the effect we identify is probably driven by the systematic difference between those born earlier and those born later in the same birth cohort. To address this concern, in our preferred specification, we include birth month fixed effects denoted by ω_m.

All models are estimated using the sample weights provided by CFPS. Standard errors are adjusted for clustering at the province level to account for any potential cor-

[11]The wealth index is constructed based on a principal component analysis of three indicators concerning the household's choices of drinking water, fuel and sanitation, and another three for access to high quality electricity, farm household and car ownership. Controlling for individual components instead of the index, or not controlling for any of them does not change our results substantially.

relation across cohorts within the same province (Bertrand et al., 2004). Because there are only 24 clusters, we also report the p-values calculated using the wild bootstrap of (Cameron et al., 2008) to correct for a small number of clusters for the key variable of interest in the baseline model.[12]

The key identification assumption for the DID strategy is that provinces with different effective dates would have had the same time trends in the outcomes in the absence of the reform. The assumption can be relaxed after we control for province-specific quadratic cohort trends and cohort-specific effects of provincial characteristics. The identification strategy is valid as long as the changes in the outcomes across cohorts in the absence of the reform would be the same for provinces of similar pre-treatment characteristics, conditional on the province-specific quadratic cohort trends. To test the validity of this assumption, we perform a number of placebo tests using the sample of older cohorts and the urban sample, both of which were not affected by the reform.[13] Section 5.2.1 provides a discussion on the details of these tests.

Another concern is that there may be other shocks occurring at the same time as the reform that might have affected the outcomes, such as the large-scale closures and mergers of rural schools since 2001, the roll-out of the New Cooperative Medical Scheme in rural China since 2003, the Tax-for-Fee reform since 2001, and the 2008-2009 financial crisis. We perform several robustness checks to address this concern in Section 5.2.2.

5 Results

5.1 Baseline results

Table 4 reports the baseline results of DID estimation for educational outcomes. The coefficient of *Semester* can be interpreted as the causal effect of one additional semester of reform exposure on the outcomes. All regressions include exogenous individual and household characteristics measured in 2010, province fixed effects and birth co-

[12]Wild bootstrap p-values for coefficients in remaining analyses are available from the authors upon request.

[13]The free compulsory education reform was not implemented in urban areas until the fall of 2008.

hort fixed effects. In columns (2) and (5), we further control for province-specific linear and quadratic birth cohort trends. Moreover, we allow the cohort or time trends to vary with certain provincial characteristics in the initial period (W_p) and we do so by adding the interaction terms between birth cohort dummies and W_p. Columns (3) and (6) additionally include birth month fixed effects.

In all three specifications, the length of reform exposure is positively associated with individuals' educational attainment. Adding controls for provincial trends and cohort-specific effects of provincial characteristics slightly change the magnitude of the coefficients, suggesting that the strict assumption of common cohort or time trends across provinces may not hold. The estimated effects increase after further including birth month fixed effects. This could be attributed to the fact that the children born in the birth months that entitled them with more program exposure (born between September and December instead of between January and August in a given year) achieve lower educational attainment.

Estimates from our preferred specification (columns (3) and (6)) suggest that one additional semester of reform exposure increases the probability of being enrolled in school in 2010 by 8.4 percentage points. Yet the effect on years of schooling in 2010 is insignificant and small. This may be ascribed to the fact that 37.6% of individuals in our sample have not completed their schooling in 2010. Thus, the estimated effect for years of schooling in 2010 may not fully reflect the reform effect on completed schooling. The educational outcomes in 2014, when 92% of the individuals have completed their schooling, can better capture the effect of the reform on lifetime schooling. The estimates suggest that one additional semester of reform exposure leads to a 7.9 percentage points increase in the probability of high school graduation and an increase in schooling by 0.56 years in 2014.[14]

Table 5 shows the estimation results for cognitive test scores and self-reported health status in 2010. All columns of regressions include a full set of controls (as in columns (3) and (6) of Table 4). The results suggest that one additional semester of reform

[14]In other words, one additional year exposure to the reform leads to an increase in schooling by 1.12 years in 2014. This could be possible, for example, one semester exposure to the reform may increase the promotion rate from junior high school to senior high school. A student enrolled in senior high school tend to complete three years schooling for the diploma.

exposure increases math test scores by 0.119 standard deviation, equivalent to answering 0.75 more questions correctly out of 24 total questions in the math test. However, the coefficients of *Semester* on word test scores and self-reported health status are positive but statistically insignificant.

The coefficients of *Semester* are intention-to-treat (ITT) estimates, averaging across individuals with higher and lower likelihoods of being affected by the reform. Some individuals may not be treated by the reform even though their treatment *Semester* is not zero, for instance, if they have been receiving fee exemptions since the TEOS reform.[15] Thus, the average effect of treatment on the treated is larger than the ITT estimates.

The sample of individuals whose treatment is treated as zero may also be affected by the reform through the reform exposure of their siblings. The money saved from siblings' fee exemptions may increase the amount of resources that can be allocated to the child that is not eligible for the free compulsory education.[16] To take the potential spillover effects of the reform into account, we construct the total number of semesters of all siblings' reform exposure and include it in the regression. The results presented in Appendix Table A.3 show that the coefficients of all siblings' reform exposure are insignificant on all seven outcome variables, supporting the hypothesis of the flypaper effect found in Shi (2012). Moreover, after controlling for the total number of semesters of all siblings, the key results remain.

The baseline specifications assume a linear relationship between the outcomes and the number of semesters exposed to the free compulsory education. In Table A.4 in Appendix A, we explore non-linear specifications by adding the square of *Semester* in the regression. The quadratic terms are not statistically significant at conventional levels, rendering support to the baseline linear specification.

[15]The TEOS reform exempted 34 million rural students from paying tuition and miscellaneous fees in 2005 (The Central People's Government of the People's Republic of China, 2006), accounting for approximately 20% of 6-15 year-old rural children.

[16]Shi (2012) finds that educational fee reductions increase voluntary educational spending on the same children who receive the fee reductions, providing no evidence of the redistribution of government transfers to other family members.

5.2 Validity of DID Strategy

5.2.1 Placebo tests

In this section, we re-estimate the model using two placebo tests. The sample used in the first test contains the older cohorts in rural areas, and the second sample comprises the same cohorts as the one we used in the main analysis but from urban areas. To conserve space, we only report the coefficients of the key variable, *i.e.*, *Semester*, and coefficients estimated for the remaining covariates are omitted.

We re-estimate the DID model over the pre-treatment cohorts, but with the assumption that the treatment took effect 4, 6, 8, 10, 12, and 14 years earlier than the actual timing of the free compulsory education reform. Accordingly, we apply individuals in rural areas born between 1984-1989, 1982-1987, 1980-1985, 1978-1983, 1976-1981, and 1974-1979 who were not exposed to the reform during their 6-15 years old. The "placebo *Semester*" is constructed based on the assumed timing of the reform in each province and the birth date of each individual. If our empirical specification is valid, the DID estimator should be statistically insignificant and close to zero. The point estimates and 95% confidence intervals of DID estimator are displayed in Figure 2 for four outcome variables that are significantly affected by the reform according to our baseline results, *i.e.*, enrollment status in 2010, high school graduation in 2014, completed years of schooling in 2014, and math test scores in 2010. As illustrated in Figure 3, the coefficients of *Semester* are mostly close to zero and statistically insignificant in the placebo tests, justifying the application of DID strategy. The insignificant coefficients in the placebo tests also imply that our results are not driven by whether an individual's birth date is before or after the 1st of September, based on which *Semester* is constructed.

The free compulsory education reform was not implemented in urban areas until September 2008. We apply the same estimations on the urban sample who were born between 1988 and 1993 to test whether there is any sudden province-level shocks that coincide with the reform and might have affected the outcomes.[17] To this end, we assign the treatment status *Semester* to urban sample using the roll-out status of the reform in

[17]The enrollment rate of 15-18 year-old children in urban areas is approximately 90% in 2005. Although it is remarkably higher than that in rural areas, there is still room for improvement in senior high school enrollment for the urban sample.

rural areas in the same province. We expect that the "placebo" treatment status has no significant effects on any outcome variables. As shown in Panel A of Table 6, the coefficients of *Semester* in the placebo tests are insignificant for all outcome variables, except health status. Nevertheless, the insignificant coefficients of *Semester* may be because households in urban areas are less likely to have credit constraints than their rural counterparts. Thus, the human capital investment decisions in these households are not affected as much as those in rural households by the same province-level shocks. We then restrict the sample to individuals from households of lower socioeconomic status (SES), that is, those whose fathers do not complete senior high school education.[18] The results are reported in Panel B of Table 6. Again, the coefficients of *Semester* are statistically insignificant for all outcome variables, suggesting that our main results are not driven by province-level shocks that affect both urban and rural areas.

5.2.2 Confounding factors

The DID strategy also assumes that no other shocks that might have affected the outcomes occurred simultaneously as the free compulsory education reform. To test for this assumption, we reestimate the equation 1 but account for the impacts of concurrent shocks that may confound our results.

Closures and Mergers of Rural Schools

In order to improve the quality of education in rural areas and reduce expenditure on education, China has carried out the large-scale closures and mergers of rural schools since 2001. As a result, a large number of rural schools in remote villages have been closed. It is estimated that the number of primary schools has fallen by more than 50%, from 384,004 in 2001 to 169,045 in 2010 (Educational Statistics Yearbook of China, 2002, 2011). Students in remote villages were transferred to county or township schools, which results in a considerable increase in the cost of education. Thus, the

[18] Using fathers' educational attainment to measure the households' SES has two advantages over other measurements. First, the fathers of sampled individuals are unlikely to be affected by the reform and therefore their educational attainment is exogenous to the reform. Second, in rural China, the husband is usually the one who have a stronger influence over joint household decisions, e.g. children's educational investment (Carlsson et al., 2012).

large-scale closures and mergers of rural schools may increase dropout rate. If more closures and mergers of rural schools take place in less-developed western provinces, the omission of the accessibility of schools may bias our estimates.

To address this issue, in Panel A of Table 7, we control for the province-level school accessibility, measured by the numbers of primary, junior high and senior high schools per 1,000 children during the corresponding periods when the child was supposed to be enrolled in primary, junior high and senior high school.[19] As indicated in Panel A, after controlling for the accessibility of schools, the coefficients of semesters do not change much.

New Cooperative Medical Scheme

China launched the New Cooperative Medical Scheme (NCMS) pilots in July 2003, which aims to provide health insurance coverage for the nation's entire rural population by 2010. The access to public health insurance may help improve the school enrollment of school-aged children in rural areas when households experience negative health shocks (Chen and Jin, 2012; Liu, 2016). As the NCMS pilots were firstly introduced in economically more developed counties, our estimates of reform effects tend to be biased if the roll-out of two reforms coincide. To address this concern, we add controls for the interaction terms between the effective years of the NCMS at the county level and birth year dummies and reestimate equation 1. Our baseline results are robust to the inclusion of NCMS, as shown in Panel B of Table 7.

Tax-for-Fee reform

The Tax-for-Fee reform was initiated in 2001 by the Chinese government. It was first implemented in Anhui and Jiangsu in 2001, and gradually rolled out to other provinces by 2003. The primary objective of the reform is to introduce greater fiscal discipline at

[19]For a child born in 1990, for example, the number of primary schools per student is the mean of the number of rural primary schools in each year between 1996 and 2002, divided by the number of children at ages 6-12 in 2005. The respective numbers of junior and senior high schools are calculated in a similar way. Information on the numbers of rural schools is extracted from the Educational Statistic Yearbooks of China from 1994 to 2010.

the local level, by replacing the range of taxes, fees and levies that had previously been imposed on farmers with a standardized agricultural tax. The agricultural tax was abolished later in different years across provinces and its final abolition in Hebei, Shandong and Yunnan provinces in 2006 marked the ending of the reform.

The tax-for-fee reform raises per capita income of rural residents, but at the same time has a negative effect on public good provision (e.g., roads, irrigation projects and school construction) (Luo et al., 2007). Our estimates tend to be biased if the Tax-for-Fee reform can affect human capital development, and the roll-outs of the Tax-for-Fee reform and the free compulsory educational reform overlap. To account for the potential impact of the Tax-for-Fee reform on the outcomes for different cohorts, we interact birth year dummies with the beginning and ending year of the reform at the province level.[20] We add controls for these interaction terms and re-estimate the effects of the free compulsory education reform on the outcomes. Our results are not sensitive to controlling for the impact of the Tax-for-Fee reform (see Panel C of Table 7), suggesting that our findings are unlikely to be driven by the Tax-for-Fee reform.

Financial Crisis

The collapse of international trade during the 2008-2009 financial crisis may reduce the demand for unskilled workers, and thus, decrease the dropout rates during this period. Moreover, the financial crisis tends to have a greater effect on coastal provinces, for example, Guangdong and Zhejiang, and individuals who completed their senior high education in 2008. Thus, our estimates of reform effects may be confounded by the 2008-2009 financial crisis.

We proxy the employment shock of financial crisis by the percentage change in the total employment in the manufacturing industry in 2008 relative to 2004 at the province level, which is calculated based on the data from the first and second national economic censuses. We account for the heterogeneous effects of the financial crisis on individuals in different birth cohorts by controlling for the interaction terms between the employment change at the province level and birth year dummies. The results are reported in Panel D of Table 7, implying that our main results are robust to the inclusion of the

[20]The year when the reform was completed is that when the agricultural tax was entirely removed.

financial crisis.

5.3 Heterogeneous effects

The effect of the free compulsory education reform on individuals' human capital may depend on the gender and household SES of individuals. In this section, we investigate the heterogeneous effects of the reform. We first interact the treatment status *Semester* with the gender of individuals and report the results in Panel A of Table 8. Overall, the reform effects on educational attainments and cognitive test scores are smaller for males, but that on health is smaller for females. However, the coefficients on the interaction term are statistically insignificant for 6 out of 7 outcomes, suggesting minor heterogeneity in the reform effects by gender.

We next interact the treatment variable *Semester* with a dummy that equals one if the father did not complete junior high school education and zero otherwise, to investigate the heterogeneous effects of the reform by household SES.[21] The results in Panel B of Table 8 show that the reform effects on educational attainment and cognitive achievement are stronger for individuals from low SES households. However, though insignificant, the reform may have a stronger impact on health for those with better educated father. Such results suggest that reductions in the costs of compulsory education tend to relax the financial constraint for poor households and thus increase educational investment on the children. For households without credit constraint, on the other hand, reductions in the costs of compulsory education may induce other forms of human capital investment, for example, health.

[21] Footnote 18 explains why we use father's education to proxy SES. Although children from extremely poor households may have already been protected by the TEOS before the launch of the free compulsory education reform, they merely account for a small portion of low SES households. For example, in Guizhou, 85% individuals in the sample are from low SES households, but only one quarter of rural students were covered by the TEOS in 2005 according to government documents. Similarly, in Shandong province, while there are 44% individuals in the sample whose father has less than a junior high school degree, the proportion of rural students that were covered by TEOS in 2005 was 6.5%.

6 Conclusion

This paper examines the long-run effects of a free compulsory education reform in rural China on individuals' educational attainment, cognitive achievement and health. The free compulsory education reform, which aims to reduce the costs of compulsory education for rural students and promote enrollment in rural China, was initiated in rural areas of 2 municipalities and 11 provinces in China in 2006 and gradually expanded to the entire nation. The variation in the phase-in of the reform across provinces allows us to apply a difference-in-differences strategy to identify the causal impact of the exposure to the reform during compulsory education period (ages 6-15) on a variety of early adulthood human capital development.

Existing studies suggest that the reform may boost short-term enrollment in primary and junior high schools (Chyi and Zhou, 2014; Shi, 2016), and reductions in compulsory educational fees may increase voluntary educational investment on the targeted children (Shi, 2012). We show that the increased enrollment and educational investment may be translated into long-term gains in human capital. One additional semester exposure to the free compulsory education increases the probability of high school graduation by 7.9 percentage points and completed years of schooling by 0.56 years in 2014. With regard to cognitive achievement and health, one additional semester of free compulsory education increases math test scores by 0.119 standard deviations (SDs), and although insignificantly, word test scores by 0.074 SDs and self-reported health by 0.121 SDs.

We find that the reform has a stronger impact on educational attainment of children with less educated fathers. This finding is important from a policy perspective, given the fact that intergenerational income and educational mobility has declined sharply since the economic reform in China, particularly in rural and western areas (Fan et al., 2015). Chen et al. (2015) attribute the decreasing intergenerational mobility in education to educational policies, i.e., educational expansion along with the rapidly rising costs of education. Our study provides evidence that effective educational policies reducing the private costs of basic education may slow down the decline in intergenerational mobility of education in rural China. Furthermore, under the reform the central government allocates a larger proportion of the funding for free compulsory education in poorer provinces than in richer ones. Given the high regional inequality in educational attainment in China (Fleisher et al., 2010), our finding implies that increasing human capital investment in less-developed areas may be an effective way to both increase intergenerational educational mobility and reduce regional inequality in education.

Our results suggest that the estimates of short-run effects on enrollment tend to underestimate the effects on the lifetime schooling and the earnings of adults. Hence, understanding the reform effects on longer-term outcomes is necessary for the estimation of the internal rate of return on the compulsory education subsidies. Future research can investigate how the reform affects labor market outcomes at later stages of life.

References

Angrist, J. D. and Krueger, A. B. (1991). Does compulsory school attendance affect schooling and earnings? *Quarterly Journal of Economics*, CVI(November):979–1014.

Baez, J. E. and Camacho, A. (2011). Assessing the long-term effects of conditional cash transfers on human capital: Evidence from Colombia. IZA Discussion Paper No.5751.

Barham, T., Macours, K., and Maluccio, J. a. (2013). More schooling and more learning? effects of a three-year conditional cash transfer program after 10 years.

Barrera-Osorio, F., Linden, L. L., and Saavedra, J. E. (2015). Medium term educational consequences of alternative conditional cash transfer designs: Experimental evidence from Colombia. Unpublished Manuscript.

Behrman, J. R. (2010). *Investment in education-inputs and incentives*, volume 5. Elsevier BV, 1 edition.

Behrman, J. R., Parker, S. W., and Todd, P. E. (2011). Do conditional cash transfers for schooling generate lasting benefits?: A five-year followup of PROGRESA/Oportunidades. *Journal of Human Resources*, 46(1):203–236.

Bertrand, M., Duflo, E., and Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics*, 119:249–275.

Cameron, a. C., Gelbach, J. B., and Miller, D. L. (2008). Bootstrap-based improvements for inference with clustered errors. *Review of Economics and Statistics*, 90(3):414–427.

Card, D. (1999). The causal effect of education on earnings. *Handbook of labor economics*, 3:1801–1863.

Carlsson, F., He, H., Martinsson, P., Qin, P., and Sutter, M. (2012). Household decision making in rural China: Using experiments to estimate the influences of spouses. *Journal of Economic Behavior and Organization*, 84(2):525–536.

Carlsson, M., Dahl, G. B., Öckert, B., and Rooth, D.-O. (2015). The effect of schooling on cognitive skills. *Review of Economics and Statistics*, 97(3):533–547.

Case, A., Lubotsky, D., and Paxson, C. (2002). Economic status and health in childhood: The origin of the gradient. *American Economic Review*, 92(5):1308–1334.

Chen, Y. and Jin, G. Z. (2012). Does health insurance coverage lead to better health and educational outcomes? Evidence from rural China. *Journal of Health Economics*, 31(1):1–14.

Chen, Y., Suresh, N., Tinghua, Y., and Noam, Y. (2015). Intergenerational mobility and institutional change in 20th century china. *Explorations in Economic History*, 58:44–73.

Chyi, H. and Zhou, B. (2014). The effects of tuition reforms on school enrollment in rural China. *Economics of Education Review*, 38:104–123.

Connelly, R. and Zheng, Z. (2003). Determinants of school enrollment and completion of 10 to 18 year olds in China. *Economics of Education Review*, 22(4):379–388.

Deming, D. (2009). Early childhood intervention and life-cycle skill development: Evidence from Head Start. *American Economic Journal: Applied Economics*, 1(3):111–134.

Fan, Y., Yi, J., and Zhang, J. (2015). The Great Gatsby curve in China: Cross-sectional inequality and intergenerational mobility. mimeo.

Fleisher, B., Li, H., and Zhao, M. Q. (2010). Human capital, economic growth, and regional inequality in China. *Journal of Development Economics*, 92(2):215–231.

Glewwe, P. and Kremer, M. (2006). Schools, teachers, and education outcomes in developing countries. *Handbook of the Economics of Education*, 2(06):945–1017.

Grepin, K. A. and Bharadwaj, P. (2015). Maternal education and child mortality in Zimbabwe. *Journal of Health Economics*, 44:97–117.

Güneş, P. M. (2015). The role of maternal education in child health: Evidence from a compulsory schooling law. *Economics of Education Review*, 47:1–16.

Heckman, J. J., Stixrud, J., and Urzua, S. (2006). The Effects of Cognitive and Noncognitive Abilities on Labor Market Outcomes and Social Behavior. *Journal of Labor Economics*, 24(3):411–482.

Hubei Provincial Department of Education (2007). `http://www.hbe.gov.cn/content.php?id=3925`. Accessed: 2016-10-24.

King, E. M. and Behrman, J. R. (2009). Timing and duration of exposure in evaluations of social programs. *World Bank Research Observer*, 24(1):55–82.

Liu, K. (2016). Insuring against health shocks: Health insurance and household choices. *Journal of Health Economics*, 46:16–32.

Luo, R., Zhang, L., Huang, J., and Rozelle, S. (2007). Elections, fiscal reform and public goods provision in rural China. *Journal of Comparative Economics*, 35(3):583–611.

Meghir, C. and Palme, M. (2005). Educational reform, ability, and family background. *American Economic Review*, 95(1):414–424.

Molina-Millan, T., Barham, T., Macours, K., Maluccio, J. A., and Stampini, M. (2016). Long-term impacts of conditional cash transfers in Latin America: Review of the evidence. Technical report, Inter-American Development Bank.

National Bureau of Statistics of China (2005). `http://data.stats.gov.cn/easyquery.htm?cn=E0103`. Accessed: 2016-10-24.

National Bureau of Statistics of China (2006). *China Statistical Year Book*. China Statistical Press, Beijing.

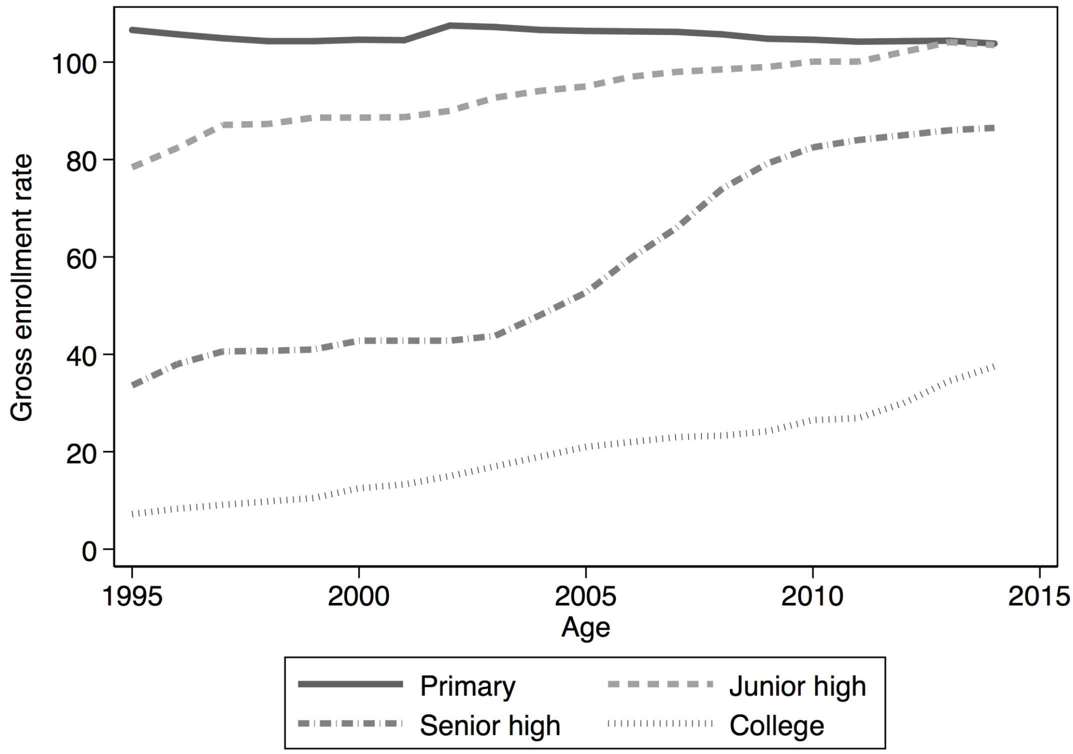
Orazem, P. F. and King, E. M. (2007). Schooling in developing countries: The roles of supply, demand and government policy. *Handbook of Development Economics*, 4(07):3475–3559.

Shi, X. (2012). Does an intra-household flypaper effect exist? Evidence from the educational fee reduction reform in rural China. *Journal of Development Economics*, 99:459–473.

Shi, X. (2016). The impact of educational fee reduction reform on school enrolment in rural China. *Journal of Development Studies*, Forthcoming.

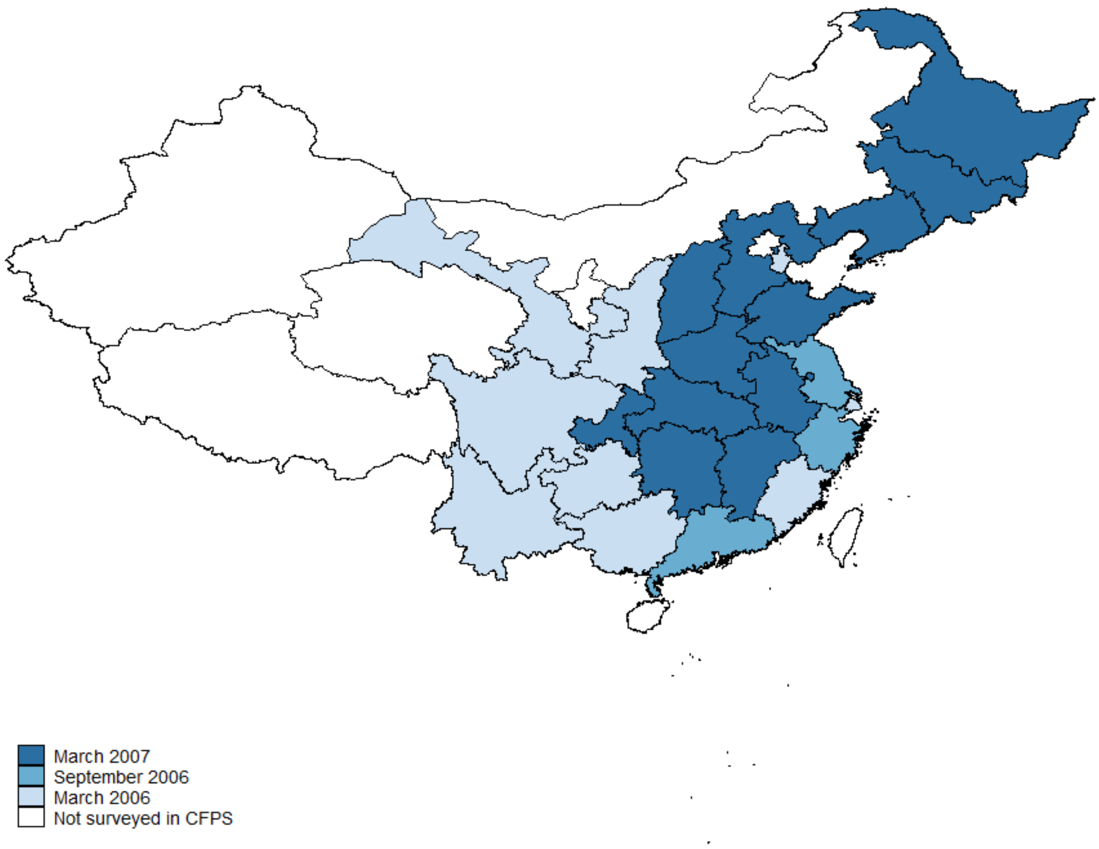
Silles, M. A. (2009). The causal effect of education on health: Evidence from the United Kingdom. *Economics of Education Review*, 28(1):122–128.

The Central People's Government of the People's Republic of China (2006). `http://www.gov.cn/jrzg/2006-04/25/content_265438.htm`. Accessed: 2016-10-24.



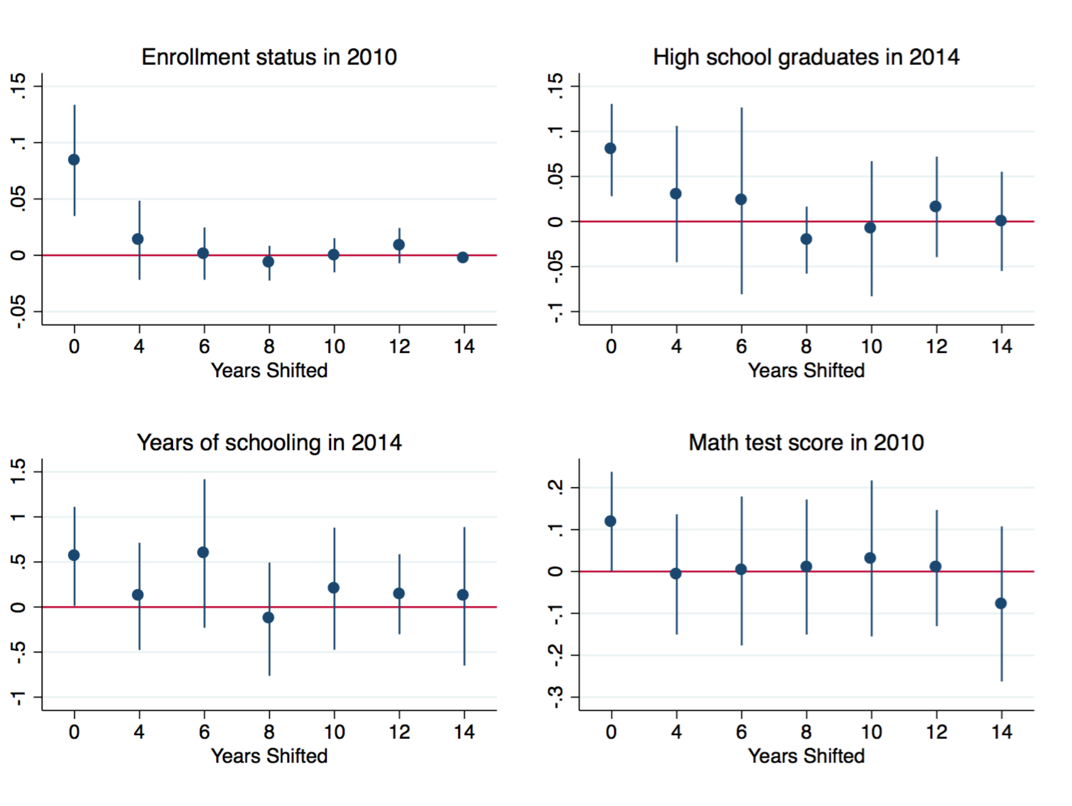
Data source: China Statistical Abstract 2015.

Figure 1: Gross enrollment rate in China



Note: Authors' own construction based on various local legal and administrative documents.

Figure 2: Rollout of the free compulsory education reform



Note: We re-estimate the DID model over the pre-treatment cohorts, but with the assumption that the treatment took effect 4, 6, 8, 10, 12, and 14 years earlier than the actual timing of the free compulsory education reform. The point estimates and 95% confidence intervals of DID estimator are displayed.

Figure 3: Estimated coefficient from the time-shifted placebo tests

Table 1: Effective date of the reform

Effective Date	Provinces
March 2006	Tianjin, Shanghai, Fujian, Guangxi, Chongqing, Sichuan, Guizhou, Yunnan, Shaanxi, Gansu
September 2006	Jiangsu, Zhejiang, Guangdong
March 2007	Hebei, Shanxi, Liaoning, Jilin, Heilongjiang, Anhui, Jiangxi, Shandong, Henan, Hubei, Hunan

Note: Authors' own construction based on various local legal and administrative documents.

Table 2: Treatment status by birth year

	Year of birth						
# semesters eligible for free education	1988	1989	1990	1991	1992	1993	Total
0	259	265	224	92	0	0	840
1	0	0	27	92	76	0	195
2	0	0	0	19	15	0	34
3	0	0	0	42	115	71	228
4	0	0	0	0	11	22	33
5	0	0	0	0	32	110	142
6	0	0	0	0	0	13	13
7	0	0	0	0	0	40	40

Note: Authors' own calculation based on the effective date of the reform in each province and birth dates of individuals in the sample.

Table 3: Summary statistics

Variables	(1) N	(2) mean	(3) sd
Panal A: Treatment variable			
Semester	1,525	1.408	1.962
Panel B: Outcome variables			
Currently in school in 2010	1,525	0.376	0.485
Completed years of schooling in 2010	1,525	9.171	3.552
High school graduates in 2014	1,388	0.445	0.497
Completed years of schooling in 2014	1,388	9.990	4.240
Standardized math test score	1,525	0.000	0.998
Standardized word recognition test score	1,525	0.000	0.998
Standardized self-rated health	1,525	-0.000	0.998
Panel C: Exogeneous control variables			
Age	1,525	19.52	1.718
Male	1,525	0.487	0.500
Number of siblings	1,524	1.469	1.041
Wealth index	1,525	-0.036	1.237
Household size	1,525	4.874	1.620
Father's years of schooling	1,429	6.276	4.094
Mother's years of schooling	1,461	3.763	4.061

Note: Based on CFPS 2010, 2014.

Table 4: Effects of the reform on educational attainments in 2010 and 2014

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	A. Enrolled in school in 2010			*B. Years of schooling in 2010*		
Semester	0.053***	0.077***	0.084***	0.230	0.227	0.220
	(0.014)	(0.019)	(0.024)	(0.135)	(0.143)	(0.208)
			[0.000]			[0.328]
Male	0.009	0.003	0.004	0.029	0.027	0.007
	(0.027)	(0.033)	(0.033)	(0.189)	(0.215)	(0.208)
Siblings	-0.035**	-0.022	-0.022	-0.565**	-0.508*	-0.507*
	(0.016)	(0.017)	(0.017)	(0.248)	(0.257)	(0.254)
Wealth index	0.003	-0.005	-0.010	0.340***	0.249**	0.233**
	(0.009)	(0.011)	(0.010)	(0.098)	(0.114)	(0.104)
HH size	-0.007	-0.009	-0.008	-0.013	-0.017	-0.015
	(0.013)	(0.013)	(0.013)	(0.053)	(0.057)	(0.056)
Father's years of schooling	0.013***	0.013***	0.014***	0.180***	0.176***	0.180***
	(0.003)	(0.003)	(0.003)	(0.022)	(0.019)	(0.021)
Mother's years of schooling	0.006	0.005	0.005	0.105**	0.099**	0.100**
	(0.004)	(0.004)	(0.004)	(0.038)	(0.039)	(0.039)
Observations	1,525	1,525	1,525	1,525	1,525	1,525
R^2	0.259	0.326	0.341	0.311	0.371	0.382
	C. High school graduate in 2014			*D. Years of schooling in 2014*		
Semester	0.041*	0.047**	0.079***	0.428**	0.395*	0.563**
	(0.023)	(0.021)	(0.025)	(0.187)	(0.220)	(0.265)
			[0.001]			[0.044]
Male	-0.029	-0.027	-0.028	-0.137	-0.167	-0.183
	(0.035)	(0.038)	(0.037)	(0.301)	(0.325)	(0.326)
Siblings	-0.048*	-0.040	-0.041	-0.607**	-0.554*	-0.559*
	(0.025)	(0.025)	(0.024)	(0.278)	(0.302)	(0.299)
Wealth index	0.020*	0.011	0.008	0.318*	0.273	0.218
	(0.011)	(0.012)	(0.012)	(0.158)	(0.160)	(0.138)
HH size	-0.026**	-0.028**	-0.028**	-0.237**	-0.235**	-0.236**
	(0.012)	(0.011)	(0.011)	(0.086)	(0.090)	(0.087)
Father's years of schooling	0.021***	0.020***	0.019***	0.214***	0.197***	0.198***
	(0.004)	(0.004)	(0.004)	(0.031)	(0.028)	(0.027)
Mother's years of schooling	0.011**	0.011**	0.012**	0.096**	0.101**	0.103**
	(0.005)	(0.005)	(0.005)	(0.043)	(0.043)	(0.040)
Observations	1,388	1,388	1,388	1,388	1,388	1,388
R^2	0.214	0.285	0.298	0.298	0.364	0.385
Province quadratic trends	No	Yes	Yes	No	Yes	Yes
Cohort*2005 province char.	No	Yes	Yes	No	Yes	Yes
Birth month FE	No	No	Yes	No	No	Yes

Note: Each column of each panel is from a separate regression. Community and birth year fixed effects are included in all regressions. Estimates are weighted using sampling weights provided in the data. Robust standard errors in parentheses are corrected for clustering at provincial level. The *p*-values computed using the wild bootstrap of Cameron et al. (2008) with 1,500 replications to correct for a small number of clusters are in brackets in columns (3) and (6). *p < 0.1. ** p < 0.05. *** p < 0.01.

Table 5: Effects of the reform on cognitive test scores and health outcomes

Variables	Math (1)	Word (2)	General health (3)
Semester	0.119**	0.074	0.121
	(0.057)	(0.088)	(0.099)
	[0.049]	[0.433]	[0.259]
Male	-0.012	-0.124*	0.147**
	(0.071)	(0.072)	(0.061)
Siblings	-0.154*	-0.124	0.059*
	(0.082)	(0.101)	(0.032)
Wealth index	0.049	0.049	0.071*
	(0.031)	(0.041)	(0.037)
HH size	-0.014	-0.020	-0.001
	(0.014)	(0.023)	(0.025)
Father's years of schooling	0.044***	0.043***	0.009
	(0.005)	(0.009)	(0.012)
Mother's years of schooling	0.026**	0.021*	0.003
	(0.012)	(0.011)	(0.008)
Observations	1,525	1,525	1,525
R^2	0.326	0.367	0.123

Note: Robust standard errors in parentheses are corrected for clustering at provincial level. Each column is from a separate regression estimated with the same specification as in column (4) of Table 4. The *p*-values computed using the wild bootstrap of Cameron et al. (2008) with 1,500 replications to correct for a small number of clusters are in brackets. *p < 0.1. ** p < 0.05. *** p < 0.01.

Table 6: Placebo tests

Variables	(1) In school in 2010	(2) Years of schl. in 2010	(3) HS grad. in 2014	(4) Years of schl. in 2014	(5) Math	(6) Word	(7) General health
Panel A. Urban sample							
Semester	-0.046	-0.093	-0.048	-0.027	0.022	-0.013	0.111*
	(0.033)	(0.171)	(0.028)	(0.185)	(0.066)	(0.094)	(0.060)
Observations	1,179	1,179	936	936	1,179	1,179	1,178
R^2	0.382	0.429	0.365	0.450	0.352	0.272	0.187
Panel B. Urban sample, disadvantaged individuals only							
Semester	0.006	0.012	-0.049	0.111	0.105	-0.002	0.096
	(0.033)	(0.138)	(0.047)	(0.348)	(0.105)	(0.126)	(0.094)
Observations	753	753	599	599	753	753	753
R^2	0.396	0.418	0.400	0.446	0.337	0.297	0.277

Note: Robust standard errors in parentheses are corrected for clustering at the province level. Each coefficient is from a separate regression estimated with the same specification as in column (4) of Table 4. An individual is defined as disadvantaged if the father possesses less than a high school degree. *p < 0.1. ** p < 0.05. *** p < 0.01. *p < 0.1. ** p < 0.05. *** p < 0.01.

Table 7: Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variables	In school in 2010	Years of schl. in 2010	HS grad. in 2014	Years of schl. in 2014	Math	Word	General health
Panel A. Primary, junior high and senior high school availability							
Semester	0.079***	0.243	0.085***	0.499*	0.119	0.086	0.102
	(0.023)	(0.255)	(0.022)	(0.277)	(0.075)	(0.100)	(0.108)
Observations	1,525	1,525	1,388	1,388	1,525	1,525	1,525
R^2	0.345	0.385	0.299	0.389	0.330	0.368	0.125
Panel B. New cooperative medical scheme							
Semester	0.084***	0.250	0.078***	0.616**	0.129**	0.082	0.118
	(0.027)	(0.229)	(0.028)	(0.273)	(0.057)	(0.078)	(0.104)
Observations	1,508	1,508	1,372	1,372	1,508	1,508	1,508
R^2	0.351	0.392	0.307	0.395	0.337	0.376	0.126
Panel C. Tax-for-fee reform							
Semester	0.085***	0.232	0.078***	0.555**	0.127**	0.074	0.112
	(0.025)	(0.204)	(0.025)	(0.258)	(0.057)	(0.087)	(0.101)
Observations	1,525	1,525	1,388	1,388	1,525	1,525	1,525
R^2	0.354	0.386	0.304	0.394	0.332	0.372	0.130
Panel D. 2008-2009 Financial Crisis							
Semester	0.084***	0.211	0.080***	0.563**	0.117*	0.071	0.123
	(0.023)	(0.207)	(0.024)	(0.259)	(0.057)	(0.088)	(0.101)
Observations	1,525	1,525	1,388	1,388	1,525	1,525	1,525
R^2	0.344	0.383	0.299	0.387	0.326	0.367	0.124

Note: Robust standard error clustered at the provincial level in parentheses. Except for the full set of controls and fixed effects included in column (3) of Table 4, we further control for average number of primary, junior high and senior high schools per 1000 children, respectively, in Panel A, the interactions between effective years of NCMS and birth year dummies in Panel B and the interactions between effective years of the Tax-for-fee reform and the elimination of agricultural taxes and birth year dummies, repectively, in Panel C. In Panel D we add controls for the province-level changes in log employment between 2004 and 2008 interacted with birth year dummies. *p $<$ 0.1. ** p $<$ 0.05. *** p $<$ 0.01.

Table 8: Effects of the free compulsory education reform by gender and father's education

Variables	In school in 2010 (1)	Years of schl. in 2010 (2)	HS grad. in 2014 (3)	Years of schl. in 2014 (4)	Math (5)	Word (6)	General health (7)
Panel A. Gender							
Semester	0.093***	0.271	0.086***	0.609**	0.134**	0.100	0.098
	(0.024)	(0.213)	(0.024)	(0.254)	(0.063)	(0.087)	(0.110)
Male**Semester*	-0.017	-0.107	-0.017	-0.114	-0.031	-0.054*	0.047
	(0.017)	(0.090)	(0.019)	(0.145)	(0.038)	(0.030)	(0.044)
Observations	1,525	1,525	1,388	1,388	1,525	1,525	1,525
R^2	0.342	0.382	0.299	0.386	0.326	0.369	0.124
Panel B. Fathers' education level							
Semester	0.077**	0.145	0.055*	0.473	0.100	0.071	0.130
	(0.028)	(0.283)	(0.028)	(0.300)	(0.088)	(0.107)	(0.094)
Below junior high**Semester*	0.012	0.179*	0.036***	0.201**	0.043	0.031	-0.024
	(0.010)	(0.094)	(0.012)	(0.085)	(0.032)	(0.029)	(0.056)
Observations	1,429	1,429	1,310	1,310	1,429	1,429	1,429
R^2	0.340	0.385	0.301	0.390	0.324	0.374	0.123

Note: Robust standard errors in parentheses are corrected for clustering at the province level. Each coefficient is from a separate regression estimated with the same specification as in column (4) of Table 4. *p < 0.1. ** p < 0.05. *** p < 0.01.

Appendix A. Supplementary Tables

Table A.1: Potential measurement errors in the treatment variable

Variables	Enter primary school at the proper age	Length of primary school education
Semester	0.038	0.001
	(0.027)	(0.092)
Observations	1,434	1,520
R^2	0.172	0.290

Note: Robust wtandard errors in parentheses are corrected for clustering at the province level. Each coefficient is from a separate regression estimated with the same specification as in column (4) of Table 4. *p < 0.1. ** p < 0.05. *** p < 0.01.

Table A.2: Sample attrition

Variables	Not at home in 2010	Out for work	Out for study
Semester	0.007	-0.007	0.019
	(0.023)	(0.030)	(0.015)
Observations	2,741	2,741	2,741
R^2	0.172	0.140	0.076

Note: Robust standard errors in parentheses are corrected for clustering at the province level. Each coefficient is from a separate regression estimated with the same specification as in column (4) of Table 4. *p < 0.1. ** p < 0.05. *** p < 0.01.

Table A.3: Spillover effects of the reform

Variables	(1) In school in 2010	(2) Years of schl. in 2010	(3) HS grad. in 2014	(4) Years of schl. in 2014	(5) Math	(6) Word	(7) General health
Semester	0.085***	0.140	0.089***	0.527*	0.096	0.073	0.113
	(0.029)	(0.241)	(0.030)	(0.294)	(0.068)	(0.089)	(0.106)
Total semesters of all siblings	-0.001	0.007	0.001	-0.003	0.001	-0.005	0.002
	(0.001)	(0.012)	(0.002)	(0.014)	(0.003)	(0.004)	(0.003)
Observations	1,400	1,400	1,276	1,276	1,400	1,400	1,400
R^2	0.345	0.361	0.290	0.377	0.303	0.356	0.140

Note: Robust standard errors in parentheses are corrected for clustering at the province level. Each coefficient is from a separate regression estimated with the same specification as in column (4) of Table 4. *p < 0.1. ** p < 0.05. *** p < 0.01.

Table A.4: Nonlinearity in the treatment effects

Variables	(1) In school in 2010	(2) Years of schl. in 2010	(3) HS grad. in 2014	(4) Years of schl. in 2014	(5) Math	(6) Word	(7) General health
Semester	0.105***	0.006	0.031	0.371	0.059	-0.072	0.268**
	(0.034)	(0.293)	(0.049)	(0.439)	(0.083)	(0.099)	(0.116)
Semester^2	-0.003	0.033	0.008	0.030	0.009	0.023	-0.023
	(0.003)	(0.025)	(0.008)	(0.045)	(0.013)	(0.014)	(0.024)
Observations	1,525	1,525	1,388	1,388	1,525	1,525	1,525
R^2	0.342	0.382	0.299	0.386	0.326	0.369	0.125

Note: Robust standard errors in parentheses are corrected for clustering at the province level. Each coefficient is from a separate regression estimated with the same specification as in column (4) of Table 4. *p < 0.1. ** p < 0.05. *** p < 0.01.