Who Benefits Most from Education? Evidence from China

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Abstract

We 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), we 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 Introduction

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. Individuals 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, Heckman, and Vytlacil, 2011; Heckman, Urzua, and Vytlacil, 2006; Nybom, 2017; Heckman, Humphries, and Veramendi, 2018; Carneiro, Lokshin, and Umapathi, 2017) have found that individuals who are more likely to get post-compulsory education can have higher returns to education. This finding is in line with the hypothesis that individuals who are more likely to get an education have advantaged characteristics such as higher ability or a lower cost of getting an education, indicating a higher return to education. With higher gains from schooling, individuals are, more likely to select themselves into education. This pattern of selection on gains—individuals with higher returns to education are more prone to get an education—implies a concern about a potential loss of efficiency when expanding the post-compulsory to the hard-to-reach individuals. However, these studies mainly focus on developed economies where the social security network is well established, so there are fewer constraints in making decisions on educational enrollment. When factors other than self-interest affect the schooling decision, individuals who benefit most may not necessarily be those who are most likely to receive post-compulsory education. For example, when individuals are highly dependent on their families to make post-compulsory education decisions due to economic and cultural reasons, the enrollment decision is affected by not only children's return to education but also family interests, such as the opportunity cost of sending children to school and parents' belief in the return to education.

In this study, we 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' in a society with a different institutional setting than the developed economies investigated by the literature. The education attainment of interest is obtaining at least a high school degree (which we 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, we apply the marginal treatment effect (MTE) framework introduced by Björklund and Moffitt (1987) and generalized by (Heckman and Vytlacil, 2005; Heckman and Vytlacil, 2001; Heckman and Vytlacil, 1999), which relates the treatment effect (return to obtaining post-compulsory education) to the observed and unobserved characteristics that affect the likelihood of obtaining the post-compulsory education. To deal with the endogeneity of educational attainment, we 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 the different exposure to the law across cohorts. The compulsory education law determines the duration of compulsory education, which both reduces the cost of obtaining post-compulsory education and increases the demand for post-compulsory education, translating into a higher probability of pursuing a high school degree or above.

we 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 have higher returns to education than men, which points to a reverse selection on gains: individuals who are less likely to get an education have actually 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 cost individuals") benefit most from education, whereas individuals who are more likely to get the education ("low unobserved cost individuals") benefit the least.

By digging deeper into the reasons for these findings, we show that higher returns for individuals who are less likely to obtain post-compulsory education (or high education cost individuals) have lower wages in the untreated status, i.e., without having post-compulsory education. However, wages are more homogeneous across individuals after the population gets an 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. The wage inequality before getting an education could be explained by different family backgrounds, and the wage gap caused by family background is attenuated by obtaining education (Björklund, Jäntti, and Lindquist, 2009).

This paper contributes to the growing literature that estimates marginal treatment effects in the context of education. The finding of this study shows that individuals do not select themselves into post-compulsory education based on their gains found by the current literature (Carneiro, Heckman, and Vytlacil, 2011; Heckman, Urzua, and Vytlacil, 2006; Nybom, 2017; Heckman, Humphries, and Veramendi, 2018, e.g.,), on the contrary, individuals with larger returns are less likely to get an education. To the best of my knowledge, this paper provides the first evidence of the reverse selection on gains at the level of post-compulsory education. We attribute this new evidence to the different institutional settings, especially the role of family background in making schooling decisions. The reverse selection on gains is also found by Cornelissen et al. (2018) who studies the return to preschool education in Germany. [1]

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, Liu, and Zhang, 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.

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.

[1] Though uncommon in education literature, the reverse selection is found in the migration literature, which find more skilled workers are easier to migrate (Chiquiar and Hanson, 2005; Rooth and Saarela, 2007; McKenzie and Rapoport, 2010).

2 Compulsory Education Law in China

The educational attainment of different cohorts has significantly increased over time because of various education expansions since 1949. The enrollment in secondary education was 68.3% for middle school and 31.5% for high school in 1981, while these two numbers were 94.9% and 51.2% respectively in 2000 (National Bureau of Statistics of China). This paper particularly exploits the variation from one education expansion, the enforcement of compulsory education across China, as a natural experiment. In the 1980s, the Chinese government started its first structural reform on the education system, aiming at the decentralization of basic education and the implementation of nine-year compulsory education. One essential policy during the reform was the Compulsory Education Law which 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 for the entire country, including a six-year primary school and a three-year middle school. 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 stay at school until 16 years old. 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 still in their compulsory schooling years.

The Compulsory Education Law implementation dates were not uniform across the country; instead, 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 governments 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.[2]

Moreover, different cohorts were exposed to the reform differently, even in one province. The Compulsory Education Law requires that 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. For cohorts who were around (and younger than) 16 years old when the policy took effect, their required schooling can be shorter than the younger cohorts because people can leave school when they are 16 years old even though they do not finish the nine-year schooling according to the law. For example, children who were 15 years old when the policy took effect were only required to have extra one-year schooling before they could leave school, whereas children who were six years old when the policy took effect were mandatory to finish the nine-year compulsory schooling. Therefore, the law can have a smaller impact on the required schooling years for cohorts who are marginally affected than other younger cohorts.[3]Besides the requirement of the law, the gradual implementation further indicates a different exposure to the law across cohorts. In most provinces, the enforcement of the compulsory education law set the six-year primary school as the first priority and then the three-year middle school as the second step. If it was infeasible to implement the complete nine-year compulsory education immediately, which was common in most provinces, the six-year primary school could be enforced as the first phase and the three-year middle school can be further included as compulsory education.[4] As a result, children who were at the age

[2]Table A.1 lists by province the years of the implementation of the law.

[3]Table A.1 lists by province the first eligible birth cohort for the reform. The cohort older than the first eligible cohort was not affected by the reform.

[4]Take Sichuan province, the most populous province in 1987, for example. The government identified four regions based on their socio-economic development and set different goals of implementing the nine-year compulsory education accordingly. For the most developed region, making up 19.7% of the population, the goal was to enforce the nine-year compulsory education in the whole region by 1990. Meanwhile, for the second least developed region making up 14.6% of the population, the goal was to introduce the mandatory six-year primary school by 1990 and the mandatory three-year middle school by 2000.

of primary school (younger cohorts), were more likely to be affected by the law compared to those who were at the age of middle school (older cohorts). In summary, the more years of the child eligible for compulsory education, the larger the potential effect of the compulsory education law (Ma, 2019).

The Compulsory Education Law in 1986 specified the duration of compulsory education, and the increase in compulsory schooling also reduced the costs of post-compulsory education. For those children not enrolled in middle school (or even primary school) in the absence of compulsory education law due to various reasons, they can attain primary school and middle school education at much lower costs (free in principle). The nine-year compulsory education can equip them with both prerequisites and access to the high school entrance exam, which can, in turn, lead to a higher probability to pursuing a high school degree or above at a lower cost. Aside from reducing the cost of getting an education, the law also indirectly increases the demand for degrees higher than middle school in the labor market. A higher (than middle school) degree could be more desired in signaling one's ability in the labor market after the middle school degree became compulsory, encouraging higher enrollment in high school or any further program. Therefore, the law can lead to both a lower cost of and a higher demand for post-compulsory educational attainment, which is also consistent with the trend of the enrollment rates. The enrollment rate of middle school stayed at 70% steadily from 1980 to 1985 (five years prior to the law), and it increased to more than 90% in 1995; meanwhile, the enrollment rate of high school grew from 40% in 1985 to 50% in 1995.[5]

In summary, there are two sources of variation from the Compulsory Education Law 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 in other provinces with a later year of the

[5] Enrollment rates are from National Bureau of Statistics (https://data.stats.gov.cn/easyquery.htm?cn=C01)

law's implementation. On the other hand, children from different cohorts are affected by the law differently. Children born in an early cohort may be too old to be fully affected by the law, whereas children born in later cohorts could be affected by the law to a larger extent.

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 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.[6]

In the MTE literature, the distribution of U_D is often normalized to uniform distribution 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:

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

MTE is the average treatment effect for the individual with observed characteristics $X = x$ and unobserved cost to treatment $U_D = u$ (or the u_{th} quantile of U_D).[7] MTE also allows for

[6]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.

[7]MTE is defined on *Marginal* individuals in receiving treatment because individuals with $U_D = u$ are

the heterogeneity in both the observable X and the unobservable cost 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 cost 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 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 (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 cost to treatment U_D. Taking the derivative of Equation

also ones with $\{P(Z) = u\} \cap \{I_D = 0\}$ (indifferent in receiving treatment with propensity score u).

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 cost), 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

$$(U_0, U_1, U_D) \text{ is independent of } Z_0, \text{ conditional on } X$$

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 cost 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 em pirical 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[8] Following Brinch, Mogstad, and Wiswall (2017), we impose the second assumption as follows:

Assumption 2

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

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

[8]See a more detailed discussion in Brinch, Mogstad, and Wiswall (2017).

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.[9]

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.

[9] 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 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.

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:

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

$(\beta_1 - \beta_0)$ 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 cost 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 cost 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 cost are with larger treatment effects.

For robustness checks, we choose alternative orders (3 and 4) for the polynomial function $K(p)$. Moreover, to allow for a more flexible specification of $K(p)$, we 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. Appendix D provides more details of these two alternative estimation approaches.

4 Data and variables

The data comes from China Household Finance Survey (CFHS), which is a biannual panel survey starting in 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 participated in the first wave of the survey in 2011, and the number of participants increased to 40,000 in wave 4 in 2017.

4.1 Sample restriction

This study focuses on 25,982 individual-year observations from wave 3 and wave 4. We first restrict the sample to 230,793 observations which did not migrate across provinces. The exclusion of migrants across provinces is to deal with the concern that the migration pattern can be non-random, which invalidates the instrumental variable constructed based on the location (province) information. We 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 interest in this study when a too wide range of cohorts are included. Section 4.4 provides a more detailed discussion on this restriction. Last, we keep 25,982 observations with valid information on all variables used in the analysis.[10]

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

[10]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 respondent either did not work for a paid job, or was doing agricultural jobs or self-employed. However, the association between the selection and the instrument is close to zero and insignificant. See a more detailed discussion in the Appendix B.1.

jobs. This criterion excludes people who were not in the labor market and those engaged in farming or self-employed.[11] 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. About 7% of the respondents only report their wages by the intervals in our sample.

4.3 Educational attainment

The main independent variable of interest is educational attainment. we use a binary variable that equals one if the respondent's highest obtained academic degree is high school or above 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.[12] 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 a 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, we construct an indicator for the exposure to the Compulsory Education Law following Ma (2019),

[11] See a more detailed discussion on the sample restriction in Appendix B.2.

[12] Despite that it is much more likely to pursue further education for those with an academic degree compared to those with a vocational degree, the vocational degree does not sufficiently mean a lower wage in the 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.

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

where *cohort* indicates the birth year, $Cohort_1$ is the birth year of the oldest affected cohort (children 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 (children who were six years old when the policy took effect in most provinces). When the law becomes effective, children who are not affected by the law because they are 16 or older are not exposed to the law ($Z_0 = 0$), and children who are at pre-school age (i.e., six years old or younger) are fully exposed to the law ($Z_0 = 1$). The impact of the law on the marginally affected cohorts—children who were around 15 years old when the law became effective—can be smaller than it on younger cohorts because the law has different requirements for the minimal schooling years and has a gradual implementation process. In this study, following Ma (2019), we use a linear extrapolation of the exposure for children who are between fully exposed and not exposed to capture the idea that the more eligible to the law, the higher the impact it could be.[13]

For the instrument to be valid, the changes in education policies and the exposure to such policy changes should be independent of any unobserved characteristic that explains wages. 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, we control for the age pattern of the income by including age and age quadratic. Differential economic development can

[13]We estimate the model with a binary IV which equals 0 if individuals are older than 15 years old (so the Compulsory Education Law does not affect the individual) and 1 otherwise. The estimation results of the MTE are similar to the results of the main specification in this study. However, we should be cautious about the results because the F statistic from the first stage is marginally weak (9.8) based on the rule-of-thumb in this case.

lead to the disparity in the timing of the compulsory education law's implementation. So we 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, we apply two types of sample restrictions. First, we only focus on individuals who did not migrate across provinces to alleviate the concern that 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, we keep individuals who did not migrate or only migrated within the province.[14] Second, we 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 A.3 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.

4.5 Control variables

Besides age, age quadratic, province fixed effect, and province-specific age trend, we 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 a 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, we also control for the law effectiveness or program intensity by the local educational attainment before the law enforcement. More precisely, we 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.

[14] See more discussion on the definition of migration in Appendix B.2.

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 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.

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.[15] There is

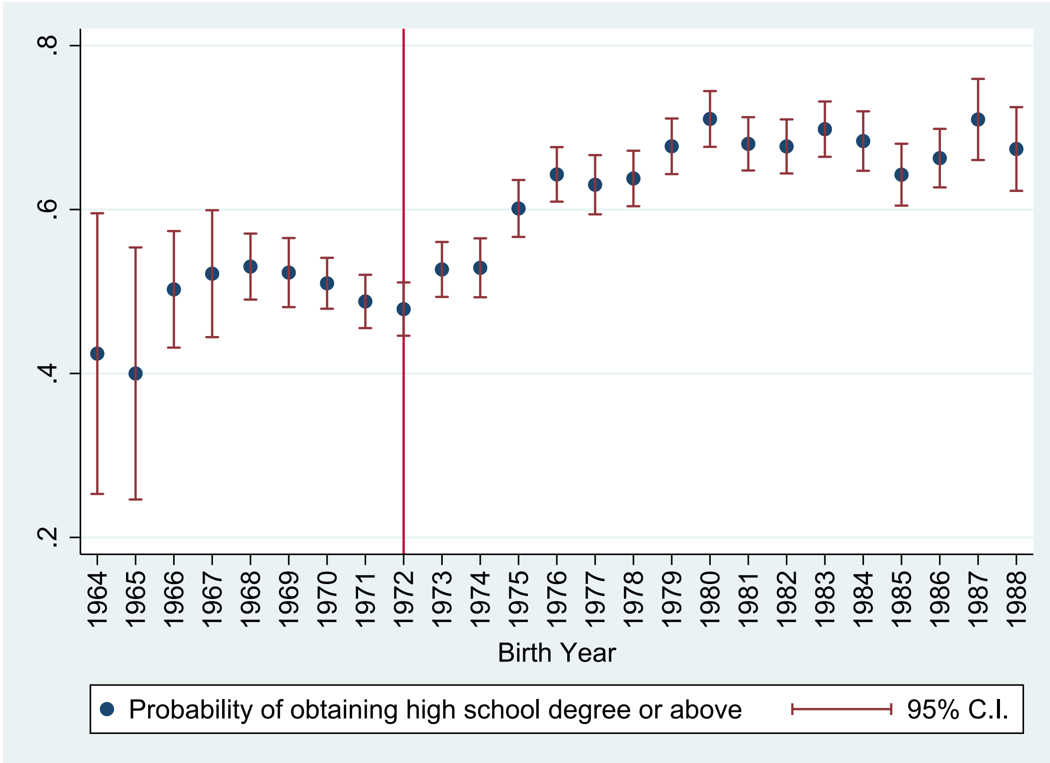
[15]These observations are the largest subsample defined by the implementation year of the law, accounting for about 40% of the whole sample.

Table 1: Summary statistics

Variable	All	Obtain at least high school degree	
		No	Yes
Outcome variable			
Monthly salary ($)	569.038	451.188	655.263
	(541.498)	(382.028)	(619.184)
Treatment variable			
Obtain high school degree	0.577	-	-
	(0.494)	-	-
Instrumental variable			
Exposure to the reform	0.532	0.453	0.590
	(0.407)	(0.402)	(0.401)
Covariates			
Age	38.349	39.433	37.556
	(5.557)	(5.433)	(5.513)
Male	0.571	.601	0.549
	(0.495)	(0.490)	(0.498)
Married	0.910	0.918	0.904
	(0.286)	(0.274)	(0.295)
Rural Hukou	0.412	0.733	0.177
	(0.492)	(0.442)	(0.382)
Average years of schooling for ineligible cohorts	10.033	8.585	11.092
	(2.232)	(1.971)	(1.769)
Number of observations	37,111	15,680	21,431

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 ≈ 6.91 Chinese Yuan.

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



no apparent increasing trend in the educational attainment for cohorts that are older than the first affected cohort, i.e., the 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 is 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.[16]

Therefore, we 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, we 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 a 22% higher chance of obtaining post-compulsory education than a female who is not affected by the reform. Moreover, the same exposure leads to around 11.4% higher chance of getting post-compulsory education for women than men, or equivalently, the effect of the exposure is almost double for women compared to men.

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.[17] 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

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

[17]Although sizable support is found, the dispersion of the propensity score within the treated or the untreated indicates some constraints on who gets into more education. In other words, the included observed variables do not capture all determinants of educational attainment; Meanwhile, the unobserved characteristics captured by U_D explain the remaining. U_D is the unobserved cost or constraint of obtaining post-compulsory education by definition. For example, the family background, which is not included in the covariates but heavily affects the children's educational attainment, can explain the dispersion of the propensity score. See a more detailed discussion about the family background in Section 6. The dispersion also highlights the importance of the unobservable affecting educational attainment, which is addressed by the MTE.

Figure 2: Exposure to reform by gender

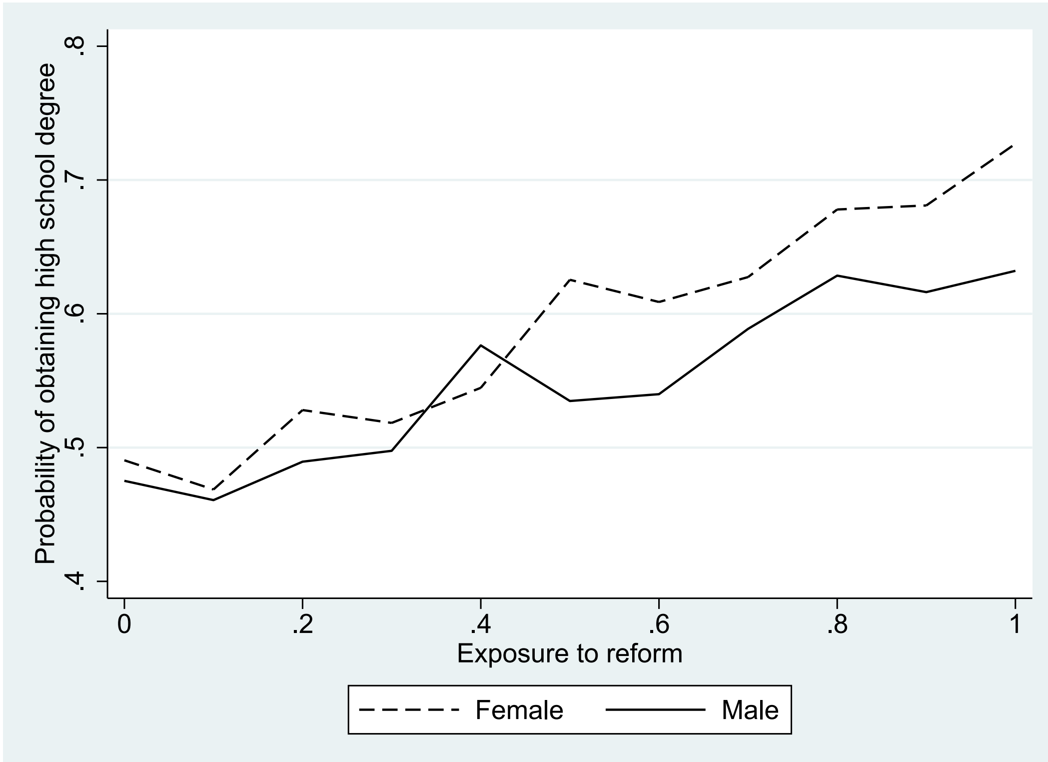
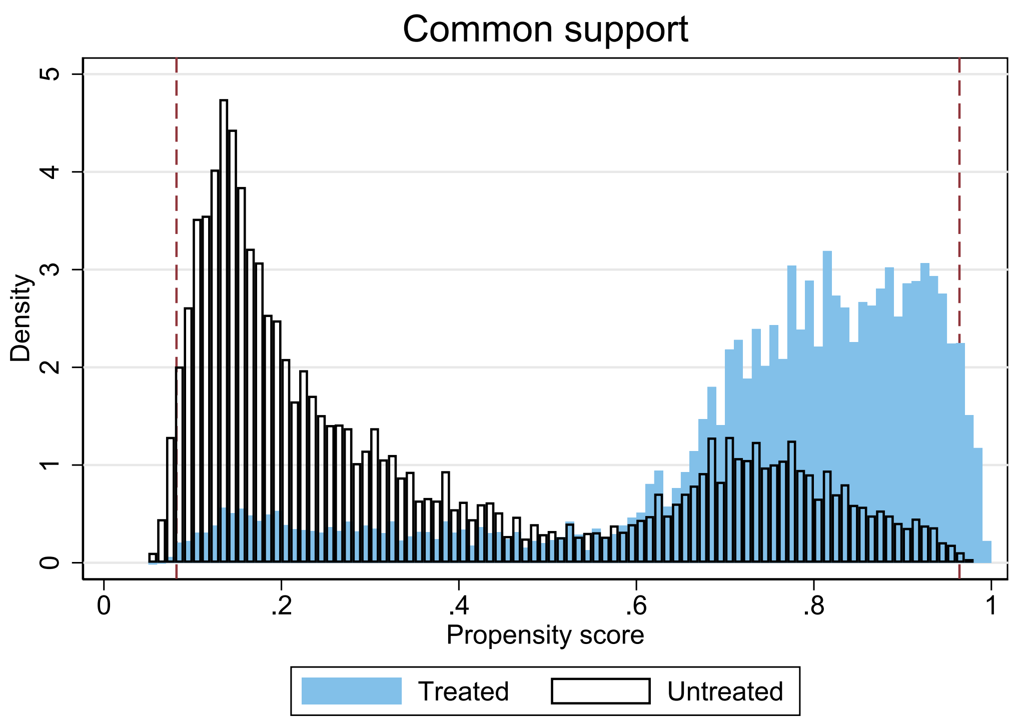


Table 2: First stage estimation results: Marginal effects at means from the logistic regression

	(1)	(2)	(3)	(4)
Independent variables	Obtain high school degree or above			
Exposure	0.205***	0.189***	0.173***	0.164***
	(0.037)	(0.038)	(0.027)	(0.028)
Exposure × Male	-0.125***	-0.125***	-0.113***	-0.113***
	(0.017)	(0.017)	(0.016)	(0.016)
χ^2 for test of the excluded instruments	68.829	65.495	68.996	65.748
p-value for test of the excluded instruments	0.000	0.000	0.000	0.000
Control for One-child-policy	No	Yes	No	Yes
Alternative measure for the exposure	No	No	Yes	Yes
Observations	37,111	37,111	37,111	37,111

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 3: Estimated propensity score



by the parametric assumption. Therefore, to further ease the concern that the identification heavily rests on the arbitrary parametric specification, we 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.[18]

5.2 Treatment Effect Heterogeneity in observed and unobserved characteristics

Table 3 summarizes the heterogeneous returns to education in the main specification. First, we find heterogeneous returns to education with respect to observable. For example, the return to education for women is about 30% larger than men, which is in line with the

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

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, Liu, and Zhang, 2012, e.g.,).

we also find the heterogeneity with respect to the unobservable cost 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 cost 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 cost benefit even more from it than others. Namely, we find the reverse selection on gains based on the unobserved cost 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 cost ($0.75 \leq u \leq 1$) is almost three times as it for individuals with the least 25% unobserved cost ($0 \leq u \leq 0.25$).[19]. 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

[19] $2.752 \times ((1 + 0.75)/2 - (0 + 0.25)/2) = 2.064$

enrollment is in line with the exposure to the reform, a high school degree or above almost doubles the salary after tax. This LATE estimate is also comparable to the estimates from other studies (Chen, Jiang, and Zhou, 2020; Mishra and Smyth, 2013). For example, Mishra and Smyth (2013) finds that an additional year of schooling leads to an 18.31% increase in income.[20]

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, we investigate the relationship between the return to education and the likelihood of obtaining the education explained by the observable. Specifically, following Zhou and Xie (2019), we summarize the likelihood explained by the observable by propensity score $P(D=1|X)$, which is the prediction of the post-compulsory education enrollment based on the observed characteristics.[21] 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 the 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 unobserved characteristics have higher returns to education.

[20]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.

[21]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 cost to treatment U_D. They also prove that we can replace the multi-dimension observed characteristics with the propensity score without loss of generosity. See more in their paper.

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	0.130	0.041	0.002
Age squared	-0.001	0.001	0.009
Male	-0.274	0.022	0.000
Currently married	-0.115	0.043	0.007
Have a rural Hukou	-0.415	0.606	0.493
Average schooling years for ineligible cohorts	-0.181	0.158	0.252
Constant	2.183	2.833	0.441
$\boldsymbol{k(u) = \gamma_1 u}$			
γ	2.033	0.934	0.029
LATE (based on MTE)	0.905	0.159	0.000
LATE (based on 2SLS)	1.121	0.207	0.000
Test of observable heterogeneity			0.000
Test of essential heterogeneity			0.029

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
Propensity score	-0.501	0.005	0.000
Constant	0.440	0.004	0.000

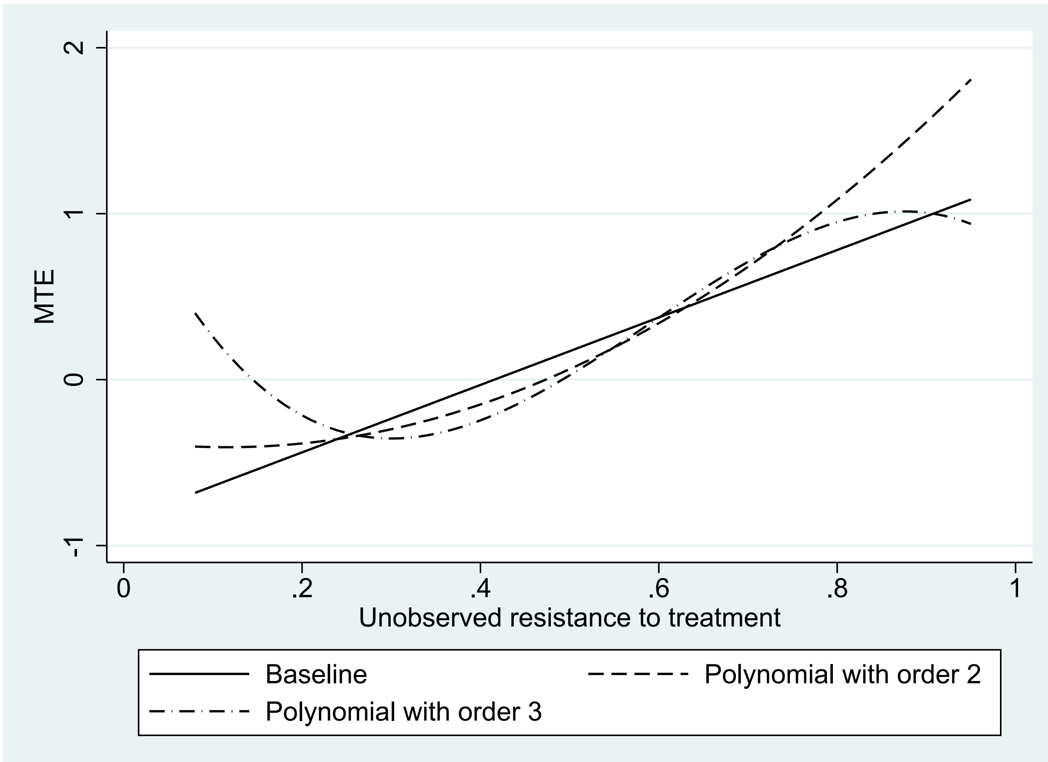
5.3 Robustness checks

The pattern of the reverse selection on gains is robust to a number of robustness checks. First, we 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, we specify $k(u)$ as a polynomial function with order 2 and 3. Figure 4 shows the MTE as a function of the unobserved cost.[22] The curvature of these different specifications

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

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 cost, which stands for the significant curvature in MTE.

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



The polynomial relation between the unobserved cost 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.

The pattern of the reverse selection on gains can still be found in the baseline specification ($k(u)$ 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 cost 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 a relatively higher unobserved cost is higher than it for those with relatively lower unobserved cost. Specifically, we calculate the average of MTEs with the unobserved cost u in the 4th quartile, i.e., the average return to education for those with the most 25% unobserved cost, and compare it to the average MTE with the unobserved cost in the other quartiles. we test the hypothesis that the average return for individuals with the most 25% unobserved cost is higher than it for other individuals (with unobserved cost $u \leq 75\%$, 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 cost is significantly higher than it of all other groups in all scenarios except for one case.[23] Overall, we find that the average return to education for individuals with the most 25% unobserved cost 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, we estimate it semi-parametrically without assuming a parametric specification using Double Residual Regression (Robinson, 1988). Moreover, we 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.[24] Figure 5 shows the estimated MTE as a function of the unobserved cost. 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 cost.

Last, we 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. we, therefore, control for the strictness of OCP by adding two measures regarding the fines and premium of excess fertility at the 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 abide by the compulsory education law, especially for the marginally

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

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

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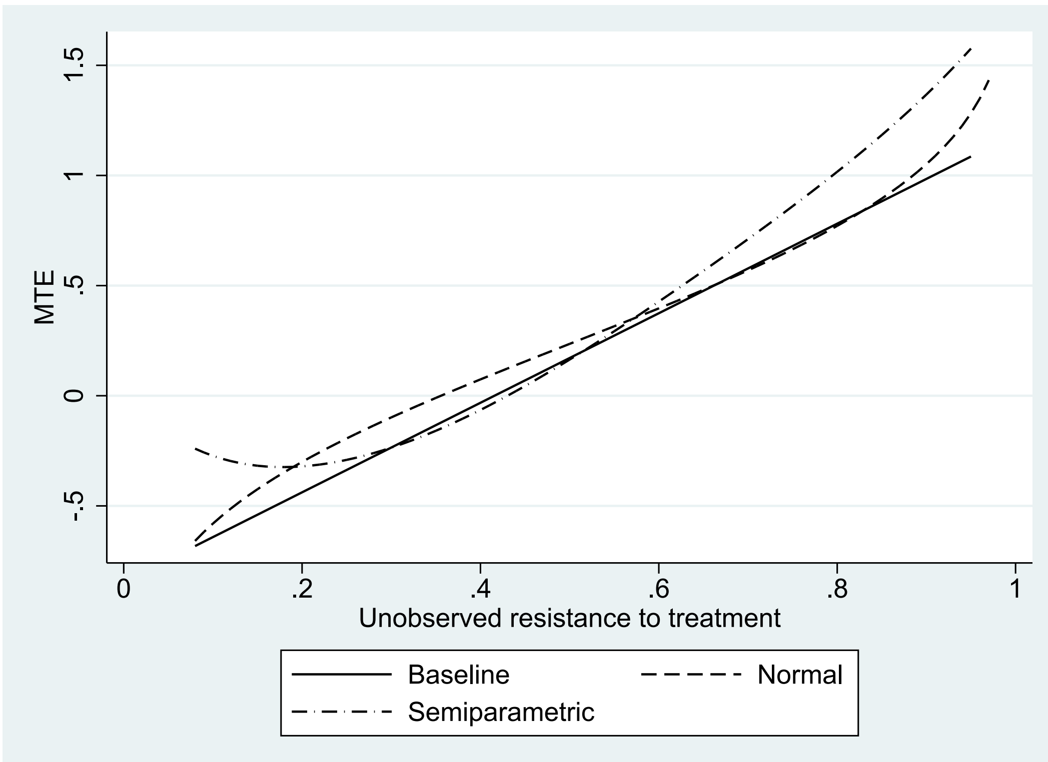
Figure 5: Estimation results of MTE with alternative identification strategies on $k(u)$



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 cost 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	2.033**	-0.799	-11.862
	(0.934)	(1.746)	(7.275)
γ_2		3.183*	25.820*
		(1.796)	(14.642)
γ_3			-14.399
			(9.295)
Test of observable heterogeneity	0.000	0.000	0.000
Test of essential heterogeneity	0.029	0.030	0.017
$MTE_{Q4} > MTE_{Q123}$	0.036	0.018	0.146
$MTE_{Q4} > MTE_{Q23}$	0.031	0.016	0.101
$MTE_{Q4} > MTE_{Q1}$	0.037	0.023	0.255

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.

affected cohorts. To deal with this compliance issue, we 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 $Z \leq 0.3$.[25] Table 6 shows that the significantly positive γ, namely the reverse selection on gains, is still robust with the two alternative specifications.

[25] 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 are not going to continue their high school education even if required to do so.

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	2.033**	1.970**	1.999**
	(0.934)	(0.879)	(0.949)
Control for OCP	No	No	Yes
Alternative measure for the exposure	No	Yes	No
Test of observable heterogeneity	0.000	0.000	0.000
Test of essential heterogeneity	0.026	0.025	0.035

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.

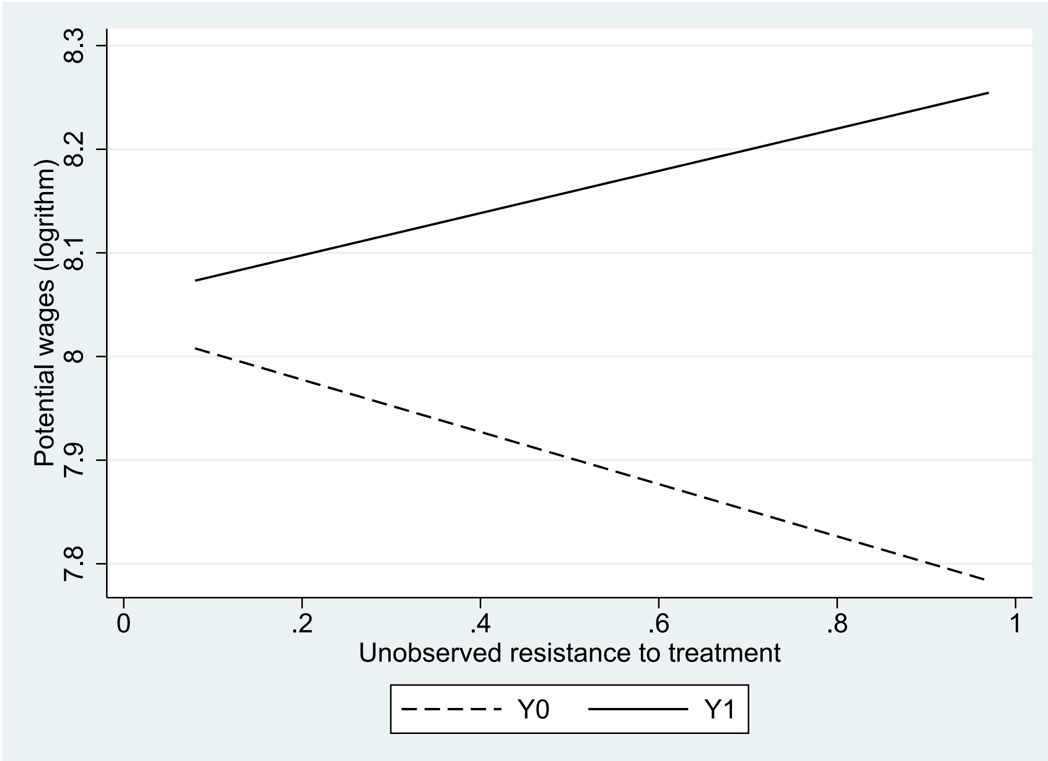
6 Interpretation

The analysis above shows that individuals less likely to obtain post-compulsory education have higher returns to education than others who are more likely to get an education. This reverse selection on gains is contrary to the finding of most studies that individuals are selected into post-compulsory education based on their gains. What explains this pattern of reverse selection? To shed more light on this question, we adopt the control function estimator described in Heckman and Vytlacil (2007) to estimate the potential wages in both the treated and the untreated states. Note that MTE is defined as the difference between two potential outcomes, i.e., $E[Y_1 - Y_0|U_D]$ (return to education), and the methodology allows the estimation of $E[Y_0|U_D]$ (potential wage without obtaining post-compulsory education) and $E[Y_1|U_D]$ (potential wage after obtaining post-compulsory education) separately.[26] By doing so, we know whether the MTE is driven by the difference in the outcomes in the treated state Y_1, or untreated state Y_0, or both.

Figure 6 shows the estimated potential wage (in logarithm) in the treated state and the untreated state. A decreasing potential wage in the untreated group $E(Y_0|U_D)$ reveals that, in the absence of education, a lower wage is associated with a higher cost of obtaining education U_D (so less likely to get an education) in the population. Thus, we find the wage gap between individuals with higher cost of obtaining education and individuals with a lower cost of obtaining an education. However, this gap is reduced as shown by a near-to-flat potential wage in the treated group $E(Y_1|U_D)$. After the population obtains post-compulsory education, the potential wage becomes much more homogeneous regardless of the cost to the educational attainment (the likelihood to get an education), despite the slightly lower level at the extremely high cost. Therefore, it reveals that the larger returns to education for children less likely to get the education (higher cost U_D) are driven by the lower wages in the untreated state (without the education), and more homogeneous wages in the treated state (after education).

[26] The MTE is also a function of covariates X, but for ease of explanation, X is taken as given here.

Figure 6: Estimation results of MTE.



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

The finding above makes us wonder what causes the wage inequality in the untreated state and what role education plays in the inequality. The wage inequality in China has been addressed by a large number of studies (e.g., Han, Liu, and Zhang (2012) and Knight and Song (2003)). The analysis above particularly finds the wage inequality with respect to the cost (thus the likelihood) of obtaining post-compulsory education: wages in later life are lower for children who are less likely to get an education. One important reason for this inequality is the uneven family background that influences schooling decisions. At the post-compulsory education level, especially high school, children's enrollment in school can still considerably rely on their parents. Families with low income or tight budget constraints may not be able or willing to support their children's schooling afterward the compulsory education. Additionally, families' lack of access to information on post-compulsory education and its potential return can also hinder the children's enrollment of high school or any further program. The disadvantage in these family background factors not only have a negative impact on the children's schooling but also are associated with worse labor market outcomes for children due to intergenerational mobility for example (Björklund, Jäntti, et al., 2009). Therefore, the wage of individuals with disadvantaged backgrounds is lower than it of those with more advantaged backgrounds.

The wage inequality stands out, especially when no other factors offset the negative impact of disadvantaged family backgrounds. In the absence of post-compulsory education in the population, the lower wage is associated with a more disadvantaged family background (or higher cost of obtaining education). Fortunately, obtaining an education is a crucial factor signaling one's ability in the labor market; therefore, the attainment of post-compulsory education can alleviate the inequality in income caused by family background (Björklund, Jäntti, and Lindquist, 2009). When the population gets an education, wages become more homogeneous regardless of the disparity in family background. The reduction in inequality—a larger increase in wage of individuals with lower wages without getting an education—is captured by the MTE, indicating a larger return to education for individuals with more disadvantaged family backgrounds (less likely to get an education).[27]

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 of obtaining education by the propensity score explained by the observable, we find individuals who have a lower probability of getting an education have larger returns. The heterogeneity with respect

[27] A more direct evidence to support the hypothesis is applying the control function estimator described in Heckman and Vytlacil (2007) but replacing the outcome variable with the measure for family background. This strategy directly depicts the relationship between the potential family background and the cost of obtaining an education (a similar strategy is used by Cornelissen et al. (2018)). However, in this study, more than 60% of the observations are not with valid answers to questions related to family background, and the results are not robust to alternative ways of constructing the variables of family background.

to the unobserved characteristics reinforce the finding: individuals with higher unobserved cost to education, thus less likely to obtain the education, have higher returns to education. Overall, we find a negative relation between the return to education and the likelihood of obtaining the education, namely reverse selection on gains.

The pattern of the return to education raises the 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 costs 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, we also provide evidence that the individuals with a higher cost of 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 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 cost to let the children get a high school education or even above.

Online Appendix

A Additional Tables and Figures

Table A.1: 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.

Table A.2: Sample restriction and instrument

	(1) Trimming (Overall)	(2) Trimming (Work type)	(3) Trimming (No work)
Exposure	-0.026	-0.022	-0.024
	(0.030)	(0.035)	(0.031)
Observations	80,358	59,034	51,428
P-value	0.386	0.537	0.438

Robust standard errors in parentheses are clustered at household level. Control variables include birth year fixed effects, province fixed effects, and wave fixed effects. 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.

Table A.3: Frequency distribution of instrument Z by ages

Age \ Z	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	Total
29	0	0	0	0	0	0	0	4	0	42	1,884	1,930
30	0	0	0	0	0	0	4	0	43	0	1,909	1,956
31	0	0	0	0	0	14	0	48	0	109	1,656	1,827
32	0	0	0	0	8	0	39	0	122	19	1,614	1,802
33	0	0	0	7	0	41	0	146	24	165	1,380	1,763
34	0	0	4	0	38	0	112	38	178	135	1,286	1,791
35	0	11	0	26	0	120	21	222	138	361	972	1,871
36	9	0	41	0	113	39	213	153	357	242	632	1,799
37	11	36	0	101	35	247	119	353	253	322	243	1,720
38	63	0	124	43	256	153	302	233	340	216	67	1,797
39	55	160	51	220	163	370	271	345	195	73	0	1,903
40	200	47	265	186	362	279	303	201	70	0	0	1,913
41	254	297	210	345	263	365	228	70	0	0	0	2,032
42	576	185	403	292	363	224	53	0	0	0	0	2,096
43	824	401	311	369	221	82	0	0	0	0	0	2,208
44	1,141	294	379	242	77	0	0	0	0	0	0	2,133
45	1,574	458	238	68	0	0	0	0	0	0	0	2,338
46	1,692	255	81	0	0	0	0	0	0	0	0	2,028
47	2,097	107	0	0	0	0	0	0	0	0	0	2,204
Total	8,496	2,251	2,107	1,899	1,899	1,934	1,665	1,813	1,720	1,684	11,643	37,111

$Z_0 = 1$ always holds when respondents are 28 or younger, while $Z_0 = 0$ always holds when respondents are 48 and older. Note that this table corresponds to the sample used for the analysis.

Table A.4: Estimation results of MTE

$MTE(x,u) = (\beta_1 - \beta_0)x + k(u)$	Coefficient	Std. Err.	P-value
$\boldsymbol{\beta_1 - \beta_0}$			
Male	-0.273	0.021	0.000
Currently married	-0.110	0.043	0.010
Have a rural Hukou	-0.186	0.592	0.753
Average schooling years for ineligible cohorts	-0.101	0.153	0.510
Constant	4.224	2.490	0.090
$\boldsymbol{k(u) = \gamma_1 u}$			
γ	1.662	0.985	0.092
Test of observable heterogeneity			0.000
Test of essential heterogeneity			0.092

The estimation include gender, marital status, rural/urban Hukou, average years of schooling for ineligible cohorts, wave dummy, birth year fixed effects, 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 $\gamma_1 = 0$.

B Sample restriction

B.1 Selection on wage variable

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 A.2 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).

B.2 Migration across provinces

we 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, we 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. we 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, we 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, we cannot drop respondents who only participated in wave 3 and changed their Hukou to another province. However, we 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.

C Separability Assumption

It is possible that the Separability Assumption (Assumption 2) is not required in identifying the MTE given the instrumental variable in this study. However, it facilitates the interpretation of the results. Note that the MTE is identified based on the following equation without Assumption 2

$E[Y|X = x, P(Z) = p] = \beta_0 x + (\beta_1 - \beta_0)xp + K(x, p)$

If we use Local IV approach to identify all parameters including $K(x, p)$ parametrically, we need to put some restrictions on the formula of $K(x, p)$. According to Brinch, Mogstad, and Wiswall (2017), under the conditional independence assumption, the maximal number of parameters in $K(x, p)$ that can be identified should be one less than the distinct values that the excluded instrument can take conditional on the covariate X. There are 11 unique values that the excluded instrument can take, i.e., $(0, 0.1, 0.2, \ldots, 1)$, so there can be up to 11 parameters specified.

However, the flexible specification of $K(x, p)$ could make it difficult to interpret the

$$MTE(x, u) \equiv \frac{\partial E[Y|X = x, P = p]}{\partial p}|_{p=u} = (\beta_1 - \beta_0)x + \frac{\partial K(x, p)}{\partial p}|_{p=u}$$

In the most flexible way, even if specifying the linearity of u in MTE, we make the slope parameter varying in each combination of all values of all covariates X. It is not only impossible to identify (the number of the combination of the values of X is far larger than 11) but also hard to interpret the results, e.g, is the MTE a decreasing or increasing function of u? That is to answer the "reverse/selection on gains" in this study. In the other extreme, we make not only u but also X linear in the MTE. The issue is that we cannot separately identify $(\beta_1 - \beta_0)x$ and $\frac{\partial K(x, p)}{\partial p}$ both of which are linear in x. Thus, we need to specify a $K(x, p)$ with moderate flexibility, e.g., $K(x, p) = \beta_3 x^2 p$ (note that β_3 is a vector with the length of 7 which is the number of covariate X in this study). However, we couldn't find a concrete reason to argue that this specification outperforms other specifications including the current main specification with separability assumption. The price of the separability assumption is that we specify that the observable and the unobservable contribute to wage separately thus $K(x,p) = K(p)$, and the benefit is that the relationship between the MTE and p is easier to read and estimated.

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 $k(p) = \frac{\partial K(p)}{\partial p}$.

D.1 Semiparametric method

we first obtain the estimated $\hat{p}$ from a logistic regression. we 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, we 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, we 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.

D.2 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 $(\rho_1 - \rho_0)$, we estimate Equation 15 using regression. Same as the semiparametric approach, propensity score p is estimated by a logistic regression.