Who Benefits Most from Education? Evidence from China

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Abstract

I examine heterogeneous returns to post-compulsory education attainment (high school degree or above) in China by exploiting the variation in the educational attainment caused by a reform that introduces compulsory education with different implementation dates across provinces. Using data from China Household Finance Survey (CHFS), I find that individuals who are less likely to have post-compulsory education have higher returns to education. This finding contradicts the common conclusion on post-compulsory education that individuals select them into education based on gains. One explanation for this pattern is that children who are less likely to be enrolled in the education system have more disadvantaged backgrounds and lower wages without educational attainment. Education acts as an equalizer that leads to more homogeneous wages, resulting in larger returns for children who are less likely to be reached by post-compulsory education.

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1 Introducation

Schooling decisions, arguably among the most important choices in a people's life, occur primarily after compulsory education. Some individuals may not choose to continue further education programs for various reasons. Despite that these individuals are hard-to-reach by post-compulsory education, they can be interesting from the policy point of view because they are often directly targeted by the policy increasing post-compulsory education coverage. Their returns to education provide insight into designing and evaluating relevant education policies. Besides knowing the return for the hard-to-reach individuals, policymakers may also be interested in how these individuals benefit differently from education than others. Individuals who are less likely to obtain post-compulsory education can have different returns to education than others because of their different characteristics such as ability and family background. In such a case, a policy that promotes expansion in education enrollment can bring individuals with heterogeneous returns into the education system. Understanding how hard-to-reach individuals benefit differently from education than others, therefore, sheds light on evaluating the economic efficiency of the policy.

Studies (Carneiro et al., 2011; Heckman et al., 2006; Nybom, 2017; Heckman et al., 2018, e.g.,) have found that individuals who are more likely to get post-compulsory education can have higher returns to the education. This finding is in line with the hypothesis that individuals who are more likely to get the education have advantaged characteristics such as higher ability or a lower cost of education, indicating a higher return to education. With higher gains from schooling, individuals are, therefore, more likely to select themselves into education. However, these studies mainly focus on developed economies with different institutional settings and cultures than developing countries, which may predict a different conclusion. For example, when individuals are highly dependent on their families to make post-education decisions due to economic and cultural reasons, the enrollment decision is affected by not only children's return to education but also family interest, such as the opportunity cost of sending children to school and parents' belief in the return to education.

Consequently, individuals who are less likely to obtain post-compulsory education may not be those benefiting less from education.

In this study, I investigate the relationship between the return to education and the likelihood of obtaining education in China. The goal is to better understand the return for individuals who are unlikely to get an education and how the return differs from others'. The education attainment of interest is obtaining at least a high school degree (which I refer to as "post-compulsory education") following the current compulsory education up to middle school. The return of interest in this paper is a monetary return measured by increased wages. Specifically, I apply the marginal treatment effect (MTE) framework introduced by Björklund and Moffitt (1987) and generalized by (Heckman and Vytlacil, 2005, 2001, 1999), which relates the treatment effect (return to obtaining at least high school degree) to the observed and unobserved characteristics that affect the attainment of high school degree or above. Such a framework produces a picture of effect heterogeneity with respect to factors that determine propensity to education attainment, allowing us to compare the return to education for individuals with different likelihoods to obtain an education.

To deal with the endogeneity of educational attainment, I exploit a reform during the 1980s that introduced nationwide compulsory education law and implemented nine-year compulsory schooling in China. As the law requires, children should start their compulsory education at six years old and finish the nine-year schooling, including a six-year primary school and a three-year middle school. Children under the age of 16 when the policy took effect should either complete the nine-year compulsory schooling or turn 16 years old before leaving school. The implementation date was staggered across provinces because of differential provincial economic development. As a result, the reform creates variation from the staggered implementation time across regions and different cohorts. Although compulsory education did not include high school, compulsory schooling equips more children with the knowledge and access to high school entrance exams, translating into a higher probability of pursuing a high school degree or above.

I find substantial heterogeneity in returns to post-compulsory education with respect to both observed and unobserved characteristics determining education attainment. For observed characteristics, take gender as an example. Women are less likely to have post-compulsory education but experience higher returns to education than men, which points to a reverse selection on gains based on gender. This reverse selection on gains is based on not only single observed characteristics but also on the propensity score defined as the likelihood of obtaining education predicted by all observed characteristics. Individuals with lower propensity scores (thus unlikely to get the education explained by the observable characteristics) have higher returns to education. The selection on unobserved characteristics reinforces the finding of reverse selection on gains. Individuals with unobserved characteristics that hinder obtaining post-compulsory education (”high unobserved resistance individuals”) benefit most from education, whereas individuals who are more likely to get the education (”low unobserved resistance individuals”) benefit the least. Consequently, different from the prediction from the standard Roy Model that individuals select themselves into treatment based on their gains, I find that individuals who are least likely to obtain education benefit more than others do.

By digging deeper into the reasons for these findings, I show that higher returns for individuals who are less likely to obtain post-compulsory education (or high resistance individuals) are driven by fewer wages in the untreated status, i.e., without post-compulsory education. However, wages are more homogeneous across individuals after getting the education, generating a larger return for those less likely to obtain the education. Thus, education acts as an equalizer to reduce the wage gap across individuals.

This paper contributes to the growing literature that estimates marginal treatment effects in the context of education. Most current studies focus on the return to college in developed economy (Carneiro et al., 2011; Heckman et al., 2006; Nybom, 2017; Heckman et al., 2018, e.g.,) and a few analyzes the return to upper secondary education in developing economy (Carneiro et al., 2017, e.g.,). These studies find a selection on gains, i.e., individuals who are more likely to get the education have higher returns to education. However, the finding of this study shows that individuals may not select themselves into education based on their gains at the level of high school education or above in China. Individuals can be highly dependent on their families to make enrollment decisions on post-compulsory education (especially high school) due to economic and cultural reasons. Therefore, parents' interest is taken into account when making the enrollment decision, which can deviate the enrollment decision from the one when individuals select them into education based on their gains. Consequently, the relation between selection and gains may be reversed so that individuals with the highest enrollment resistance benefit most from the treatment. The reverse selection on gains is also found by Cornelissen et al. (2018) who studies the return to preschool education in Germany. This paper also provides new evidence of heterogeneous returns to education in China by relating the return to the likelihood of obtaining an education. Existing literature mainly focuses on heterogeneity with respect to some observables. For example, some idiosyncratic characteristics are associated with higher returns: women has higher returns (Ren and Miller, 2012a; Guifu and Hamori, 2009; Magnani and Zhu, 2012; Zhang et al., 2005; Sai, 2003); returns for natives is higher than migrants' (Messinis, 2013; Ren and Miller, 2012b; Zhang et al., 2008; De Brauw and Rozelle, 2008). Moreover, studies also find that return to college education is higher than other degrees (Zhong, 2011; Guifu and Hamori, 2009; Gustafsson and Li, 2000) and an increasing returns over time (Mishra and Smyth, 2013; Li et al., 2012; Zhang et al., 2005; Heckman and Li, 2004; Li, 2003). One study close to this paper is from Heckman and Li (2004) that examines the MTE in the context of education in China. However, they focus on the effect of college attendance based on a small sample ($N = 587$) in 2000, and they find a selection on gains based on their point estimates. To the best of my knowledge, this paper provides the first evidence of the reverse selection on gains at the level of high school degree or above for China.

The remainder of the paper is organized as follows. Section 2 discusses the reform in compulsory education in China. Section 3 summarizes the identification strategy of marginal treatment effect. Section 4 describes the data. Section 5 describes the estimation results. Section 6 discusses the hypothesis that explains the finding in Section 5. Section 6 concludes.

2 Compulsory education reform in China

This paper exploits the variation in compulsory education enforcement across China as a natural experiment. China's Compulsory Education Law was passed on April 12, 1985, and officially went into effect on July 1, 1986. It was the first law that specified nine-year compulsory schooling, including a six-year primary school and a three-year middle school, for the entire country. As the law requires, children should start primary school at six years old and complete nine-year compulsory schooling. Consequently, when the law took effect, children who were 15 years old or younger were required to either finish the nine-year compulsory schooling or turn 16 years old before leaving schooling. The law also had several regulations to reinforce the mandatory nine-year schooling. For example, compulsory education is free of charge for all children, and it became unlawful to employ any child who was still in their compulsory schooling years.

There are two essential features of China's Compulsory Education Law. First, provinces were allowed to have different effective dates for implementing the law. As the central government recognized the differential economic and educational resources across provinces, local government could decide the time to enforce the law's implementation, leaving many local governments in poor provinces or regions with insufficient resources to implement the law later than others. As a result, there are noteworthy variations in the timing of the law's implementation. The first province to implement the nine-year compulsory education was Shanghai, one of the most developed regions, which enforced the nine-year compulsory education in 1985, even before the nationwide nine-year compulsory education passed in 1986. Gansu, one of the least developed provinces in China, started the nine-year compulsory education in 1991 and was also the last province to make the nine-year education compulsory.

Second, children younger than 16 years old when the law took effect should stay in school until they complete the mandatory nine-year schooling or turn 16 years old. Attendance rate in compulsory education is also of interest to the local government because they were responsible for the school enrollment rate. Their promotions might be delayed as a punishment for the low enrollment rate.[1]

Although the compulsory education expansion in 1986 only included primary school and middle school, the reform can also increase the likelihood of obtaining subsequent education programs such as high school or even above. For children not enrolled in middle school (or primary school) in the absence of compulsory education law, the reform can equip them with both prerequisite and access to high school entrance exam, which can, in turn, lead to a higher probability to pursue high school degree or above. The demand for degrees higher than the middle school can also increase in the labor market after middle school becomes compulsory. A higher (than middle school) degree could be more desired in signaling one's ability on the labor market after the middle school degree became compulsory, encouraging higher enrollment in high school or any further program.

In summary, there are two sources of variation from the reform that can be exploited to construct an instrument for the educational outcome (post-compulsory education). On the one hand, children experience different effective dates of the law's implementation in different provinces. Children born in the same year are subject to the nine-year compulsory education in some provinces but not other provinces with a later year of the law's implementation. On the other hand, children from different cohorts are affected by the reform differently. Children born in an early cohort may be too old to be eligible for compulsory education, whereas children born in later cohorts could be affected by the law.

[1] Table 7 lists by province the years of the implementation of the law. Table 7 lists the first eligible birth cohort for the reform, and the cohort older than the first eligible cohort was not affected by the reform.

3 Estimating Marginal Returns to Education

3.1 Baseline Model Setup

Let Y_1 be the potential outcome in treated state ($D = 1$) and Y_0 be the potential outcome in untreated state ($D = 0$). The observed outcome (Y) is the realization of one potential outcome:

$$Y = (1 - D)Y_0 + DY_1 \tag{1}$$

The potential outcomes are specified as:

$$Y_j = \mu_j(X) + U_j, \quad j \in \{0, 1\} \tag{2}$$

where μ_j is a state-specific function of the observable X, and U_j is the unobservable which is normalized to $E[U_j|X] = 0$. Equation 2 indicates that the heterogeneity in the treatment effect $Y_1 - Y_0 = \mu_1(X) - \mu_0(X) + U_1 - U_0$ results from both the observed characteristics X and the unobserved characteristics. This specification defines a more flexible heterogeneity than the commonly used specification in which the treatment D is separately additive to all X (homogeneous treatment effect) and the specification in which the interaction terms between D and X are allowed (heterogeneous treatment effect with respect to only the observable). For selection to treatment (defined in this study as having high school degree or above), the following latent index model is used:

$$I_D = \mu_D(Z) - U_D \tag{3}$$

$$D = 1\{I_D > 0\} \tag{4}$$

where μ_D is a function of $Z \equiv \{X, Z_0\}$, and Z_0 is the instrument(s) for D. μ_D represents the gross benefit of receiving treatment, and U_D represents the cost or the resistance to treatment. In this study, U_D captures not only some unobserved individual characteristics but also some unobserved family background factors that impede educational attainment. The latter could be even more important because the decision on educational attainment, particularly high school enrollment, is heavily affected by parents in family.[2]

In the MTE literature, the distribution of U_D is often normalized to uniform distribuion on an unit interval. As a consequence, function $\mu_D(Z)$ can be interpreted as the propensity score (the probability of receiving treatment conditional on the unobservable Z), $P(Z) \equiv Pr(D = 1|Z) = Pr(\mu_D(Z) > U_D|Z) = \mu_D(Z)$, where the last equality holds when $U_D \sim U(0,1)$. Henceforth, the selection equation for treatment is re-defined as

$$D = 1\{P(Z) > U_D\} \tag{5}$$

MTE as a function of X and U_D accesses the heterogeneous treatment effect as follows:

$$MTE(x,u) = E(Y_1 - Y_0|X = x, U_D = u) = \mu_1(x) - \mu_0(x) + E(U_1 - U_0|X = x, U_D = u) \tag{6}$$

MTE is the average treatment effect for the individual with observed characteristics $X = x$ and unobserved resistance to treatment $U_D = u$ (or the u_{th} quantile of U_D).[3] MTE also allows for the heterogeneity in both the observable X and the unobservable resistance to receive treatment u. In this study, the MTE summarizes the heterogeneous returns to education with respect to the observable (e.g., gender) and the unobservable resistance to educational attainment (e.g., parents' attitude to the return to education). As a consequence, we can directly examine the heterogeneous returns to education with respect to the likelihood of obtaining education that is described by X and U_D, which is the treatment effect of interest

[2] The decision on the high school enrollment is usually made when a student is 15 years old in China, and the child is still highly financially dependent on their parents.

[3] MTE is defined on *Marginal* individuals in receiving treatment because individuals with $U_D = u$ are also ones with $\{P(Z) = u\} \cap \{I_D = 0\}$ (indifferent in receiving treatment with propensity score u).

in this study. Moreover, compared to the average treatment effect for the whole population, MTE focuses on a more granular subpopulation and thus can be used to construct some other treatment effects of interest. For example, with a binary instrument Z_0 that shifts the propensity score from $p_0(x) \equiv Pr(D = 1|X = x, Z_0 = 0)$ to $p_1(x) \equiv Pr(D = 1|X = x, Z_0 = 1)$, Local Average Treatment Effect (LATE) based on Wald estimator is the average of MTEs for a subgroup of individuals:

$$\begin{aligned} LATE(x) &= \frac{E(Y|Z_0 = 1, X = x) - E(Y|Z_0 = 0, X = x)}{E(D|Z_0 = 1, X = x) - E(D|Z_0 = 0, X = x)} \\ &= \frac{1}{p_1(x) - p_0(x)} \int_{p_0(x)}^{p_1(x)} MTE(x, p)dp \end{aligned} \tag{7}$$

3.2 Identification

One way of identifying MTE is using the method of local IV developed by Heckman and Vytlacil (1999, 2001, 2005). This method identifies MTE as the derivative of the conditional expectation of Y with respect to the propensity score. More precisely, we have

$$\begin{aligned} E(Y|X = x, P(Z) = p) &= \mu_0(x) + p(\mu_1(x) - \mu_0(x)) + pE(U_1 - U_0|X = x, U_D \leq p) \\ &= \mu_0(x) + p(\mu_1(x) - \mu_0(x)) + K(x, p) \end{aligned} \tag{8}$$

where $K(x, p) \equiv pE(U_1 - U_0|X = x, U_D \leq p)$. $K(x, p)$ is a function of X and p that captures heterogeneity along the unobserved resistance to treatment U_D. Taking the derivative of Equation 8 with respect to p and evaluating it at u, we get MTE

$$\begin{aligned} MTE(X = x, U_D = u) &= \frac{\partial E(Y|X = x, P(Z) = p)}{\partial p}|_{p=u} \\ &= \mu_1(x) - \mu_0(x) + k(x, u) \end{aligned} \tag{9}$$

where $k(x, u) = E(U_1 - U_0|X = x, U_D = u)$. Intuitively, conditioning on $X = x$, when an infinitesimal shift occurs in the propensity score at p (changing the treatment status from untreated state to treated state), the corresponding change in Y is the treatment effect for individuals who have $X = x$ and have p as the propensity score (or unobserved resistance), which is exactly MTE. Equation 9 also indicates that, without further assumptions, we need additional variation conditional on X to identify $\mu_1(x) - \mu_0(x)$ and $k(x, u)$ separately to identify MTE. This additional variation comes from the excluded instrument Z_0, and $MTE(x, p)$ is identified under the following assumption on the instrument.

Assumption 1 (U_0, U_1, U_D) *is independent of* Z_0*, conditional on* X

The conditional independence assumption requires that the instrument is independent of the unobservable in the outcome equations and the selection equation. The conditional independence between Z and (U_0, U_1, U_D) implies and is also implied by the standard IV assumptions of conditional independence and monotonicity (Vytlacil 2002).

Besides the assumptions that are required in the literature using Instrumental Variable (IV), there are often more assumptions in estimating MTE. The local IV estimator motivated by Equation 9 indicates that the support of the propensity score P conditional on X determines the support of the unobserved resistance U_D in MTE. Therefore, substantial variation in P conditional on X (which solely comes from the excluded instrument Z_0) is needed to identify $MTE(x, u)$ on a wide range of $U_D \in [0, 1]$. For this reason, additional assumptions are usually required, e.g., at least one of the instruments is continuous, which makes it possible to have a full support in MTE. However, it can be challenging to find proper continuous instrument(s) with sufficient variation conditional on observed covariates in many empirical studies, including this study. In the case of discrete instrumental variables, alternative approaches include restricting the specifications in the model and specifying a less flexible relation among random variables.[4] Following Brinch (2017), I impose the second assumption as follows:

Assumption 2 $E(Y_j | U_D, X = x) = \mu_j(x) + E(U_j | U_D), \quad j \in \{0, 1\}$

[4]See a more detailed discussion in Brinch (2017)

Assumption 2 specifies a more restrictive version of Equation 2 because it implies that the observable and the unobservable contribute to the potential outcome in a substitute manner. consequently, MTE in Equation 6 can be written as

$$MTE(x, u) = \mu_1(x) - \mu_0(x) + E(U_1 - U_0 | U_D = u) \tag{10}$$

Equation 10 implies that $MTE(x, u)$ can be identified over the support of u, which is determined by the support of the estimated propensity score P, unconditional on X. Therefore, Assumption 2 makes the discrete instrumental variable feasible in identifying MTE.

After imposing Assumption 2, the treatment effect is still allowed to vary by X and U_D but not by the interaction between the two, and it is weaker than the additive separability assumption between D and X, which is commonly used in empirical analysis such as a linear specification $Y = \alpha D + \beta X + U$. Furthermore, Assumption 2 is implied by (but does not imply) the full independence assumption about random variables, i.e., $(Z, X \perp U_0, U_1, U_D)$ which is assumed in some applied works estimating MTE.

Assumption 2 holds when there is no endogenous variable in X in the outcome (wage) equation, which is also required in many applied works like the standard IV estimation approach. Furthermore, the separability assumption can also be motivated by the typically assumed technology function for the earning equation in which the observed and unobserved characteristics contribute to human capital in a substitute manner.[5]

[5] In the Mincer-type human capital model (Mincer, 1974), the earning is a function of human capital and other unobserved characteristics:

$$Y = wHe^u$$

where Y is the earning, w is the wage rate per unit of human capital, H is human capital, and u is other unobserved determinants of earning. Human capital H is a function of schooling S, working experience E and its square, and other unobserved characteristics v.

$$H = e^{\beta_1 S + \beta_2 E + \beta_3 E^2 + \Gamma X + v}$$

Then the logrithm of earning function can be written as:

$$Ln(Y) = \beta_0 + \beta_1 S + \beta_2 E + \beta_3 E^2 + \Gamma X + \epsilon$$

where $\beta_0 = Ln(w)$ and $\epsilon = u + v$.

In the Mincer-type earning function, observed and unobserved characteristics v contribute to human capital

Under Assumption 1 and 2, we have:

$$\begin{aligned} E(Y|X=x,P(Z)=p) &= \mu_0(x) + p(\mu_1(x) - \mu_0(x)) + K(p) \\ MTE(x,p) = \frac{\partial E(Y|P(Z)=p, X=x)}{\partial p} &= \mu_1(x) - \mu_0(x) + k(p) \end{aligned} \tag{11}$$

where $K(p) = pE(U_1 - U_0|U_D \leq p)$ and $k(p) = E(U_1 - U_0|U_D = p)$.

3.3 Estimation procedures

Equation 11 suggests the following estimation procedures: We start by estimating the propensity score $\widehat{P}(Z)$ based on Equation 4 using a probability model such as probit or logit model. We then make assumptions about the functional form of the unknown function μ_1, μ_0 and $K(p)$. With these assumed functional forms, we estimate $\widehat{\mu_0}$, $\widehat{\mu_1 - \mu_0}$ and $\widehat{K}(p)$ separately based on the equation $E(Y|X=x, P=p)$ in Equation 11. Last, we calculate MTE by taking the derivative with respect to p.

In the main specification, the propensity score P is estimated from the the logistic regression. Both μ_0 and μ_1 are specified to be linear: $\mu_0(x) = \beta_0 x$ and $\mu_1(x) = \beta_1 x$. Thereby, the conditional expectation of Y is written as:

$$E(Y|X=x,P(Z)=p) = x\beta_0 + x(\beta_1 - \beta_0)p + K(p) \tag{12}$$

Furthermore, $K(p)$ is specified as a polynomial function of p with order 2 in the main specification. Note that MTE is then a linear formula in p as follows:

$$MTE(x,u) = x(\beta_1 - \beta_0) + \gamma u \tag{13}$$

in a substitute manner. For example, one of the factors explained by v is the parental investement, and it can be substituted by formal schooling in forming the individual's human capital. Unobserved characteristics u which includes labor market shocks are also assumed to be not interactive with the determinants of human capital. Therefore, when the outcome is logrithm of wage, Assumption 2 which does not allow for interaction between observable X and unobservable U_D is in line with the set-up of the typically assumed Mincer-like techonology function for earnings.

$(\beta_1 - \beta_0)$ captures the heterogeneous treatment effects with respect to the observable characteristics X, while γ corresponds to the heterogeneous treatment effects with respect to the unobserved resistance to treatment. A negative γ indicates that the treatment effect is larger for those who are more likely to be selected to treatment because of lower unobserved resistance to treatment, which is in line with the prediction of the Roy Model, namely selection on gains. On the contrary, a positive γ indicates the reverse selection on gains, i.e., individuals who are less likely to receive the treatment due to the higher unobserved resistance are with larger treatment effects.

For robustness checks, I choose alternative orders (3 and 4) for the polynomial function $K(p)$. Moreover, to allow for a more flexible specification of $K(p)$, I estimate Equation 11 semi-parametrically using Double residual regression (Robinson, 1988). The last alternative estimation approach is to assume a joint normal distribution among (U_0, U_1, U_D) which is also used in some applied works estimating MTE.[6]

4 Data and variables

The data comes from China Household Finance Survey (CFHS), which is a biannual panel survey starting from 2011. The survey mainly collects information about respondents' basic demographics, income, financial assets, consumption, and other components of wealth. A national representative sample of 8,438 households participate in the first wave of the survey in 2011, and the number of participants increases to 40,000 in wave 4 in 2017.

4.1 Sample restriction

This study focuses on 260,195 individual-year observations from wave 3 and 4. I first restrict the sample to 230,793 observations who did not migrate across provinces. The exclusion of migrants across provinces is to deal with the concern that the migration pattern can be

[6] See more of these two alternative estimation approaches in Appendix.

non-random, which invalidates the instrumental variable constructed based on the location (province) information. I then restrict the sample to 52,842 observations aged between 31 and 47 to alleviate the concern that the instrument is less pertinent to the reform of insterest in this study when a too wide range of cohorts are included. Section 4.3 provides a more detailed discussion on this restriction. Last, I keep 25,982 observations with valid information on all variables used in the analysis.[7]

4.2 Outcome: Wage

The outcome variable wage is measured by the monthly after-tax salary in the last year. This measurement is constructed as the ratio of the yearly after-tax salary to the period (in month) of employment in the last year. Note that this wage information is only available for respondents who were on the labor market and who were also employees or doing freelance jobs. This criterion excludes people who were not on the labor market and those engaged in farming or self-employed.[8] Respondents choose an interval to report their wages (e.g., between 50k and 100k) when they are reluctant to provide an accurate number. In such case, the wage is measured by: (1) $\frac{a_0 + a_1}{2}$ when interval (a_0, a_1) is chosen (2) $\frac{a_0}{2}$ when interval "below a_0" is chosen (3) a_1 when interval "above a_1" is chosen. There are about 7% of the respondents who only report their wages by the intervals in our sample.

4.3 Educational attainment

The main independent variable of interest is educational attainment. I use a binary variable that equals one if the respondent's highest obtained academic degree is high school or above

[7]The province where respondent lived during school ages are proxied by the province information documented in Hukou, a system of household registration in China, after only focusing on those who did not change their location in Hukou to another province. The sample restriction of non-missing variables drops 26,688 observations due to missing information on salary and 172 observations due to missing information on other variables. Missing information on salary is mainly because the respondenteither either did not work for paid job or were doing agricultural jobs or self-employed. However, I find that the association between the selection and the instrument is close to zero and insignificant. See a more detailed discussion on the definition of migration and the sample restriction in Appendix.

[8]See a more detailed discussion on the definition of migration and the sample restriction in Appendix.

and zero otherwise. There are two types of tracks following the middle school accomplishment: *normal high school* and *vocational high school*. The major difference between these two is the curriculum. The normal high school is academic-oriented and mainly prepares students for the national entrance exam to colleges or universities, whereas the vocational high school mainly prepares students for occupational skills.[9] In this paper, the binary variable to measure educational attainment equals one if the respondent obtains at least a normal high school degree or vocational high school degree or above. Because compulsory education only includes primary school and middle school, the return to education in this study can also be interpreted as the return to post-compulsory education.

4.4 Instrument for educational attainment

As for the instrument for educational attainment, I follow Ma (2019),

$$Z_0 \equiv \begin{cases} 0 & if \quad Cohort < Cohort_1 \\ \frac{Cohort - Cohort_1 + 1}{10} & if \quad Cohort_1 \leq Cohort \leq Cohort_9 \\ 1 & if \quad Cohort > Cohort_9 \end{cases} \tag{14}$$

where *cohort* indicates the birth year, $Cohort_1$ is the birth year of the oldest affected cohort (people who were 15 years old when the policy took effect in most provinces), and $Cohort_9$ is the birth year of the last affected cohort (people who were six years old when the policy takes effect in most provinces). This linear extrapolation of the exposure is based on the finding that the more years of the child eligible to compulsory education, the larger the potential effect of the compulsory education law is (Ma, 2019).

For the instrument to be valid, the changes in education policies and the exposure to such

[9] Despite that it is much more likely to pursue further education for those with academic degree compared to those with the vocational degree, the vocational degree does not sufficiently mean a lower wage in years 1980s and mid-1990s, which is also the period to make decisions on academic/vocational high school for respondents in the sample of this study. During that period, the vocational degree was sometimes even more rewarding because it usually gave people a decent job requiring technical skills, often regarded as a stable and higher-income job in the economy dominated by state-owned enterprises.

policy changes should be independent of any unobserved characteristic that explains wage. If some provincial factors other than compulsory education expansion affects labor income in the same way as compulsory education does, the instrument is no longer valid. Moreover, the validity of the instrument is also violated when not controlling for the age trend in the wage pattern. Younger cohorts more likely to be affected by the reform could also have different wages because they have less working experience. Therefore, I control for the age pattern of the income by including age and age quadratic. Differential economic development can lead to the disparity in the timing of the compulsory education law's implementation. So I have province-fixed effects to capture the province-level time-invariant heterogeneity and province-specific age trend to capture the deviation from the national trend.

To make the instrument more convincing, I apply two types of sample restrictions. First, I only focus on individuals who did not migrate across provinces to alleviate the concern that the non-random migration can undermine the instrument's validity. Migration patterns could depend on individuals' ability or family background, which also influences their wages. Therefore, I keep individuals who did not migrate or only migrate within the province. Second, I keep individuals whose ages are between 31 and 47. As defined in Equation 14, when including a too wide range of ages, Z is dominated by 0 or 1 and less pertinent to compulsory education expansion of interest. Table 9 shows the frequency distribution of Z by different ages between 31 and 47. For each age cohort, we can always find some variations in Z, which means that the variation of the instrument Z does not solely come from the comparison between different birth cohorts.

In addition, as a robustness check, I take into account one particular confounding policy: One Child Policy (OCP) which was implemented after 1978 with different strictness and effectiveness across provinces. The OCP can have an impact on multiple outcomes, including education and labor income of children (Ebenstein (2010)).

4.5 Control variables

Besides age, age quadratic, province fixed effect, and province-specific age trend, I also control several individual-level characteristics, including gender, marital status, Hukou status, and wave dummy. Hukou is a household registration system in China, certifying that the holder is the legal resident of a particular area, especially of the rural or urban area. Hukou status is a binary variable that equals one if an individual belongs to the rural area and zero otherwise. Last, upon the province fixed effects and the province-specific age trend, I also control for the law effectiveness or program intensity by the local educational attainment before the law enforcement. More precisely, I include the average schooling years of the cohorts born five or fewer years before the oldest (first) affected cohort by the law in rural or urban areas in a particular province.

4.6 Summary

Table 1 shows the summary statistics of all variables. Slightly more than half of the respondents have a high school degree or above, and all respondents earn about 587 dollars as the salary after tax on average. The respondents are exposed to the reform by 0.526, which indicates that the respondents are about five years younger than the oldest affected cohort. For all respondents with slightly more men, the average age is about 39, and most of them have got married. There are more respondents with urban Hukou. The average schooling years of the cohorts born five or fewer years prior to the oldest affected cohort is about 9.844 years which means that these cohorts have also reached middle school degrees (9 years of schooling) even though they are not affected by the reform. There are also slightly more observables from wave 3. Table 1 also shows the summary statistics by educational attainment. There are mainly two noticeable differences. First, the respondents with a high school degree or above earn about 200 dollars more than those without a high school degree. Second, 74% of the respondents who do not have high school degrees have rural Hukou, whereas only 17% of the respondents who have high school degrees or above have rural Hukou.

Table 1: Summary statistics

Variable	All	Obtain at least high school degree	
		No	Yes
Outcome variable			
Monthly salary ($)	587.305	466.314	686.855
	(536.000)	(383.803)	(616.881)
Treatment variable			
Obtain high school degree	0.549	-	-
	(0.498)	-	-
Instrumental variable			
Exposure to the reform	0.526	0.453	0.587
	(0.395)	(0.389)	(0.389)
Covariates			
Age	39.302	40.139	38.613
	(4.989)	(4.899)	(4.958)
Male	0.570	0.595	0.549
	(0.495)	(0.491)	(0.498)
Married	0.916	0.919	0.914
	(0.277)	(0.272)	(0.280)
Rural Hukou	0.43	0.740	0.174
	(0.495)	(0.438)	(0.379)
Average years of schooling for ineligible cohorts	9.844	8.482	10.965
	(2.185)	(1.921)	(1.698)
Participate in wave 4	0.493	0.490	0.495
	(0.5)	(0.500)	(0.500)
Number of observations	25,982	11,728	14,254

Sample average is in number, and the standard deviation is in parenthesis. Monthly salary is measured in US dollar based on the exchange rate 1 US dollar $\approx$ 6.91 Chinese Yuan.

5 Results

5.1 First stage estimation

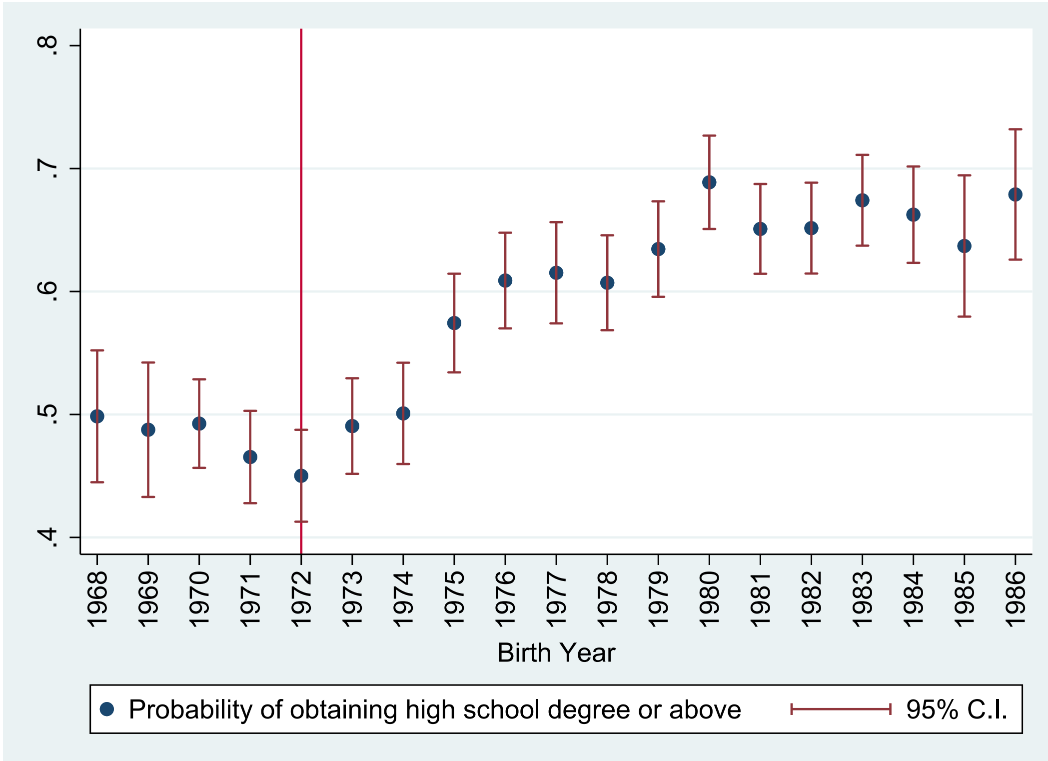
Given the implementation year of the law, individuals affected by the compulsory education law are expected to have higher educational attainment than older cohorts who are not affected by the law. With more years eligible for compulsory education, the educational attainment is also expected to be higher, up to fully affected by the law. Figure 1 shows the probability of obtaining post-compulsory education by birth cohorts based on individuals living in the provinces when the compulsory education law took effect in 1987.[10] There is no apparent increasing trend in the educational attainment for cohorts that are older than the first affected cohort, i.e., cohort aged 15 when the law takes effect (indicated by the red vertical line). Starting from the first affected cohort (1972), the probability of obtaining post-compulsory education increases over cohorts, even though the increase for the first two affected cohorts is limited. For the fully affected cohorts, i.e., cohorts younger than the last affected cohort (1981), there is no evident increase in educational attainment.

Even though compulsory education law aimed at all children who are eligible regardless of gender, it may have different effects on men and women for various reasons, such as the inequality in educational opportunity between males and females. Figure 2 profiles the average educational attainment (obtaining a high school degree or above) by the value of the instrumental variable. The same exposure to the law is associated with higher educational attainment for women than men, at almost all levels of the exposure. One explanation for this heterogeneity is that the lower education level prior to the reform for women leads to a more considerable impact of the compulsory education law. In the subsample of respondents who are not affected by the reform, the probability of obtaining at least a middle school degree is 0.03 higher for women than men significantly.[11]

[10] These observations are the largest subsample defined by the implementation year of the law, accounting for about 40% of the whole sample.

[11] The significant difference is from the OLS estimation of educational attainment (middle school degree or above) on gender, conditional on age and province fixed effects. The estimated parameter corresponding

Figure 1: Educational attainment in provinces implementing the law in 1987



Therefore, I introduce an additional term $Z \times$ Male where Male is the dummy for gender to capture the heterogeneous effects of compulsory education on children's educational attainment by gender. The estimated parameters of the first-stage logit model in the main specification are displayed in column (1) of Table 2. To ease the interpretation, I report the marginal effects of the instruments while fixing all other covariates at sample means. Individuals who are more exposed to the compulsory education law have a higher probability of obtaining a high school degree or above. Conditional on all covariates, a woman who is fully affected by the reform, i.e., six years old or younger when the compulsory education law took effect, has around 22% higher chance to obtain post-compulsory education than a female who is not affected by the reform. Moreover, the same exposure leads to around 11.4% higher chance to get post-compulsory education for women than men, or equivalently, the effect of the exposure is almost double for women compared to men.

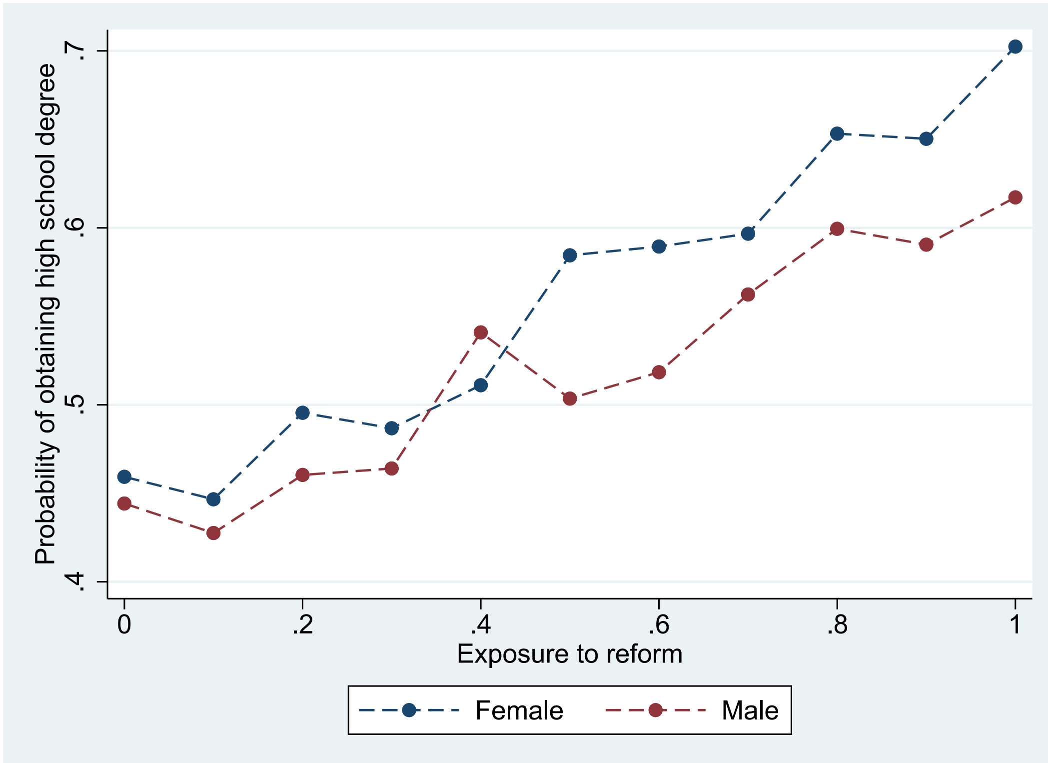
to gender is 0.03 and significant at 5% significance level.

Table 2: First stage estimation results: Marginal effects at means from the logistic regression

	(1)	(2)	(3)	(4)
	Obtain high school degree or above			
Exposure	0.220***	0.204***	0.184***	0.175***
	(0.049)	(0.050)	(0.034)	(0.034)
Exposure × Male	-0.114***	-0.115***	-0.102***	-0.102***
	(0.020)	(0.020)	(0.019)	(0.019)
χ^2 for test of the excluded instruments	42.55	40.68	44.96	42.97
p-value for test of the excluded instruments	0.0000	0.0000	0.0000	0.0000
Control for One-child-policy	No	Yes	No	Yes
Alternative measure for the exposure	No	No	Yes	Yes
Observations	26,582	26,582	26,582	26,582

Robust standard errors in parentheses are clustered at household level. All regressions include covariates: age, age squre, gender, marital status, hukou status (rural/non-rural), province fixed effect, linear province-specific age trend, wave dummy and average schooling years of those who are ineligible to the reform. One-child-policy includes two measures (amount of the fine and the duration of the fine) for the intensity of the implementation of the one child policy (See more in Ebenstein (2010)). The alternative measure for the exposure is the same as the one in the main specification (Z_0 defined in Equation 14) when $Z_0 \geq 0.4$ and equals zero when $Z_0 \leq 0.3$. The main specification for this study is based on Column (1). See more discussion on these two alternative specification in the robustness check section.

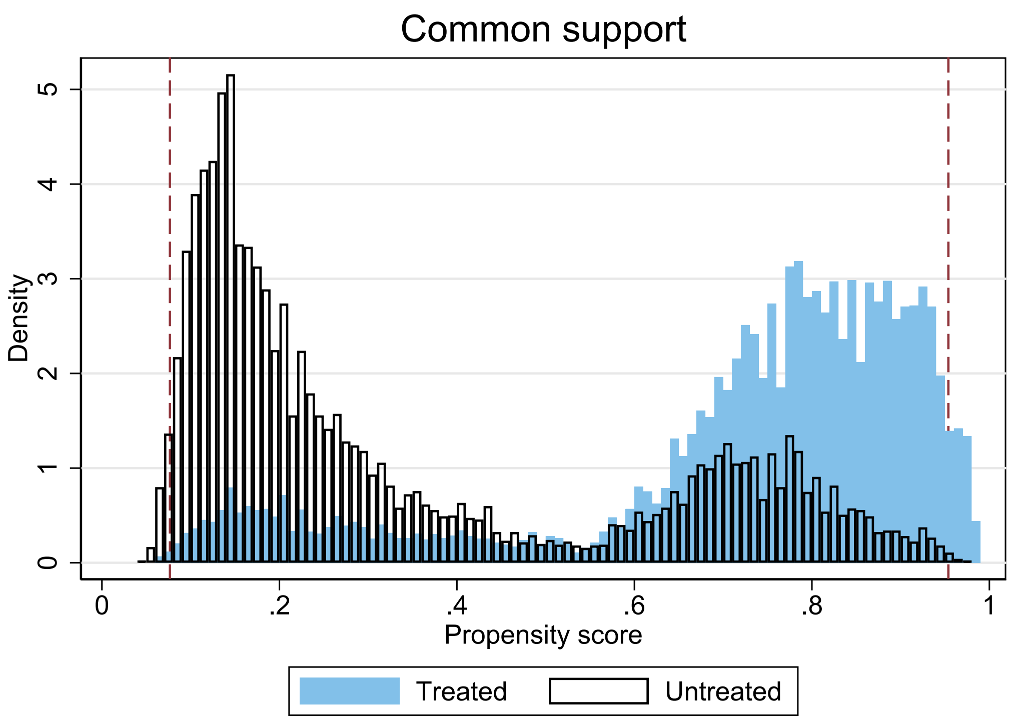
Figure 2: Exposure to reform by gender



The first-stage estimation generates sizable common support for the propensity score $P(Z)$ as shown in Figure 3. The estimated propensity score in the common support, namely the overlapped set of $P(Z)$ between treated and untreated, ranges from 0.06 to 0.97. Without additional parametric assumptions on curvature, MTE can only be identified up to the range of common support of $P(Z)$. For the range outside the common support or the common support with a few observations, the identification of MTE is totally or heavily determined by the parametric assumption. Therefore, to further ease the concern that the identification heavily rests on the arbitrary parametric specification, I trim the points of support with the 0.1% lowest densities and construct the common support as the points of overlapping support between the treated and untreated. As a result, the common support after trimming ranges from 0.08 to 0.95.[12]

[12] A relatively small fraction of observations (27 out of 25,882) are dropped due to the trimming. After removing these 27 observations, I fit the baseline propensity score model on the trimmed sample again. A similar trimming strategy is used by Nybom (2017).

Figure 3: Estimated propensity score



5.2 Treatment Effect Heterogeneity in observed and unobserved characteristics

Table 3 summarizes the heterogeneous returns to education in the main specification. First, I find heterogeneous returns to education with respect to observable. For example, the return to education for women is about 30% larger than men's, which is in line with the finding from some studies (Ren and Miller, 2012a; Guifu and Hamori, 2009, e.g.,). One possible explanation is that, compared to men, women without a high school degree or above find it more difficult to find a job with the same salary due to reasons such as gender discrimination in the labor market. Thus, education has a larger effect on women in terms of wages. Moreover, the significant estimates corresponding to age indicate that different cohorts have heterogeneous returns to education, which is also found by studies (Mishra and Smyth, 2013; Li et al., 2012, e.g.,).

I also find the heterogeneity with respect to the unobservable resistance to educational attainment. A significantly positive γ_1 indicates that the individual who is less likely to obtain post-compulsory education due to higher unobserved resistance U_D has a higher return to education. Individuals with higher U_D are those who are unlikely to obtain the post-compulsory education, conditional on all observables listed in Table 3, for various reasons. They might face a tighter budget constraint from their families, leading to a higher unobserved cost in obtaining a high school education. Or they have lower anticipation of the return to education. Overall, we find that individuals less likely to get post-compulsory education due to the higher unobservable resistance benefit even more from it than others. Namely, we find the reverse selection on gains based on unobserved resistance to education.

The pattern of reverse selection on gains based on the unobservable is not only significant but also sizable. $\gamma_1 = 2.752$ shows a considerable heterogeneity: the average return to education for individuals with the most 25% unobserved resistance ($0.75 \leq u \leq 1$) is almost three times as it for individuals with the least 25% unobserved resistance ($0 \leq u \leq 0.25$).[13]. The estimate based on Table 3 is comparable to the results from the literature. MTE can be used to construct LATE, which can also be estimated by a standard IV estimation approach. Table 3 summarizes the LATE estimated by two different approaches. The point estimate of the LATE estimator constructed based on MTE is 0.856, which is comparable to the one obtained by 2SLS, which is 1.083. Therefore, for compliers whose post-compulsory education enrollment is in line with the exposure to the reform, the high school degree or above almost doubles the salary after tax. This LATE estimate is also comparable to the estimates from other studies (Chen et al., 2020; Mishra and Smyth, 2013). For example, Mishra and Smyth (2013) find that an additional year of schooling leads to an 18.31% increase in income.[14]

A positive γ_1 indicates a reverse selection on gains based on the unobservable. Is there a similar pattern with respect to the observable? To answer this question, I investigate the relationship between the return to education and the likelihood of obtaining the education

[13] $2.752 \times ((1 + 0.75)/2 - (0 + 0.25)/2) = 2.064$

[14] Note that the average schooling years of the treated group (obtain a high school degree or above) is about 6.3 years higher than it of the control group in this study, which means that the difference in salary between the two groups is about $0.1831 \times 6.3 \approx 1.15$ times.

explained by the observable. Specifically, following Zhou and Xie (2019), I summarize the likelihood explained by the observable by the propensity score $P(D = 1|X)$ which is the prediction of the post-compulsory education enrollment based on the observed characteristics.[15] Then the correlation between the propensity score and the return to education contributed by the observable, i.e., $(\beta_1 - \beta_0)X$, shows the relation of interest. For example, a negative correlation between $(\beta_1 - \beta_0)X$ and propensity score indicates a negative correlation between the MTE and the propensity score, which means that the individual who is less likely to obtain post-compulsory education explained by the observable has a higher return to education. Table 4 confirms such negative correlation. In other words, we also find the reverse selection on gains based on the observable. Henceforth, we can conclude that individuals who are less likely to obtain post-compulsory explained by both the observable and the unobservable resistance have higher returns education.

5.3 Robustness checks

The pattern of the reverse selection on gains is robust to a number of robustness checks. First, I specify a more flexible polynomial function of $k(u)$ in MTE defined in Equation 11. In the main specification, $k(u)$ is assumed to be a linear, i.e., $k(u) = \gamma u$. As a robustness check, I specify $k(u)$ as a polynomial function with order 2 and 3. Figure 4 shows the MTE as a function of the unobserved resistance.[16] The curvature of these different specifications is also significant, as shown in Table 5 which summarizes the estimation results of MTE with different specifications in the polynomial function $k(u)$. We find significant heterogeneity in the unobserved resistance, which stands for the significant curvature in MTE.

The pattern of the reverse selection on gains can still be found in the baseline specification

[15] The key idea of the refined MTE introduced by Zhou and Xie (2019) is that the latent index structure in the choice making equation implies that all the treatment effect heterogeneity occurs along only two dimensions: (1) the propensity score $P(D = 1|X)$ and (2) the unobserved resistance to treatment U_D. They also prove that we can replace the multi-dimension observed characteristics by the propensity score without loss of generosity. See more in their paper.

[16] A even higher orders (4 and 5) in the polynomial function give similar results as the specification with order 3.

Table 3: Estimation results of MTE

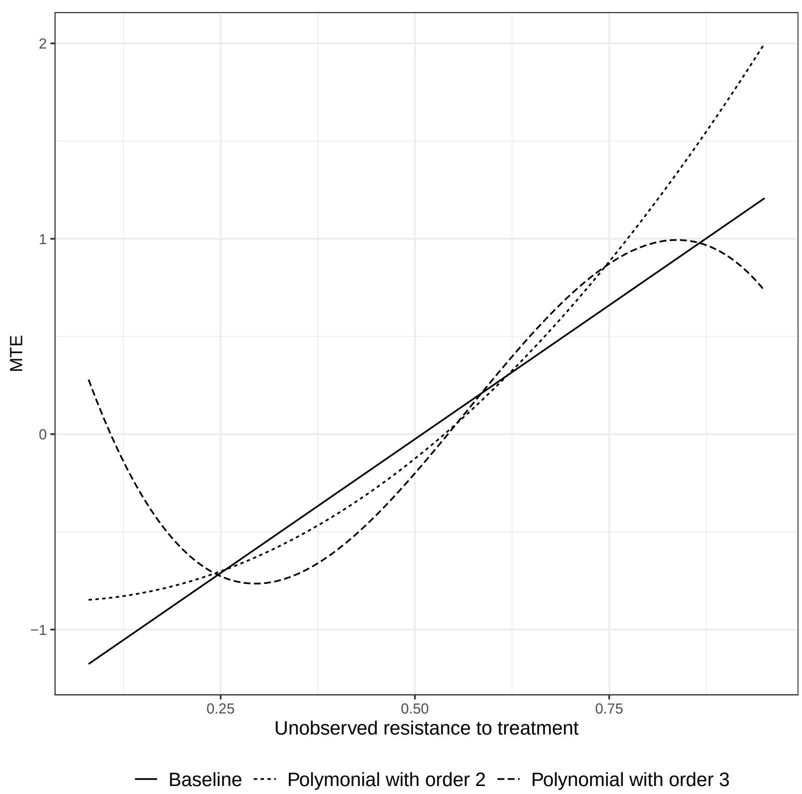
$MTE(x,u) = (\beta_1 - \beta_0)x + k(u)$			
	Coefficient	Std. Err.	P-value
$\boldsymbol{\beta_1 - \beta_0}$			
Age	0.182	0.064	0.004
Age square	-0.002	0.001	0.011
Male	-0.292	0.027	0.000
Currently married	-0.148	0.057	0.009
Rural Hukou	1.484	0.923	0.108
Average years of schooling for ineligible cohorts	0.321	0.252	0.203
Participation in Wave 4	0.060	0.029	0.036
Constant	-7.881	4.271	0.065
$\boldsymbol{k(u) = \gamma_1 u}$			
γ_1	2.752	1.384	0.047
LATE (based on MTE)	0.856	0.221	0.000
LATE (based on 2SLS)	1.083	0.250	0.000
Test of observable heterogeneity			0.0000
Test of essential heterogeneity			0.0467

The estimation include age, age square, gender, marital status, rural/urban Hukou, average years of schooling for ineligible cohorts, wave dummy and province fixed effect as the covariates. Bootstrap standard error is clustered at household level with 1999 draws. The null hypothesis of the test of observable heterogeneity is that $\beta_1 - \beta_0 = 0$ are jointly true for all observable. The null hypothesis of the test of essential heterogeneity is that $\gamma_1 = 0$.

Table 4: The correlation between $(\beta_1 - \beta_0)X$ and the propensity score from OLS

$(\beta_1 - \beta_0)X$	Coef	Std. Err.	P-value	95% Conf. Interval
Propensity score	-0.351	0.007	0.000	[-0.365, -0.337]
Constant	0.164	0.004	0.000	[0.155, 0.173]

Figure 4: Estimation results of MTE with different orders in the polynomial function $k(u)$.



The polynomial relation between the unobserved resistance to treatment and MTE is based on Table 5 while fixing all covariates at the sample averages. The baseline model specifies a polynomial with order 1.

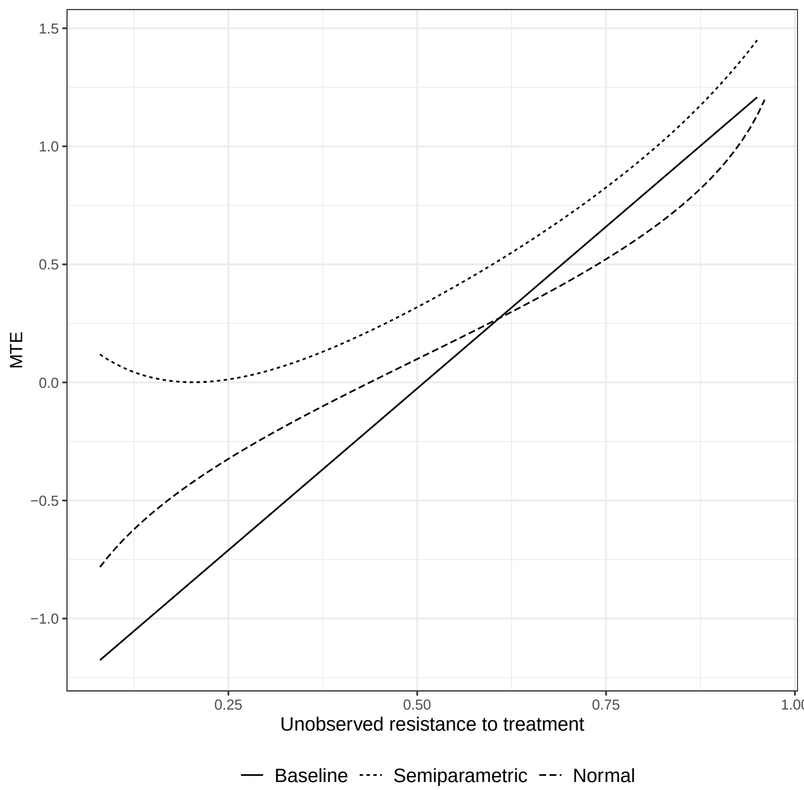
($k(u)$ as a polynomial function with order 1) and in the specification with $k(u)$ as a polynomial function with order 2. But the MTE is no longer a monotonically increasing function of the unobserved resistance when specifying $k(u)$ as a polynomial function with order 3. However, we can still draw the conclusion, to some extent, that the average return to education for individuals with relatively higher unobserved resistance is higher than it for those with relatively lower unobserved resistance. Specifically, I calculate the average of MTEs with the unobserved resistance u in the 4th quartile, i.e., the average return to education for those with the most 25% unobserved resistance, and compare it to the average MTE with the unobserved resistance in other quartiles. I test the hypothesis that the average return for individuals with the most 25% unobserved resistance is higher than it for other individuals (with unobserved resistance $u \leq 75\%$, or with $u \leq 25\%$, or with $25\% \leq u \leq 75\%$). As shown in Table 5, we find that the average MTE with the most 25% unobserved resistance is significantly higher than it of all other groups in all scenarios except for one case.[17] Overall, I find that the average return to education for individuals with the most 25% unobserved resistance is higher than it for other groups of individuals in most cases.

Second, the pattern of reverse selection on gains is also robust to alternative estimation approaches for MTE. Rather than specifying $k(u)$ as a polynomial function, I estimate it semi-parametrically without assuming a parametric specification using Double Residual Regression (Robinson, 1988). Moreover, I assume a joint normal distribution among unobservable U_0, U_1, U_D, which gives the analytic formula of $k(u)$ as a function of parameters that can be estimated.[18] Figure 5 shows the estimated MTE as a function of the unobserved resistance. Similar to the baseline specification, we also find the reverse selection on gains, i.e., the return to education is larger for individuals with higher unobserved resistance.

[17] When specifying $k(u)$ as a polynomial function with order 3, I find that the average return to education for individuals with the most 25% unobserved resistance is significantly higher than it for all other individuals ($u < 75\%$) and for individuals with $25\% \leq u \leq 75\%$, though I cannot conclude that the average return for individuals with the least 25% unobserved resistance is lower than it for individuals with the most 25% unobserved resistance.

[18] See more details of these two estimation approaches in Appendix.

Figure 5: Estimation results of MTE with alternative identification strategies on $k(u)$



The baseline model specifies a polynomial with order 1. Semiparametric method does not assume the formula of $K(u)$ which determines the relation between the unobserved resistance to treatment and the MTE. Normal method assumes a joint normal distribution among error terms U_0, U_1, V. See a more detailed discussion of these two alternative methods in Appendix.

Table 5: Estimation results of MTE

$MTE(x,u) = (\beta_1 - \beta_0)x + k(u)$	Baseline	Quadratic	Cubic
$\boldsymbol{k(u) = \sum_1^p \gamma_i u^i,\ p \in \{1,2,3\}}$			
γ_1	2.752**	-0.281	-16.294*
	(1.384)	(2.486)	(9.525)
γ_2		3.451	37.425*
		(2.456)	(19.677)
γ_3			-22.056*
			(12.823)
Test of observable heterogeneity	0.0000	0.0000	0.0000
Test of essential heterogeneity	0.0467	0.0572	0.0297
$MTE_{Q4} > MTE_{Q123}$	0.0197	0.0070	0.0852
$MTE_{Q4} > MTE_{Q23}$	0.0193	0.0069	0.0680
$MTE_{Q4} > MTE_{Q1}$	0.0229	0.0113	0.1381

All estimations include age, age square, gender, marital status, rural/urban Hukou, average years of schooling for ineligible cohorts, wave dummy and province fixed effect as the covariates. Bootstrap standard error is clustered at household level with 1999 draws. The null hypothesis of the test of observable heterogeneity is that $\beta_1 - \beta_0 = 0$ are jointly true for all observable. The null hypothesis of the test of essential heterogeneity is that $\gamma_1 = \gamma_2 = \gamma_3 = 0$. $MTE_{Q1} \equiv \frac{1}{26-k}\sum_{i=k}^{25} MTE(u=i)$ where k is the smallest u that satisfies the common support assumption; $MTE_{Q23} \equiv \frac{1}{50}\sum_{i=26}^{75} MTE(u=i)$; $MTE_{Q123} \equiv \sum_{i=k}^{75} MTE(u=i)$ where k is the smallest u that satisfies the common support assumption; $MTE_{Q4} \equiv \sum_{i=76}^{K} MTE(u=i)$ where K is the largest u that satisfies the common support assumption.

Last, I check the sensitivity of the estimation results based on Table 3 with two alternative specifications. The first is to control the One Child Policy (OCP) implemented after 1978 with differential enforcement strictness across regions. The OCP is shown to have an effect on multiple outcomes, including labor market outcomes of children (Ebenstein (2010)), which makes OCP a possible confounding policy in this study. I, therefore, control for the strictness of OCP by adding two measures regarding the fines and premium of excess fertility at province level from Ebenstein (2010): the amount of the fine and the duration of the fine. The second alternative specification is to deal with the concern that children may not completely

Table 6: Estimation results of MTE

$MTE(x,u) = (\beta_1 - \beta_0)x + k(u)$			
	(1)	(2)	(3)
$\boldsymbol{k(u) = \gamma u}$			
γ_1	2.752**	2.687**	2.797**
	(1.384)	(1.370)	(1.397)
Control for OCP	No	No	Yes
Alternative measure for the exposure	No	Yes	No
Test of observable heterogeneity	0.0000	0.0000	0.0000
Test of essential heterogeneity	0.0467	0.0500	0.0452

All estimations include age, age square, gender, marital status, rural/urban Hukou, average years of schooling for ineligible cohorts, wave dummy and province fixed effect as the covariates. Bootstrap standard error is clustered at household level with 1999 draws. The null hypothesis of the test of observable heterogeneity is that $\beta_1 - \beta_0 = 0$ are jointly true for all observable. The null hypothesis of the test of essential heterogeneity is that $\gamma_1 = 0$. Column (1) refers to the main specification.

abide by the compulsory education law, especially for the marginally affected cohorts. To deal with this compliance issue, I winsorize the instrument such that the exposure is zero when Z, the exposure defined in Equation 14, is no larger than 0.3, i.e., the new instrument $Z_{new} = Z$ when $Z \geq 0.4$ and $Z_{new} = 0$ when $Z \leq 0.3$.[19] Table 6 shows that the significantly positive γ, namely the reverse selection on gains, is still robust with the two alternative specifications.

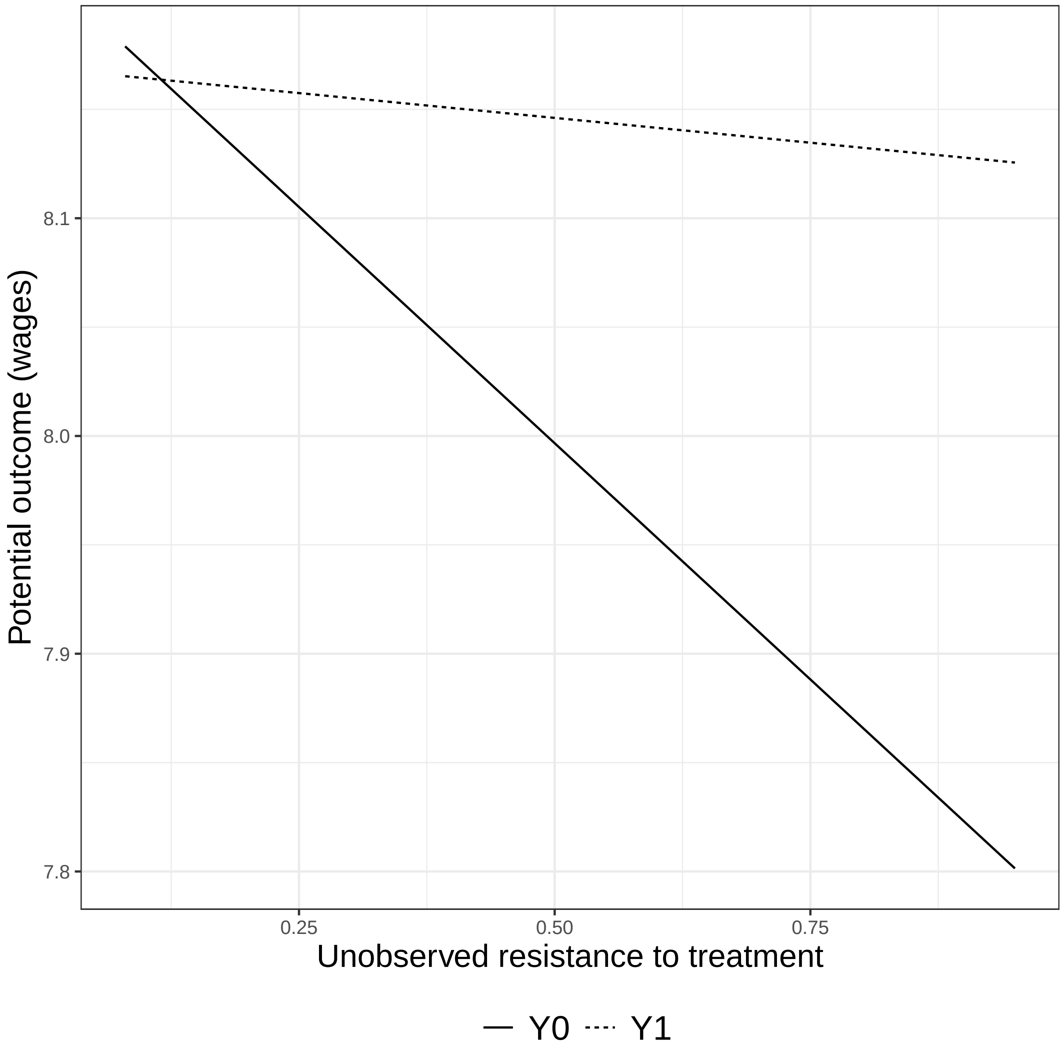
[19]The compulsory education in China includes 6-year primary school and 3-year middle. Therefore, setting the exposure defined in Equation 14 that is smaller than 0.3 to zero indicates that the individuals who have finished primary school is not going to continue their high school education even if required to do so.

6 Interpretation

The analysis above shows that individuals who are less likely to obtain post-compulsory education have higher returns to education. This reverse selection on gains is contrary to the finding of most studies that individuals are selected to post-compulsory education based on their gains. What then explains this reverse selection on gains? The hypothesis from this paper is that larger returns to education are driven by the lower wages in the untreated state (without the education), whereas wages are more homogeneous in the treated state (after education). At the post-compulsory education level, especially high school, children's enrollment in school can be considerably affected by factors other than individuals' interest (return to education), including family budget constraints and the belief in the return to education. The disadvantage in these background factors, such as limited financial support from parents or having information deficit, can hinder the enrollment of high school or any further program and lead to lower wages without education. Fortunately, after obtaining the education signaling one's ability on the labor market, wages become more homogeneous, thus indicating higher returns for those less likely to get post-compulsory education.

If the hypothesis is true, we are expected to find the distribution of wage is unequal before obtaining the education, and it becomes more even after getting the education. By adopting the control function estimator described in Heckman and Vytlacil (2007), Figure 6 shows the potential wage (in logarithm) when treated (obtain post-compulsory education) and untreated (without post-compulsory education). A decreasing potential wage in the untreated group $E(Y_0|U_D)$ reveals that a lower wage is associated with the higher resistance to attaining post-compulsory education. Thus, individuals who are unlikely to have an education are with lower wages before getting an education. A near-to-flat potential wage in the treated group $E(Y_1|U_D)$ shows that the potential wage is near to a constant regardless of the resistance to the educational attainment, despite the slightly lower level at the extremely high resistance. Therefore, education acts as an equalizer reducing the gap in wage along the distribution of the resistance to educational attainment.

Figure 6: Estimation results of MTE.



The linear relation between the unobserved resistance to treatment and MTE is assumed while fixing all covariates at the sample averages.

As additional evidence to support the hypothesis, I directly examine the relationship between the unobserved resistance and family background. When there is a negative relationship between the unobserved resistance and the indicator positively predicting family background, we can conclude that the individual with high unobserved resistance has a more disadvantaged family background. Therefore, I construct an indicator for the family background based on three variables: the educational attainment of the parent (whether obtaining at least a primary school degree), the top job position of the parent in his/her working experience (whether it is non-farmer), and Hukou status of the parent (whether registering an urban Hukou). I use the product of these three binary variables as an indicator to measure the background of mother and father separately. Finally, I use the product of the two binary indicators to measure the background of the family such that a lower level indicates a more disadvantaged family background.

Figure 7 shows the relation between the family background indicator and the unobserved resistance. We can find that regardless of the treatment group (with/out education), the higher unobserved resistance is with a lower level of the family background, i.e., the individual who is less likely to obtain the education because of higher unobserved resistance is also more likely to have a disadvantaged family background. Figure 8 shows the result by parents, and we can find that the higher resistance is also with a lower level of background indicator by parents in all cases except for the case for the untreated group for father.

Figure 7: Unobserved resistence u and family background indicator

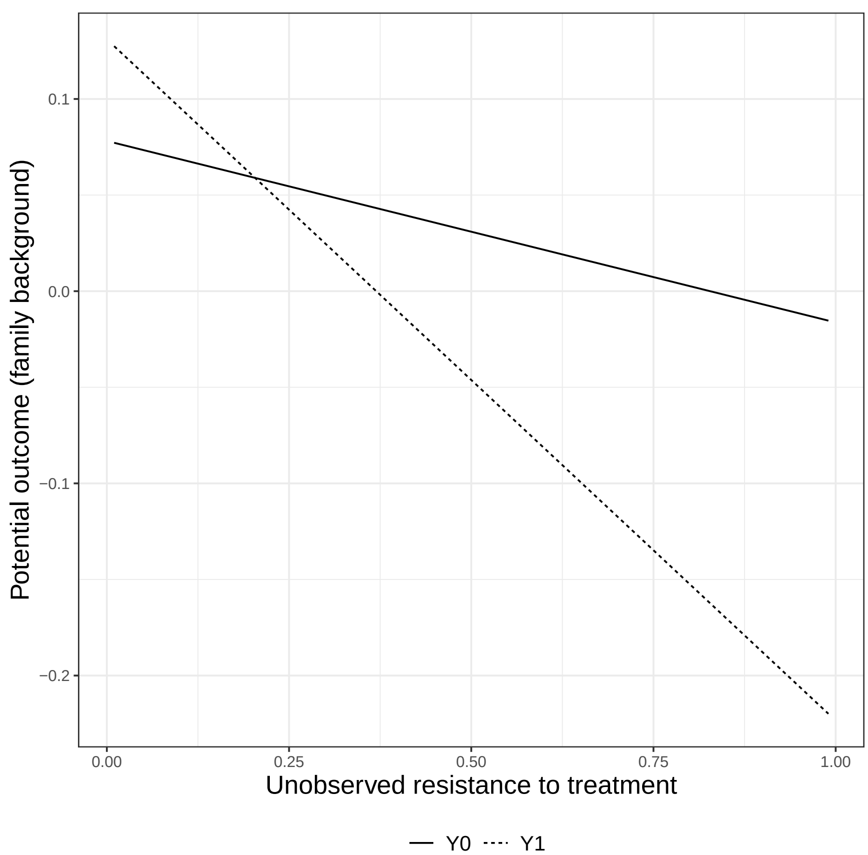
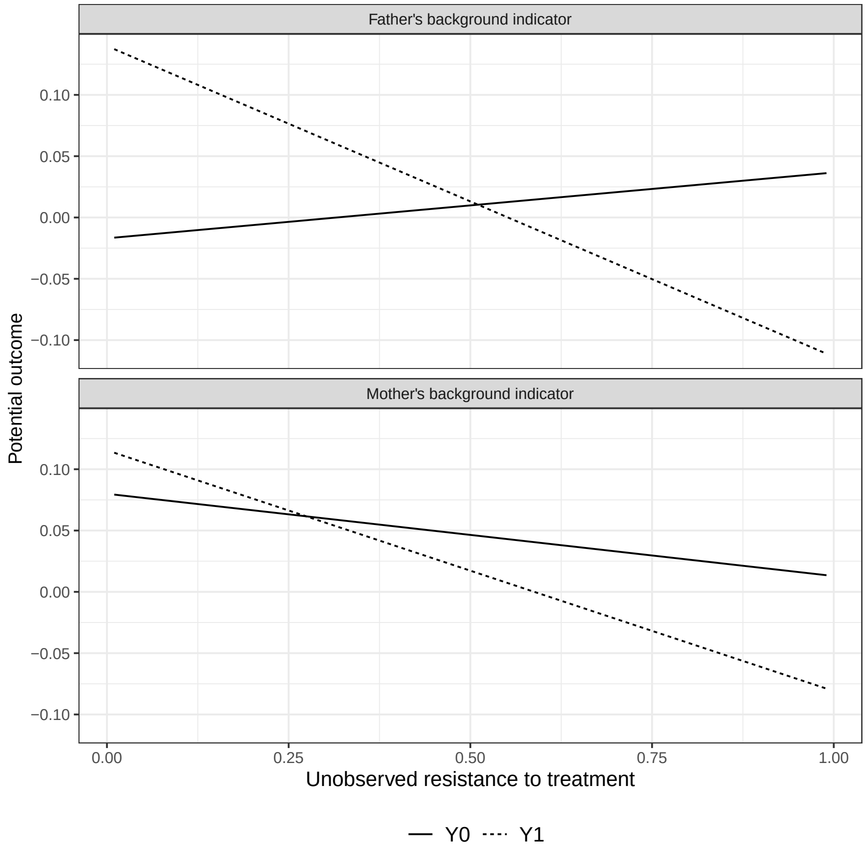


Figure 8: Unobserved resistence u and family background indicator by parents



7 Conclusion

This paper assesses the heterogeneity in return to education (high school degree or above) by estimating marginal treatment effects. The returns to education are heterogeneous with respect to the observed characteristics. When summarizing the likelihood to obtain education by the propensity score explained by the observable, I find individuals who have a lower probability of getting an education have larger returns. The heterogeneous with respect to the unobserved characteristics reinforce the finding: individuals with higher unobserved resistance to education, thus less likely to obtain the education, have higher returns to education. Overall, I find the negative relation between the return to education and the likelihood to attain the education, namely reverse selection on gains.

The pattern of the return to education raises a question, why do the individuals less likely to get post-compulsory education have higher returns? The hypothesis in this study is that the individuals who have higher resistance to educational attainment have lower wages in an untreated state (without post-compulsory education) because of their disadvantaged backgrounds. Education acts as an Equalizer reducing the income gap between different groups of people. Moreover, I also provide evidence that the individuals with higher resistance to post-compulsory education are disproportionately drawn from disadvantaged family backgrounds.

Overall, the results suggest that the enrollment decision on post-compulsory education can be influenced by factors other than an individual's return in China, which differs from when individuals make schooling decisions to maximize their utility (wage). The reverse selection on gains also suggests that the post-compulsory education may not sufficiently reach individuals who would benefit the most from the program. Therefore, this paper provides evidence to support the policies on education (recently published by the government) that prioritize the children who face high costs to get access to basic education or from poor families. Subsidizing these hard-to-reach children or encouraging them to pursue a high school degree or even above (e.g., by informing the benefit of education) could also be economically efficient and help alleviate the income gap, which is the essential goal of many policies recently. However, what may come with aiming at the hard-to-reach children is the higher cost to bring them back to school as they may live in remote areas to the closest school, or their parents may have a higher resistance to let the children get high school education or even above.

Appendices

A Effective years of the law

Table 7: Implementation of compulsory education by provinces

Province	Law effect year	First eligible birth cohort
Beijing	1986	1971
Tianjin	1987	1972
Hebei	1986	1971
Shanxi	1986	1971
Inner Mongolia	1988	1974
Liaoning	1986	1971
Jilin	1987	1972
Heilongjiang	1986	1971
Shanghai	1985	1970
Jiangsu	1987	1972
Zhejiang	1986	1970
Anhui	1987	1972
Fujian	1989	1973
Jiangxi	1986	1971
Shandong	1987	1972
Henan	1987	1972
Hubei	1987	1972
Hunan	1991	1976
Guangdong	1987	1972
Guangxi	1991	1976
Chongqing	1986	1971
Sichuan	1986	1971
Guizhou	1988	1973
Yunnan	1987	1972
Shaanxi	1988	1972
Gansu	1991	1976
Qinghai	1988	1974
Xinjiang	1988	1973

Year of implementation of the compulsory education is collected by Ma (2019) retrieving the information from China's National People's Congress and Chinese Laws and Regulations Information Database. The majority provinces and municipalities set the age of compulsory education to be from six to fifteen years old with exceptions in some poor areas where the eligible age can be from seven to sixteen.

B Sample restriction

B.1 Migration across provinces

I drop 7,610 observations of individuals who migrate across provinces from 260,195 individual-year observations from waves 3 and 4. To verify that someone ever migrates across provinces, I need to know the province where the respondent lived during the school ages and the province the respondent currently lives in. However, there is no information on the provinces where respondents lived when they were during the school ages. I proxy this information by the province documented by Hukou. Hukou is a system of household registration in China, and the location documented by the initial Hukou (when born) is based on parents' Hukou. Moreover, the location documented by Hukou is one of the crucial criteria to get access to the local welfare system and public education. So the province documented by Hukou can proxy the province where respondents were during school ages if there is no change in Hukou or only change within the province. Registering Hukou to another province is rather difficult, especially for those whose destinations are large cities, given the rigid Hukou regulation in China. However, cross-province migration and changing Hukou have become easier for respondents in the sample recently (aged between 31 and 47 in 2015 or 2017). To further validate the usage of the province documented by Hukou, I drop the respondents who changed their Hukou to another province.

One limitation of this approach is that the information on Hukou history is only available for respondents participating in wave 4. Therefore, I cannot drop respondents who only participated in wave 3 and changed their Hukou to another province. However, I argue that the proportion of these respondents is expected to be limited (about 2%). About 30% of the observations in the sample for analysis are respondents who only participate in wave 3. Around 7% of the respondents who participate in wave 4 report registering their Hukou to another province. If the migration pattern is similar in wave 3 and 4, around $30\% \times 7\% = 2.1\%$ of respondents in the sample register their Hukou to another province.

B.2 Migration across provinces

There are two main reasons for the missing information on salary: (1) respondents do not work for any paid work (2) respondents are agricultural workers or self-employed. Table 8 shows the correlation between sample selection and the instrumental variable. Column (1) shows that we do not find evidence that the instrument is correlated with the probability that the observation is trimmed due to missing information on salary. Columns (2) and (3) reinforce the conclusion that there is a small and insignificant correlation between the instrument and the probability that the salary is missing because of the work type (agricultural workers or self-employed) and employment status (whether work for any paid job).

Table 8: Sample restriction and instrument

	(1)	(2)	(3)
	Trimming (Overall)	Trimming (Work type)	Trimming (No work)
Exposure	0.004	0.008	0.007
	(0.025)	(0.028)	(0.025)
Observations	52,842	41,942	35,049
P-value	0.881	0.760	0.780

Robust standard errors in parentheses are clustered at household level. Control variables are the same as the main specification for analysis. Trimming (Overall) is an indicator that equals 1 if the information of salary is missing and 0 otherwise. Trimming (Work type) is an indicator that equals 1 if the missing salary is due to doing agricultural work or being self-employed and 0 if salary is non-missing. Trimming (No work) is an indicator that equals 1 if the missing salary is due to not doing paid job and 0 if salary is non-missing.

C Frequency distribution of instrument Z by ages

Table 9: Frequency distribution of instrument Z by ages

Age \ Z_0	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	Total
31	0	0	0	0	0	0	0	11	0	87	1,386	1,484
32	0	0	0	0	0	0	5	0	79	0	1,386	1,470
33	0	0	0	0	0	13	0	105	0	69	1,270	1,457
34	0	0	0	0	8	0	73	0	71	54	1,259	1,465
35	0	0	0	3	0	79	0	101	50	328	972	1,533
36	0	0	9	0	76	0	102	55	321	242	632	1,437
37	0	11	0	59	0	89	34	318	253	322	243	1,329
38	9	0	69	0	101	50	278	233	340	216	67	1,363
39	8	102	0	84	40	330	271	345	195	73	0	1,448
40	96	0	90	62	326	279	303	201	70	0	0	1,427
41	78	106	64	307	263	365	228	70	0	0	0	1,481
42	220	57	359	292	363	224	53	0	0	0	0	1,568
43	288	358	311	369	221	82	0	0	0	0	0	1,629
44	654	294	379	242	77	0	0	0	0	0	0	1,646
45	1,052	458	238	68	0	0	0	0	0	0	0	1,816
46	1,302	255	81	0	0	0	0	0	0	0	0	1,638
47	1,684	107	0	0	0	0	0	0	0	0	0	1,791
Total	5,391	1,748	1,600	1,486	1,475	1,511	1,347	1,439	1,379	1,391	7,215	25,982

$Z_0 = 1$ always holds when respondents are 28 or younger, while $Z_0 = 0$ always holds when respondents are 48 and older. The reason that 31 instead of 29 is chosen as the lower bound is that there are only a few observations whose $Z_0 \neq 1$ when age is either 29 or 30. Note that this table corresponds to the sample used for the analysis.

D Alternative estimation methods for MTE

The estimation equation is

$$Y = X\beta_0 + X(\beta_1 - \beta_0)p + K(p) + \epsilon \tag{15}$$

β_0, $(\beta_1 - \beta_0)$, and $K(p)$ are of interest, and $MTE(x,u) = x(\beta_1 - \beta_0) + k(p)$ where $k(p) = \dfrac{\partial K(p)}{\partial p}$.

Semiparametric method

I first obtain the estimated $\hat{p}$ from a logistic regression. I then use local polynomial (second order) regressions of Y, X, and $X \times \hat{p}$ on $\hat{p}$ to get residuals e_Y, e_X, and $e_{X\times p}$. With these residuals, I estimate the following equation using regression and

$$e_Y = e_X\beta_0 + e_{X\times p}(\beta_1 - \beta_0) + \epsilon \tag{16}$$

construct residual $\tilde{Y} = Y - X\hat{\beta}_0 - X(\widehat{\beta_1 - \beta_0})\hat{p}$ where $\hat{\beta}_0$ and $(\widehat{\beta_1 - \beta_0})$ are estimated coefficients from above. Furthremore, I use the local polynomial (second order) regression of $\tilde{Y}$ on $\hat{p}$, saving level $\widehat{K(p)}$ and slope $\widehat{k(p)} = \widehat{K'(p)}$. Finally, we have $\widehat{MTE(x}, u) = x(\widehat{\beta_1 - \beta_0}) + \widehat{k(p)}$. In the nonparametric regressions above, the bandwidths are chosen by rule-of-thumb using a polynomial of order 4, and Gaussian kernels are used.

Method assuming normality

We can also assume the following joint normal distribution among error terms U_0, U_1, U_D

$$U_0, U_1, U_D \sim \mathcal{N}(0, \Sigma)$$

$$\Sigma = \begin{pmatrix} \sigma_1^2 & & \\ \rho_{01} & \rho_0^2 & \\ \rho_0 & \rho_1 & 1 \end{pmatrix}$$

We can prove that $K(u) = -(\rho_1 - \rho_0)\phi\{\Phi^{-1}(u)\}$ and $k(u) = (\rho_1 - \rho_0)\Phi^{-1}(u)$, where ϕ is the density function of the standard normal distribution and Φ is the cumulative density function of the standard normal distribution. To get the estimation of $K(u)$ or $(\rho_1 - \rho_0)$, I estimate Equation 15 using regression. Same as the semiparametric approach, propensity score p is estimated by a logistic regression.

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