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Abstract

Parental schooling is widely thought to improve child outcomes. But most studies on parental-child relations are associative, without control for estimation problems, such as unobserved intergenerationally-correlated endowments, if causality is of interest. The few exceptions are relatively recent studies that focus on high-income countries (HICs), with their much different contexts than the low- and middle-income countries (LMICs) in which the vast majority of children globally are growing up. This paper estimates the causal (conditional on the assumptions for the model) relationships between parents' schooling and their children's schooling in the most populous LMIC, using adult identical (monozygotic, MZ) twins data from urban China. Our ordinary least-squares estimates show that one-year increases in maternal and parental schooling are associated, respectively, with 0.4 and 0.5 more years of children's schooling. However, if we control for genetic and other endowment effects by using within-MZ fixed effects, the results indicate that mothers' and fathers' schooling have no significant effects on children's schooling. Our main results remain with various robustness checks, including controlling for measurement error. These results suggest that the positive associations between children's and parents' schooling in standard cross-sectional estimates in this major LMIC are mainly due to the correlation between parents' unobserved endowments and their schooling and not the effects of their schooling per se.

Keywords

parental schooling, children's schooling, endowments, China, within-twins estimates

Disciplines

Educational Assessment, Evaluation, and Research | Educational Sociology | Family, Life Course, and Society | Social and Behavioral Sciences | Sociology

The Causal Effects of Parents' Schooling on Children's Schooling in Urban China

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Abstract: Parental schooling is widely thought to improve child outcomes. But most studies on parental-child relations are associative, without control for estimation problems, such as unobserved intergenerationally-correlated endowments, if causality is of interest. The few exceptions are relatively recent studies that focus on high-income countries (HICs), with their much different contexts than the low- and middle-income countries (LMICs) in which the vast majority of children globally are growing up. This paper estimates the causal (conditional on the assumptions for the model) relationships between parents' schooling and their children's schooling in the most populous LMIC, using adult identical (monozygotic, MZ) twins data from urban China. Our ordinary least-squares estimates show that one-year increases in maternal and parental schooling are associated, respectively, with 0.4 and 0.5 more years of children's schooling. However, if we control for genetic and other endowment effects by using within-MZ fixed effects, the results indicate that mothers' and fathers' schooling have no significant effects on children's schooling. Our main results remain with various robustness checks, including controlling for measurement error. These results suggest that the positive associations between children's and parents' schooling in standard cross-sectional estimates in this major LMIC are mainly due to the correlation between parents' unobserved endowments and their schooling and not the effects of their schooling per se.

Highlights:

- Parental schooling significantly positively associated with child schooling in urban China
- With identical twins control for endowments, maternal and paternal schooling effects insignificant
- With control for measurement error and other robustness checks, parental schooling remains insignificant
- In standard estimates parental schooling apparently proxying for endowments

Key words: parental schooling; children's schooling; endowments; China; within-twins estimates

JEL Codes: I2 Education

Conflicts of interest: The authors declare they have no conflicts of interest.

Ⅰ. Introduction

Widely-held and long-standing conventional wisdom is that parental schooling, particularly maternal schooling, importantly improves many child outcomes, including schooling, in a wide range of economies (e.g., Haveman and Wolfe, 1995; King and Mason, 2000). This perception is one of the major reasons that many governments and international organizations advocate greater investment in schooling, particularly in females, in low- and middle-income countries (LMICs) in which the vast majority of children – over 85% – globally are growing up (UNESCO Institute for Statistics, 2017; World Bank, 2018; Narayan et al., 2018). However, such policy recommendation makes more sense if parental schooling indeed has a causal effect on child outcomes in LMICs.

But most of the many studies discussed in review papers (Haveman and Wolfe, 1995; King and Manson, 2000; Black and Devereux, 2011; Torche, 2019) on parental schooling - child outcome relations are associative, without control for estimation problems such as unobserved intergenerationally-correlated endowments. The few exceptions are relatively recent studies that focus on high-income countries (HICs). These studies for HICs attempting to identify causal effects of parental schooling on children's schooling use strategies such as identical-twins (monozygotic, MZ) fixed effects based on the assumption that parents who are MZ twins have basically the same genetic and other endowments, adopted children based on the assumption that adoption is random and instrumental variables based on changes in schooling systems. A causal study on a LMIC is still lacking.

The results on what roles maternal and paternal schooling play in their children's schooling in HICs vary. Most studies find strong positive paternal schooling effects and smaller or no maternal effects. Behrman and Rosenzweig (2002, 2005), in contrast, find negative effects of mothers' schooling (that they interpret to reflect that more-schooled women, holding constant endowments, spend more time in the labor force and less time caring for their children) and positive effects of fathers' schooling on children's schooling when they control for endowments (including individuals' own and their spouses') by using U.S. adult MZ data. Other studies find that the effects of fathers' schooling are positive and the effects of mothers' schooling are close to zero, including Antonovics and Goldberger (2005) using the same data as Behrman and Rosenzweig, Plug (2004) based on a sample of adopted children in the U.S., Bjorklund, Lindahl and Plug (2006) using Swedish adoption data, and Holmlund, Lindahl and Plug (2011) using Swedish twins samples. However, mothers' schooling is found to have a positive effect on children' schooling by Sacerdote (2007) using approximately randomly-assigned Korean-American adoptees, though the effect is smaller than that of fathers' schooling in a sample of Norwegian twins (Pronzata, 2012). Some results based on an instrumental variable (IV) approach indicate that paternal schooling has no significant effects while maternal schooling has positive but small effects (Black, Devereux and Salvanes, 2005), or even large positive effects (Chevalier, 2004).

However, the data used for most adopted-children studies probably do not approximate random assignment of adoptees. Also, the IV procedure results in local-average-treatment effects (LATE) pertaining to individuals at the margin of being affected by changes in compulsory schooling regulations, not the whole distribution of schooling, and it is possible that different estimates would result were the instrumental variable a policy change that increases enrollment in higher education such as college openings (Currie and Moretti, 2003) rather than educational reforms affecting the bottom part of the schooling distribution. In contrast, the distributions of differences in schooling between members of MZ pairs tend to occur for a wide range of schooling levels, not just those close to the legal minimums (e.g., Behrman et al., 2011; Amin, Behrman and Kohler, 2015). For a full exploration of how parental schooling influences children's schooling, within-MZ fixed-effects estimates are likely preferable to IV approaches, because the former would be closer to average treatment effects rather than LATE, although the twins strategy is still far from perfect.

The most common criticisms of the twins strategy include unobserved heterogeneity in what determines schooling differences in twins that may directly affect children's schooling and measurement error that is exacerbated in fixed-effects estimates (Griliches, 1979; Bound and Solon, 1999; Kohler, Behrman and Schnittker, 2011; Amin et al., 2015). The twins strategy requires strong assumptions with respect to the random generation of differences in schooling outcomes between the twins, though these differences could be caused by random events such as injuries or assignment of inspiring teachers that can be treated as quasi-experimental. Even if possible endogeneity of differences in schooling outcomes between MZs cannot be completely ruled out, the within-MZ estimator is still found to be less biased than the OLS estimator (Li, Liu and Zhang, 2012). Some also claim that there is a problem of external validity because twins differ from the whole population (e.g., lower birth weight distributions). However the control for endowments in the within-MZ estimates controls for whatever ways that MZs differ from the larger population (Kohler, Behrman and Schnittker, 2011; Amin et al., 2015).

This study helps fill the gap in the literature on causal studies of parental schooling on children's schooling in LMICs. We use Chinese adult twins data to estimate the causal net effects of parents' schooling on children's schooling by applying the MZ fixed-effects strategy of Behrman and Rosenzweig (2002), as well as the causal gross effects using the standard within-MZ approach. The Chinese Twins Survey contains information on schooling attainment for the MZ twins respondents, their spouses and their children. In addition, it includes information on earnings on the current job and work time for both the respondents and their spouses and reports of each twin on the other twin's schooling and on the other twin's spouse's schooling. This dataset is the first socioeconomic adult twins dataset for China and, to our knowledge, for LMICs more generally. It allows us to study the causal relationship between parental schooling and their children's schooling by controlling for unobserved individual-specific endowment components.

In China, the increasingly heated issue of intergenerational mobility has drawn more and more attention both from the public and from scholars. A number of recent studies focus on the intergenerational transmission between parents and their children of schooling (Golley and Kong, 2013) or income (Guo and Min, 2008; Gong, Leigh and Meng, 2012). But most of these studies are descriptive and do not investigate causal relations between parents' schooling and children's schooling. And none of them employs twins data to control for omitted ability, motivation and family background. It is well-known that positive correlations between schooling and heritable ability and other unobserved factors will likely lead to upward bias in OLS estimates of cross-sectional relations between the schooling of parents and their children. To the best of our knowledge, our study is the first to investigate the causal effect of parents' schooling on their children's schooling using adult twins data in China and in LMICs more broadly.

Consistent with previous results from conventional cross-section studies, our OLS estimates show positive relationships between the schooling of both parents and their children's schooling. Specifically, one-year increases in maternal and parental schooling are associated with higher children's schooling by 0.4 and 0.5 years, respectively. However, after controlling for individuals' own endowments and the schooling and endowments of their spouses by applying within-MZ fixed effects, we find that maternal and paternal schooling effects are insignificant.

Our study contributes to the recent scholarly literature on causal effects of parents' schooling on children's schooling in a much more different LMIC context than the previous studies on HICs. Our findings also are important for policy makers. If parents' schooling is largely responsible for creating an environment in which children can learn more and prosper, then increasing the schooling of one generation *inter alia* will have long-term consequences through intergenerational spillover effects on subsequent generations. However, if inherited abilities and other endowments account for children's success in school, then improving schooling for one generation may have limited effect on the next generation. Rather than try to boost children's schooling in part via the parental channel, governments should shift focus to more direct interventions, such as early childhood programs and universal access to kindergarten, especially for the poor and disadvantaged.

This paper is structured as follows. Section 2 introduces the identification strategy and estimation method. Section 3 describes the data. Section 4 reports the results. Section 5 presents various robustness checks. Section 6 concludes.

Ⅱ. Model

The theoretical underpinnings of empirical estimates of intergenerational schooling relations are intergenerational family investment models (Becker and Tomes, 1979, 1986; Behrman, Pollak and Taubman, 1982, 1995; Solon, 1999, 2004). The typical estimated reduced-form equation is:

$$S_{ij}^{c} = \delta_0 + \delta_1 S_{ij}^{p} + \theta_1 h_{ij}^{p} + e_{ij}^{c} \qquad (1)$$

where S_{ij}^{c} is the schooling of child i in family j. The explanatory variables are parental schooling S_{ij}^{p}; other parental factors h_{ij}^{p} that affect child schooling (treated as a scalar rather than a vector for simplicity); and child-specific characteristics e_{ij}^{c} that represent everything else affecting children's schooling but orthogonal to S_{ij}^{p} and h_{ij}^{p}. The coefficient δ_1 measures the causal effect of parents' schooling on children's schooling, including income effects if capital markets are imperfect, parenting effects if more-schooled parents are better parents and role-model effects if parents' schooling serves as standards for their children. The parental factors h_{ij}^{p} that affect children's schooling may be observed or unobserved; we focus on the latter because they are what cause biases in OLS estimates. Genetic endowments for abilities and motivations are important examples of such usually unobserved family factors. Note that these factors may affect child schooling directly (e.g., parents with more ability or more innate motivation may invest more in their children's) or indirectly (e.g., children with parents with more ability may have inherited greater ability and therefore achieve higher schooling); for simplicity, and at no cost in terms of our interpretations below, we consolidate the direct and indirect effects into h_{ij}^{p}.

In general δ_1 in Equation (1) cannot be identified by OLS regressions. The plim of the OLS estimator is:

$$\text{plim } \delta_{1OLS} = \delta_1 + \theta_1 \text{cov}(S_{ij}^{p}, h_{ij}^{p}) / \text{var}(S_{ij}^{p}) \qquad (2)$$

Identification of δ_1 requires the assumption that either θ_1 is zero or the unobserved parental factors h_{ij}^{p} are not correlated with parental schooling. These assumptions are very strong because, for example, genetic endowments of ability and motivation are likely to have affected parental schooling. The MZ fixed-effects approach in which parents are identical twins assumes that unobserved characteristics h_{ij}^{p} can be removed by using fixed-effects or within-estimates for MZs who have identical genetics at conception and substantially shared environments in childhood, so that δ_1 can be estimated consistently. For example, by taking the difference in schooling in Equation (1) between the children of MZ parents, the model becomes,

$$\Delta S_j^c = \delta_1 \Delta S_j^p + \theta_1 \Delta h_j^p + \Delta e_j^c \tag{3}$$

Using only MZ parents who are genetically identical in addition to having shared basically the same family environment in childhood so that $\Delta h_j^p \cong 0$), the least squares estimator from a regression of the difference in schooling between the children of MZ parents ΔS_j^c on the difference in schooling between the MZ parents ΔS_j^p is,

$$\text{plim } \delta_1 = \ \delta_1. \tag{4}$$

There are two identifying assumptions here: (1) MZ parents are identical in h_{ij}^{p} and (2) some MZ parents are non-identical in their years of schooling. Because within-MZ estimation needs large enough within-MZ variation in schooling, we check the within-MZ difference in schooling and find over 45% of adult MZ pairs in our data have differences in years of schooling (Table 2). Therefore, under the possibly strong assumption that the differences in schooling of the MZ parents are generated by some random events such as car accidents or injuries in childhood, the impacts of h_{ij}^{p} are differenced-out and the MZ fixed-effects estimator of δ_1 is consistent.

It should be noted that if there is unobserved heterogeneity beyond what is in h_{ij}^{p} that affects both parental schooling and children's schooling, the fixed-effects estimate of δ_1 is biased (Griliches, 1979; Bound and Solon, 1999). But bounds may be established on the true value of δ_1 in this case. For example, if the unobserved heterogeneity beyond what is in h_{ij}^{p} is positively correlated with both parents' schooling and children's schooling, then the fixed-effects MZ estimate is an upper bound for the true value of δ_1 (Behrman et al., 2011; Kohler, Behrman and Schnittker, 2011; Amin et al., 2015). Li, Liu and Zhang (2012) find that the within-MZ fixed-effects estimator is less biased than the OLS estimator when estimating returns to schooling, even after they consider the potential endogeneity of schooling differences between MZs.

Measurement error is also more of a problem with the twins approach than with OLS level estimates because within-MZ differencing, as with any fixed-effects procedure, amplifies classical measurement error bias towards zero. Ashenfelter and Krueger (1994) and Behrman, Pollak and Taubman (1994) note that self-reported schooling is usually measured with error and propose to correct for measurement error by using a report on schooling from another source as an instrumental variable, for example using twin 1's schooling reported by twin 2 or using the twins' schooling reported by their adult children.

The estimate of δ_1 in Equation (3) is the gross effects of parental schooling on their children' schooling, inclusive of assortative mating. We can run separate regressions for fathers who are twins or for mothers who are twins, without controlling for their spouse's schooling. However, if we are concerned about how raising the schooling of fathers or mothers alone affects the schooling of their children, we have to include the schooling of both parents in Equation (1) and estimate the net schooling effects for each parent, excluding assortative mating effects that may enlarge any effects of parental schooling because improved schooling is associated with higher-quality spouses and greater resources in the household.

To this point in this section we have referred to parental schooling and other factors, but have not differentiated between the parents. If children's schooling outcomes are influenced by both parents, then the parental schooling effect δ_1 in Equation (1) includes both the effect of MZ parents and their spouses. While the MZs may have equal endowments, in general their spouses do not. Behrman and Rosenzweig (2002) take both unmeasured heritable traits and marital sorting into account when estimating the effects of mothers' schooling on their childrens' schooling. They consider two fundamental problems with interpreting intergenerational schooling associations between women and children as causal. The first is the unobserved variable bias if unobserved parental endowments h_{ij}^{p} are correlated with parental schooling S_{ij}^{p} as discussed below Equation (1). The second is marital sorting, because more-schooled women tend to marry more-schooled men who tend to have greater endowments given positive endowment-schooling correlations. Thus, to obtain the net effect of one parent's schooling, it is necessary to test whether the endowments of the two parents are correlated with each other's endowments and schooling, as a result of nonrandom matching in the marriage market. If there is assortative mating as reported in many studies (e.g., Mare, 1991; Behrman and Rosenzweig, 2002; Mare and Maralani, 2006), then spousal schooling and endowments should be taken into account in analysis of intergenerational schooling effects.

We posit that there is assortative mating, along the same lines as Behrman and Rosenzweig (2002):

$$S_j^{m} = \gamma_1 S_j^{f} + \gamma_2 h_j^{f} + \omega_{ij} \tag{5}$$

Equation (5) relates the schooling of mothers, superscript m, to the schooling and endowments of fathers, superscript f (a symmetrical relation holds for the schooling of fathers). γ_1 is the effect of fathers' schooling on the schooling of the spouses they obtain in the marriage market, γ_2 is the effect of fathers' endowments on the schooling of the spouses they obtain in the marriage market, and ω_{ij} is a stochastic disturbance. From the parameter γ_1, we can evaluate whether there exists assortative mating on parents' schooling, net of their endowments. However, it is worth noting that if γ_2 is nonzero and there is correlation between one's schooling and endowments (S_j^{f} and h_j^{f}, or S_j^{m} and h_j^{m}), then estimates of γ_1 from cross sections will be different from the estimated γ_1 using MZ fixed-effects estimators because the former includes the effects of assortative mating on unobserved endowments.

Given assortative mating, we need to take both mothers' and fathers' schooling and endowments into consideration to obtain estimates of the net effects of each parent's schooling, as in Behrman and Rosenzweig (2002):

$$S_{ij}^{c} = \delta_1 S_j^m + \delta_2 S_j^f + \theta_1 h_j^m + \theta_2 h_j^f + \varepsilon_{ij}^c \tag{6}$$

where S_{ij}^{c} is the schooling of child i in family j, S_j^m and S_j^f are the schooling of the mother and father respectively, h_j^m and h_j^f are the endowments for the two parents, and ε_{ij}^c is a child-specific characteristic.

Behrman and Rosenzweig (2002) divide parents' endowments into two parts, earnings endowments and parenting endowments. However, since we cannot identify the effects of parenting endowments and other endowments separately and both types of endowments may reflect both genetic and environmental factors, we do not differentiate parenting endowments from earning endowments; instead we include only one parental factor (e.g., h_j^f) in Equations (5) and (6).

If the mothers are the twins, then the mothers' common endowments h_j^m can be eliminated by differencing the above equation between mothers within-MZ pairs. But the difference in fathers' endowments remains. The fixed-effects MZ estimates of paternal schooling effects include not only the effects of paternal schooling on their children's schooling, but also the effects of whom they marry, leading to estimates of the gross effects of parental schooling on child schooling (inclusive of marital market effects) but upward bias in the estimate of the net effects if there is positive assortative mating on both schooling and endowments. To avoid the possible bias in our net estimates caused by omitted fathers' endowments, we first estimate fathers' endowments. Fathers' earnings could be used as a measure for their endowments and included in Equation (7). However, adding earnings in the equation will lead to a downward bias in the estimate of the fathers' schooling effect on their children's schooling because schooling and earnings are positively correlated. Therefore, we need to remove the effect of schooling from earnings by estimating the determinants of earnings. With information on fathers' schooling, work experience and earnings, we can estimate fathers' endowments h_j^f by using the following earnings equation:

$$log\, Y_{ij} = \beta S_{ij} + \beta_E E_{ij} + h_{ij} + v_{ij} \tag{7}$$

where S_{ij} and E_{ij} are schooling and post-school work experience of the ith member of family j, log Y_{ij} is the log earnings, and h_{ij} is an unobserved endowment, with an orthogonal stochastic disturbance term v_{ij}.

It is well-known that OLS estimates of Equation (7) are biased due to omitted endowments. So we estimate Equation (7) by using MZ fixed effects to eliminate own-endowment bias. If we assume that there is no difference in the returns to schooling and work experience and the distribution of earnings shocks between twins and non-twins, we can apply the estimated parameters in Equation (7) using MZs, to their spouses and obtain their spouses' earnings endowments.

One measure of unobserved earnings endowments is,

$$h_j^f + v_j^f = log\, Y_j^f - (\beta S_j^f + \beta_E E_j^f) \tag{8}$$

The residuals in equation (8) exclude the effects of schooling and experience from earnings, but they still include both the earnings endowment h_j^f and the noise term v_j^f. If v_j^f is mostly measurement error or is an independently and identically distributed shock, then the residuals obtained in equation (8) measure endowments with error. This means that, in general, all coefficients will be biased and inconsistent if endowments and schooling are correlated because generally measurement error in a single variable causes inconsistency in all estimates (Wooldridge, 2008). So we follow Behrman and Rosenzweig (2002), who constructed an alternative measure of the spouse endowment that nets out the noise term:

$$h_j^f = log\, Y_j^f - (\beta S_j^f + \beta_E E_j^f + v_j^f) \tag{9}$$

Ⅲ. Data

We use the Chinese Twins Survey. This survey and questionnaire were designed by Mark Rosenzweig and Junsen Zhang, and carried out by the Urban Survey Unit of the National Bureau of Statistics in June and July 2002 in five Chinese cities, including Chengdu, Chongqing, Harbin, Hefei and Wuhan. Based on existing twins questionnaires in the U.S. and elsewhere, this survey covered a wide range of socioeconomic information and was completed through household face-to-face interviews. Adult twins were identified by the local Statistical Bureau through multiple channels, including colleagues, friends, relatives, newspaper advertising, neighborhood notices, neighborhood management committees, and household records from local public-security bureaus. The various channels created a roughly equal probability of contacting all of the twins in the surveyed cities, which makes the twins sample obtained approximately representative of twins pairs who live in the same cities. The survey was conducted with considerable care. Junsen Zhang made several site checks and closely supervised and monitored the data inputting.

The Chinese Twins Survey is the first socioeconomic twins dataset in China. It includes 3012 individuals from twins households, with adult twins (both identical or monozygotic, MZ and non-identical or dizygotic, DZ) born between 1940 and 1985. Twins are considered MZ if both twins in a pair responded that they have identical hair color, looks and gender. 914 complete pairs of MZs (1828 individuals) are identified for the following analysis. The summary statistics for these MZs are reported in Table 1.

To analyze the effects of parental schooling and endowments on children's schooling, we need data on parents' schooling, earnings and children's schooling. The data set has information on each individual twin's schooling, his or her twin's schooling, the spouse's schooling and the schooling of the twin's spouse. The years of schooling of the individual and the twin reported by the respondent are the sum of all of the actual years of schooling that these twins attended at each schooling level, regardless of whether they graduated or not. The respondents' spouses' schooling is directly obtained from the question "How many years did your current spouse spend on formal schooling from elementary school on?", and the schooling of the twin's spouse is obtained from the question on the respondent's sibling "If this person is married, what is the highest schooling attainment of this person's spouse?". Thus, we have two reports for the schooling of each of the twins and twins' spouses (one reported by the respondent and the other by his or her twin.). We have information on each child's highest schooling attainment, and calculate children's years of schooling by considering primary school=6 years of schooling, middle school=9, high school=12, technical school=12, college=15, university=16, masters and above=18. We obtain information on the reported previous month's income from wages or salaries, including bonuses and allowances for respondents and spouses. Lifetime work experience for the respondent is the number of years in full-time work since the age of 16. The spouses' work tenure is calculated using age minus the years of schooling minus 6 (the assumed primary school entering age). This calculation may overestimate the spouses' work tenure, but we have no information that permits better estimates.

To investigate whether within-MZ estimates are closer to average treatment effects (ATE) for broader populations rather than local average treatment effects (LATE) obtained by IV-approaches that are based on compulsory schooling variations and individuals influenced by the instruments employed, we need to see if within-MZ schooling differences exist over most schooling levels rather than just over a narrow range in the distribution of schooling. In addition, the existence of differences in schooling outcomes of twins helps provide evidence that it is reasonable to assume that some twins parents are non-identical in their years of schooling, although the assumption underlying the generation of differences in twins' schooling may be strong. Table 2 provides detailed analysis of differences in years of schooling within MZ pairs. On average, the mean difference in years of schooling within pairs is 1.2, for total male and female MZs used for our analyses. Over 45% of twin pairs have differences in years of schooling with considerable within-MZ pair variation. The variation in years of schooling exists across substantial ranges in the schooling distribution. To demonstrate the pattern of schooling differences across the schooling distribution, Table 2 summarizes differences in years of schooling for twins pairs in which at least one twin has attained one of the following educational categories: (1) middle school or below (9 years of schooling or less), (2) high school (10-12 years of schooling), (3) college (13-15 years of schooling) or (4) university or above (16 years of schooling or more). When at least one twin has 16 years of schooling or more, the within-MZ differences are the largest, 2.5 and 2.8 years for females and males respectively.

Ⅳ.Results

1. Estimation of the Determinants of Earnings using Twins Sample

To estimate equation (6), we need to construct measures of spouses' endowments from equations (7) and (8). In this section, we report the estimated returns to schooling using different methods. We estimate the earnings equation (7) by using 492 pairs of MZs with earnings data. We start with the OLS regressions using the whole MZ sample, and then conduct the within-MZ estimation. We allow schooling to be measured with error and use the cross-twins reports of schooling as an instrumental variable for the individual's schooling to eliminate the bias caused by random measurement error.

The first two columns of Table (3) report OLS and OLS with measurement-error-correction estimates of the effects of schooling and work experience and age on the log of monthly earnings from the MZ sample. The estimates indicate that both schooling and work experience have statistically significant coefficient estimates, and measurement error in schooling biases downward the estimates of schooling returns. The results suggest that one more year of schooling increases an individual's earnings by 8.4% and 8.8%. However, with control for unobserved endowments by applying within-MZ fixed effects, the estimated returns to schooling are much smaller, as shown in the last two columns of Table (3). The estimates of within-MZ and within-MZ with measurement-error correction show that one year more schooling leads to an increase in an individual's earnings by 2.7% and 3.3%, which are still statistically significant but much smaller than the OLS estimates. The comparison of estimates between OLS and within-MZ indicates that there is a positive correlation between the unobserved endowments and schooling. The OLS estimates overstate schooling returns. Consistent with previous studies (Behrman and Rosenzweig, 2002; Amin, 2011), measurement error in schooling causes underestimates of schooling returns. Our estimates are similar to those obtained in earlier studies on earnings returns to schooling based on the Chinese Twins sample (Zhang et al., 2007 and Li et al., 2012).

2. Assortative Mating: Effects of Own Schooling on Spouse's Schooling

The results from estimating the earnings equation (7) indicate that schooling is correlated with own endowments. Now we need to investigate whether there is a relationship between own endowments and spouses' schooling. We estimate the assortative mating equation (5) using a subsample of the MZ pairs in which both twins were married. Table 4 reports the results from OLS, OLS with correction for measurement-error, within-MZ, and within-MZ with correction for measurement-error estimation. Our estimates suggest that in comparison with estimates from within-MZ twins, OLS estimates of assortative mating on schooling are biased upwards, overstating the effect of own schooling on the spouse's schooling. Measurement errors cause downward biases in both OLS and within-MZ twins estimates, just as for the estimates of own earnings effects of schooling in Table 3. The OLS estimates indicate that a one-year increase in wives' schooling results in husbands with 0.51 years higher schooling, and a one-year increase in husbands' schooling results in wives with 0.53 years higher schooling. Results from within-MZ twins are also positive but much smaller. For example, within-MZ estimates show that a one-year increase in husbands' schooling only increases the schooling of the spouses they attract by 0.23 years, about one-half of the OLS estimate.

The differences between the OLS and within-MZ estimates of the impacts of own schooling on spouse's schooling are indicative of the extent to which there is assortative mating on unobserved endowments. When we net out endowment effects by first-differencing within MZ pairs, the effect of higher schooling of an individual of given endowments on his or her partner's schooling is nearly 50% less than estimated by the cross-sectional associations between the schooling of spouses. It is obvious that there exists assortative mating between MZ twins and their spouses on endowments that are correlated with schooling.

3. Intergenerational Schooling Effects

In this section, we estimate the effects of parental schooling on their children's schooling by taking into consideration the role of unobserved endowments. We use subsamples of the MZ twins in which each twin in the twins-pairs was currently married and had at least one child aged 16 or older. This subsample has 272 individuals. We report the means and standard deviations for the key variables in Table 5. We restrict the child's age to 16 or older for two reasons. First, if we assume that children begin schooling at the age of 6, then after they finish 9 years of compulsory schooling, they will be 15 years old. The decision whether or not children will continue their schooling after 9 years of compulsory schooling depends on children and their parents. So if there is an effect of parents' schooling on children' schooling, then the impact will only be possible after children finish their compulsory schooling. Second, in China children aged 16 or older can enter the labor market and become employed.

The sample characteristics of the couples with MZ mothers' and with MZ fathers' are given in Table 5. The average years of schooling are 9.8 for MZ males and 9.7 for their spouses, while MZ females have higher years of schooling, 10.6 for female twins and 11.0 for their husbands. For both MZ males and females, there is not much difference in the years of schooling and monthly earnings between husbands and wives, although husbands on average have more years of schooling and earnings than their wives. Husbands also are 2 years older on average than their wives. As for children, the average age of children is above 20, and most have finished high school.

Table 6 reports the estimates of the gross effects of parental schooling on the children's schooling including assortative mating based on the subsample of married MZs.

Columns 1 and 3 in Table 6 report OLS estimates that can be interpreted as causal under the assumption that unobserved endowments are uncorrelated with schooling. The results indicate that the effect of mothers' schooling on their children's schooling is positive and significant. Without fathers' schooling included, a one-year increase in the schooling of mothers results in a 0.4 year increase in the schooling of their children.

The results in Column 2 show the gross effect of maternal schooling on children's schooling, by using within-MZ mothers estimators and controlling for the potential impact of mothers' endowments that may be correlated with their own schooling and with those of their spouses. When the impact of mothers' endowments is controlled by employing the MZ fixed-effects strategy, the gross effect of mothers' schooling, including the effect of their schooling on whom they married, is much smaller than the OLS estimate and insignificant. The comparison with the results in the first column, when we exclude fathers' schooling and endowments, suggest that the positive OLS relationship, inclusive of the effects on whom she marries, between mothers' schooling and children's schooling results from the correlation between mothers' unobserved endowments and schooling. The MZ fixed-effects estimates suggest, in contrast, that increases in the schooling of mothers with the same endowments have no significant effect on the schooling of their children.

Considering that women and men play different roles in childrearing, we do not expect the results from MZ females and males to be identical. The last two columns report estimates of the effects of paternal schooling on children's schooling for the subsample of MZ fathers by using MZ fixed effects to control for the impact of fathers' endowments. The OLS estimates of fathers' schooling effects are positive and statistically significant and are larger than the estimates for mothers' schooling. The results in the third column suggest that were a causal interpretation appropriate, an increase in fathers' schooling by one year would raise children's schooling by 0.5 years, 25% more than the OLS estimate of maternal schooling effects. However, using MZ fixed effects to control for fathers' endowments, the gross paternal schooling effect becomes negative and statistically insignificant in Column 4, which suggests that the significant positive relationship between fathers' and children's schooling is mainly due to the correlation between fathers' endowments and schooling. Therefore, these estimates imply that among fathers with the same endowments, those who are more-schooled may have children who are if anything less-schooled, including the effect of their schooling on whom they married, though this coefficient estimate is not significantly nonzero at conventional levels.

Because of positive assortative mating between MZ females and their husbands, as shown in Table 4, we add husbands' schooling into the regression to eliminate the influence of women's schooling on the schooling of the husbands whom they attract in the marriage market and thus obtain estimates of the net effects of women's schooling in Table 7. The first two columns report the OLS estimates. The results indicate that, compared with a 0.4 year increase in the schooling of their children in Column 1 in Table 6, including fathers' schooling reduces the mothers' schooling coefficient estimate by 25%, a reduction that reflects assortative mating on schooling between women and their husbands. When fathers' earnings are included as estimates of their earnings endowment in Column 2, the estimated fathers' schooling effect becomes insignificant while the coefficient of mothers' earnings is positive and significant. The results indicate that the effect of mothers' schooling on their children's schooling is positive and significant, whether or not fathers' schooling is included in the regression, comparable to the cross-sectional results in the literature.

After controlling for both women's endowment and husbands' schooling in Column 3, the coefficient estimate of mothers' schooling is still positive but becomes smaller and insignificant. The last three columns report the regression results when we take fathers' endowments into consideration. The coefficient estimates of maternal schooling are small and insignificant no matter whether fathers' earnings endowments are measured by actual earnings, or by actual earnings net of the effect of schooling and work experience.

The effect of fathers' schooling on their children's schooling is positive in all specifications except the second one, but only the coefficient estimate in the first column is significant, which suggests that the estimates are sensitive to the inclusion and measurement of fathers' endowments. Without fathers' endowments, as shown in Column 1, an increase in fathers' schooling by one year significantly raises their children's schooling by 0.2 years. However, when fathers' endowments are included, the estimated effect of fathers' schooling becomes negative and insignificant in Column 2. In Columns 3-6, when mothers' endowments are controlled for, the estimated fathers' schooling effect is still positive but small and not significant, whether or not fathers' earnings endowments are added and no matter how fathers' earnings endowments are measured.

Table 8 reports estimates of the net effects of paternal schooling on children's schooling for the subsample of MZ fathers after controlling for wives' schooling. The cross-sectional estimates of fathers' schooling effects in the first column suggest that an increase in fathers' schooling by one year would raise children's schooling by 0.5 years, basically the same as the gross estimate obtained without mothers' schooling included in Column 3 in Table 6. However, after controlling for fathers' endowments by applying within-MZ fixed effects, the paternal schooling effect in Column 3 is marginally negative though fairly imprecise and not robust to changes in model specifications (e.g., including adding representations of mother's endowments in Table 8, and the robustness checks), under which the coefficients of fathers' schooling remain negative but are no longer even marginally significant. The result in Column 3 suggests that the positive relationship between fathers' and children's schooling is mainly due to the correlation between fathers' endowments and schooling. Including mothers' schooling reduces the estimated paternal schooling effect, which reflects assortative mating on schooling between MZ fathers and their wives. Taking mothers' endowments into account decreases substantially the sample size, because most of the mothers surveyed have no earnings. When we control for, in addition to fathers' endowments, mothers' schooling and endowments, the paternal schooling coefficient estimate is negative and insignificant. However, the association between mothers' endowments and children's schooling is positive, but also not significant.

This paper finds no significant effect of mothers' schooling, which is consistent with previous findings in many twins' studies (Antonovics and Goldberger, 2005; Holmlund et al., 2011). Behrman and Rosenzweig (2002, 2005) find a negative maternal schooling effect, but the effect is only marginally significant at the 10 percent level. Our result also confirms the negligible role of maternal schooling obtained using different identification strategies such as adopted children (Plug, 2004, Bjorklund et al., 2006), although a small positive LATE maternal schooling effect is found in studies based on an IV approach (Black et al., 2005).

Our results indicate that the fathers' schooling effect is negative but statistically insignificant or only marginally significant. Although our findings are in contrast to the positive effect of paternal schooling found in other studies using twins (Behrman and Rosenzweig, 2002, 2005; Antonovics and Goldberger, 2005; Pronzato, 2012) and adopted children (Plug, 2004; Bjorklund et al., 2006), they are consistent with those of Black et al. (2005) who employ compulsory schooling reform as an instrumental variable.

Ⅴ. Robustness Checks

This section presents a series of robustness checks relating to measurement errors, the sample size, the timing of the schooling of the parents, children who are still in school at the age of 16, schooling effects of other family members, and including birth weights to at least partially represent endowments.

A. Measurement Errors

One important issue with twins fixed-effects estimates, as noted above, is measurement error. It is well-known that classical measurement error in regressors leads to a bias towards zero in the regression coefficient estimates. If reported schooling measures true schooling with random error, then estimates obtained by differencing across MZs are likely to magnify the bias due to such measurement error, although it may solve the problem of omitted variable bias.

Fortunately, we can solve the problem of measurement error bias by making use of instrumental variables using other reports. Ashenfelter and Krueger (1994) suggested two good instrumental variables. Suppose twins report their own and their siblings' schooling and thus we have two measures of each individual's schooling. Write S_j^k for twin k's report of twin j's schooling and allow for classical measurement error in schooling. In the differenced equation, we use $S_1^1 - S_2^2$ as the regressor and $S_1^2 - S_2^1$ as the instrumental variable. The IV estimator will be consistent, and we call this IV model as IVFE-1, as in Li, Liu and Zhang (2012).

Next, we further relax the classical assumption that the measurement errors in S_1^1 and S_2^1 (or S_1^2 and S_2^2) are uncorrelated. It is possible that a twin who reports an upward-biased measurement of the schooling of both his own and his twin, and thus the measurement errors in S_1^1 and S_2^1 are positively correlated due to an individual-specific common measurement-error component. In the presence of correlated measurement errors, the IVFE-1 estimates will be biased if the measurement error terms in $S_1^1 - S_2^2$ and $S_1^2 - S_2^1$ are correlated. Therefore, we use another instrumental variable to obtain a consistent estimator. Ashenfelter and Krueger (1994) suggested the use of $S_1^1 - S_2^1$ as the regressor and $S_1^2 - S_2^2$ as the IV to eliminate the common measurement error in one respondent's reports. We call this model IVFE-2.

Specifically, in our study twins report their own, their spouses', their twins' and their twins' spouses' schooling. Thus, we have two measures for the schooling of each respondent and their spouse: (1) schooling reported by the respondent, and (2) schooling reported by the respondent's twin. Tables 9 and 10 report the results of MZ fixed-effects estimates of the effects of parental schooling on children's schooling using the instrumental variable method for married MZ females and males. Overall, our IVFE estimates suggest that parents' schooling has no significant impacts on their children's schooling after controlling for parents' endowments.

B. Sample Size

As discussed earlier, when we restrict our samples to twins who are married and with children older than 16, our final sample size becomes small, which may raise concern about the power of our results. To address this problem, we conduct several robustness checks. First of all, we employ re-sampling methods as robustness checks, similarly to Gertler et al. (2013) and Heckman et al. (2010, 2014). To illustrate, we compute p-values using small-sample permutation tests, for which only the assumption of exchangeability is required. Freedman and Lane (1983) prove that for simple regression of a dependent variable y on an explanatory variable x, permutation tests are applicable under the null hypothesis that the coefficient of x should be 0 and the resulting tests yield exact significance levels. For within-MZ estimation, we do N=1000 permutations. As shown in Columns 1-2 in Table 11, the p-value for the two-sided test for male MZ twins is 0.072, significant at the 10% but not the standard 5% level, while the p-value for female MZ twins is 0.274, insignificant.

Secondly, we also explore increasing the sample size by including both MZ and DZ twins, regardless of whether they have income or are currently employed. This increases the sample sizes by factors of about 2-3. As shown in Columns 3-6, the results indicate that the schooling of both fathers and mothers have no significant effects on the schooling of their children when the sample sizes are increased by including both MZ and DZ twins. Thus, concerns over the sample size of our study are somewhat mitigated.

C. The Timing of Schooling of Parents

Another possible concern is the heterogeneity caused by the Cultural Revolution (CR) with respect to the timing of the schooling of the parents. The school interruption caused by the tumult of the CR during 1966-1976 influenced the schooling attainment of the population born in 1947-1966 and could result in a change in the relation between parental schooling and children' schooling. Because parental age is on average 50 in our sample in 2002, their schooling would have been disrupted by the CR, thus possibly resulting in a disconnect between parental and children's schooling for cohorts for which parents were young adults during the Cultural Revolution. To investigate this issue, we explore what happens if we restrict our sample based on the CR. Following Zhang et al. (2007), we define the CR cohort as those who were aged 7-19 in 1966-1976 or aged 33-55 in 2002 when our twins data were collected. Firstly, we only use MZ twins as in the previous analysis and regress separately for CR cohorts and non-CR cohorts. The results in Columns 1-3 in Table 12 indicate that for male and female MZs, the schooling effects remain insignificant for both the CR cohort and non-CR cohort.

Additionally, we include both MZs and DZs and implement the same estimation procedure for CR and non-CR cohorts. As shown in Columns 4-7 in Table 12, the schooling effects of both fathers and mothers are insignificant for both CR and non-CR cohorts when we combine both MZs and DZs for the estimates.

As a final check on this issue, we also add a dummy variable for whether or not the individual was sent down during the CR, and add the interaction term of schooling and the sent-down dummy variable. The results in the last two columns in Table 12 show that there are no significant differences in the schooling effects between sent-down groups and groups that were not sent down. These numerous checks suggest that our results are not biased by the composition of the group of parents whose schooling would have been interrupted by the CR.

D. Children Still in School at Age 16

One concern might be that, some individuals have not completed schooling by age 16 (although they have finished nine years of compulsory education) and including children who have not completed schooling may bias the estimates towards zero. To investigate this issue, we test the sensitivity of our results by using only (a) children aged 18 or older and (b) only children who have completed their schooling. The results presented in Table 13 show that both paternal and maternal schooling are insignificant.

E. Schooling Effects of Other Family Members

An additional test addresses potential problems with the schooling effects of other family members such as children's uncles, aunts, or grandparents, because their schooling may be important in societies such as China. To check whether or not the schooling of uncles or aunts is associated with the schooling of their nephews or nieces, we add the schooling of the twins' siblings (except the co-twins) who have the highest level of education among all the siblings into the regression. The OLS results in Columns 1-4 in Table 14, based on both MZ and DZ twins, indicate that uncles' schooling has significantly positive associations with children's schooling for both male and female twins, and after controlling for uncles' schooling, both paternal and maternal schooling effects are insignificant. The coefficients of aunts' schooling are significant only for male twins and insignificant for female twins. The within-twin estimates, however, control for the schooling effects of uncles or aunts who are the twins' siblings since there are identical for both twins in a pair.

In addition, we also add the schooling of grandparents into the regressions, because in China it is common that children are raised by their grandparents, especially for the left-behind children whose parents migrate to other cities to work. As shown in

Columns 5-8 in Table 14, the schooling of grandparents is significantly positively associated with the schooling of their grandchildren for male twins, although the estimated grandmothers' schooling effect is relatively smaller than that of grandfathers. The grandparents' schooling effects are insignificant for both male and female twins. We also take the schooling of grandparents-in-law into consideration and find that the coefficients of their schooling are not significant. However, the grandparental schooling effect again is controlled in within-twin estimation since both twins in a pair have the same grandparents.

In Table 14, we only consider the schooling of the most highly-educated non-twin sibling. What if the sibling with the highest level of education is the co-twin? In our sample including both MZ and DZ twins, individuals whose most highly educated sibling is their co-twin account for 28%. Thus, we add the schooling of the most highly educated sibling including co-twin siblings into the regression and apply the within-twins fixed-effects model. The results in Table 15 show that the schooling of both fathers and mothers does not have significant coefficients after controlling for the schooling of uncles or aunts including the co-twins.

F. Control for Endowments Using Birth Weights

To this point we have controlled for endowments at conception using MZ fixed-effects estimates. A number of studies have used birth weights to represent endowments at birth (Conley and Bennett 2000, 2001; Currie and Hyson 1999; Richards et al 2001). Most of these studies are cross-sectional, which raises a question of interpretation of what birth weights are representing because birth weights are correlated with observed family background characteristics such as parental education and income. However, as emphasized in Behrman and Rosenzweig (2004) and subsequent studies using twins including Rosenzweig and Zhang (2009) using the same Chinese twins that we use in this study, MZ fixed-effects estimates of birth weight impacts control perfectly for endowments at conception and thus the birth weight estimates using this approach represent the part of the endowment at birth due to differential exposure *in utero*, which reflects chance factors such as differential proximity to the placenta but not any conscious decisions of the parents. We have data on the twins' birth weights, though not on the birth weights of their spouses. In Table 15 we present estimates of the gross effects of parents' schooling with the twin parent's birth weights included. For female MZ twins, the MZ fixed-effects estimates suggest that the part of endowments at birth represented by birth weights is a significant and substantial predictor of child schooling, with 3.6 additional years of child schooling for every addition kilogram of mother's birth weight. For males, birth weight does not have a significant coefficient estimate. But for the purpose of this study, the important point is that with control for birth weights, the MZ fixed-effects estimates of both mother's and father's schooling remain positive but very small and insignificant. Thus, the basic results of this study are robust to controlling for endowment changes *in utero* in addition to endowments at conception.

Ⅵ. Conclusion

In this paper we estimate the causal relationship between parents' schooling and their children's schooling in China. We use adult MZ twins data to control for unobserved parental endowments. We find that the positive cross-sectional relationship between the schooling of parents and their children that is estimated by the OLS model that dominates in the literature is substantially biased upward as a result of correlations between schooling and unobserved endowments. Our findings suggest that the endowments play more important roles than schooling in explaining the positive association between parental schooling and their children's schooling.

Our OLS estimates show a positive relationship between parents' schooling and their children's schooling, comparable to those cross-sectional regression estimates in the literature. The OLS estimates indicate that one-year increases in maternal and paternal schooling are associated with higher children's schooling by 0.4 and 0.5 years, respectively. However, our MZ fixed-effect estimates, controlling for parental endowments, indicate that mothers' schooling does not have beneficial impacts on their children's schooling and fathers' schooling coefficients are negative but statistically insignificant or only marginally significant. These results suggest that the positive relationship between children's and parents' schooling is due to the correlation between unobserved endowments and schooling.

Our sensitivity analyses suggest that the twins fixed-effects estimates are biased toward zero because of measurement error. However, correction of measurement error does not make the maternal and paternal schooling coefficient estimates significantly positive. Our main results remain consistent in various other robustness checks.

In contrast to estimated results based on cross-sectional regressions, our findings clearly indicate that increasing mothers' or fathers' schooling would not significantly raise the levels of schooling of their children. The results indicate that after we control for the endowments that twins share in common, such as unobserved inherited genetic endowments and family background, parental schooling has no significant effect on children's schooling. The OLS estimates thus represent not only the effects of schooling itself, but of unobserved endowments such as abilities and motivations that are correlated with parental schooling and that directly affect investments in children's schooling.

Why does parental schooling itself (net of the endowment effects) not have a positive effect on children's schooling? That mothers' schooling plays no significant role in children's schooling may be because the schooling level of mothers in our sample is not high; most of them just finished middle school (the average of mothers' schooling is less than 10 years). Low-schooled mothers may lack sufficient knowledge and parenting skills. Fathers with higher schooling may spend more time in the labor market and thus less time in childrearing activities that influence children's academic performance even though fathers who work more may serve as positive role models for their children (Woelfel & Haller, 1971). Based on data from the Chinese Child Twins Survey in 2002, we construct three measurements of fathers' home time and results in Table 17 indicate a significantly negative relationship between fathers' years of schooling and their possible home time spent with their children.

Taking these results (although they may be context-specific and may not generalize to countries where parents have different average schooling levels) at face value, they offer mixed policy recommendations. Raising parents' schooling may not increase children's schooling. Thus, if governments desire to increase children's schooling, they may need to jump completely out of the parental channel and focus more on more direct interventions on children's schooling, e.g. improving pre-school access, especially for the poor.

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Table 1 Descriptive Statistics of the Twins Sample: Male and Female MZ Twins

Variable	All MZ twins	Male MZ twins	Female MZ twins	Married MZ twins	Married Male MZ twins	Married Female MZ twins
Schooling (years)	12.2	12.1	12.4	11.9	11.6	12.2
	(2.9)	(2.9)	(2.9)	(3.0)	(3.2)	(3.0)
Age	34.7	34.6	34.9	39.9	41.0	38.7
	(9.7)	(9.8)	(9.5)	(7.8)	(7.7)	(7.7)
Work experience (years in full-time work since age of 16)	15.0	14.9	15.1	20.2	21.3	18.9
	(9.9)	(10.1)	(9.8)	(8.5)	(8.4)	(8.5)
Earnings (log monthly wages including bonuses and subsidies)	6.6	6.7	6.5	6.7	6.8	6.5
	(0.6)	(0.5)	(0.6)	(0.6)	(0.6)	(0.6)
Spousal schooling (years)	11.7	11.1	12.3	11.5	10.9	12.3
	(3.1)	(3.1)	(3.0)	(3.1)	(3.1)	(2.9)
Spousal age	38.3	36.8	40.2	39.5	38.3	40.8
	(8.4)	(8.4)	(8.0)	(8.0)	(8.0)	(7.8)
Spousal earnings (log monthly wages including bonuses and subsidies)	6.7	6.5	6.9	6.7	6.6	6.9
	(0.6)	(0.7)	(0.6)	(0.6)	(0.7)	(0.6)
Sample size	984	586	398	558	298	260

Notes: The means and standard deviations (in parentheses) are reported in Table 1.

All MZ twins include married and non-married MZ twins, so in the table the mean of spouse's age is larger than that of MZ twins' age both for male and female MZ twins due to the young non-married twins whom we include because we use all the twins, married or not, in the earnings function estimates.

Table 2 Difference in Years of Schooling within Twins Pairs (MZ Twins only)

Variable	All MZ Twins	Female MZ Twins	Male MZ Twins
Absolute within-twins difference in years of schooling	1.2	1.1	1.2
	(1.68)	(1.67)	(1.69)
Number of twins pairs	918	401	517
By education level（twins pairs in which at least one twin has education category）			
Below High School	1.2	1.3	1.2
(9 years of schooling or less)	(1.89)	(1.99)	(1.82)
Number of twins pairs	359	152	207
High School	1.5	1.4	1.5
(10-12 years of schooling)	(1.59)	(1.51)	(1.65)
Number of twins pairs	500	220	280
College	2.0	1.9	2.0
(13-15 years of schooling)	(1.82)	(1.81)	(1.83)
Number of twins pairs	251	110	141
University	2.7	2.5	2.8
(16 years of schooling or more)	(2.22)	(2.42)	(2.09)
Number of twin pairs	116	47	69

Notes: The means and standard deviations (in parentheses) are reported in Table 2.

Table 3 Estimates of the Determinants of log Monthly Earnings with MZ Twins Sample: Male and Female MZ Twins

	Level-OLS (1)	Level-IV (2)	Within-MZ (3)	Within-MZ+IV (4)
Schooling	0.084***	0.088***	0.027**	0.033*
	(0.006)	(0.006)	(0.012)	(0.019)
Work experience	0.011*	0.011**	0.016*	0.017*
	(0.006)	(0.006)	(0.009)	(0.010)
Male	0.204***	0.205***		
	(0.032)	(0.031)		
Age	0.035***	0.034**		
	(0.013)	(0.014)		
Age squared	−0.001***	−0.001**		
	(0.000)	(0.000)		
Twin pairs			492	492
Observations	984	984		

Notes: Standard errors in parentheses.

* significant at the 10% level, ** significant at the 5% level, ***significant at the 1% level.

Table 4 Estimates of the Effects of Own Schooling on the Spouse's Schooling: Married MZ Twins

	Level-OLS	Level-IV	Within-MZ	Within-MZ+IV
	(1)	(2)	(3)	(4)
Married Female MZ Twins				
Schooling	0.509***	0.553***	0.132	0.341*
	(0.046)	(0.048)	(0.128)	(0.175)
Observations	400	400	200	200
Married Male MZ Twins				
Schooling	0.529***	0.536***	0.232**	0.248**
	(0.039)	(0.041)	(0.093)	(0.113)
Observations	422	422	211	211

Notes: Standard errors in parentheses.

* significant at 10% level, ** significant at 5% level, ***significant at 1% level.

Table 5 Characteristics of Parents and Children in Currently Married MZ Twins with Child≥16

	MZ Male Twins	MZ Female Twins
Mothers schooling	9.7	10.6
	(3.4)	(2.5)
Fathers schooling	9.8	11.0
	(2.9)	(3.1)
Mothers earnings	6.5	6.3
(ln monthly earning)	(0.8)	(0.8)
Fathers earnings	6.7	6.5
(ln monthly earning)	(0.5)	(0.8)
Mothers age	50.9	50.2
	(5.1)	(6.0)
Fathers age	53.0	52.3
	(5.0)	(6.5)
Childs age	22.0	21.5
	(5.0)	(5.6)
Childs schooling	12.3	12.2
	(2.7)	(2.5)
Number of twins	154	118

Notes: Standard deviations in parentheses.

Table 6 Estimates of the Gross Effect of Parents' Schooling on Children's Schooling: MZ Twins

	Female MZ Twins		Male MZ Twins	
	OLS	Within-MZ	OLS	Within-MZ
	(1)	(2)	(3)	(4)
Mothers schooling	0.403***	0.149		
	(0.109)	(0.121)		
Fathers schooling			0.514***	−0.322
			(0.140)	(0.199)
Observations	118	118	154	154

Notes: Standard errors clustered at the family (twins pair) level in parentheses.
* significant at 10% level, ** significant at 5% level, ***significant at 1% level.

Table 7 Estimates of the Net Effect of Parents' Schooling on Children's Schooling: Female MZ Twins

	OLS	OLS	Within-MZ	Within-MZ	Within-MZ	Within-MZ
	(1)	(2)	(3)	(4)	(5)	(6)
Mothers schooling	0.299***	0.306***	0.154	0.127	0.129	0.129
	(0.109)	(0.108)	(0.119)	(0.120)	(0.120)	(0.117)
Fathers schooling	0.216**	−0.081	0.120	0.169	0.158	0.128
	(0.102)	(0.139)	(0.072)	(0.161)	(0.149)	(0.064)
Fathers log earnings		1.359***		−0.152		
		(0.468)		(0.629)		
Fathers endowment with noise term					−0.104	
					(0.624)	
Fathers endowment without noise term						0.436
						(0.738)
Observations	116	100	116	90	90	90

Notes: Standard errors clustered at the family (twin pair) level in parentheses.

* significant at 10% level, ** significant at 5% level, ***significant at 1% level.

Table 8 Estimates of the Net Effect of Parents' Schooling on Children's Schooling: Male MZ Twins

	OLS (1)	OLS (2)	Within-MZ (3)	Within-MZ (4)	Within-MZ (5)	Within-MZ (6)
Mothers schooling	0.028	−0.159	0.162	−0.010	0.020	0.224
	(0.102)	(0.118)	(0.100)	(0.201)	(0.204)	(0.270)
Fathers schooling	0.500***	−0.053	−0.376*	−0.143	−0.140	−0.184
	(0.145)	(0.196)	(0.187)	(0.175)	(0.183)	(0.236)
Mothers log earnings		1.106**		1.492		
		(0.438)		(0.936)		
Mothers endowment with noise term					1.434	
					(0.915)	
Mothers endowment without noise term						0.704
						(0.552)
Observations	152	46	152	34	34	34

Notes: Standard errors clustered at the family (twins pair) level in parentheses.

* significant at the 10% level, ** significant at the 5% level, ***significant at the 1% level.

Table 9 Instrumental Variable within-MZ Estimates of the Effects of Parental Schooling on Children's Schooling: Married Female MZ Twins

	IVFE-1				IVFE-2			
Mothers schooling	−0.013	0.100	0.114	−0.178	0.299	1.052	0.655	0.196
	(0.686)	(0.151)	(0.16)	(7.02)	(0.41)	(6.48)	(2.095)	(0.177)
Fathers schooling	1.303	0.706	0.681	−6.512	1.152	3.664	2.062	0.386
	(2.273)	(0.660)	(0.634)	(129.4)	(1.496)	(26.52)	(8.258)	(0.386)
Fathers log earnings		−2.051				−4.001		
		(2.278)				(31.51)		
Fathers endowment with noise term			−2.011				−1.921	
			(2.282)				(9.352)	
Fathers endowment without noise term				9.500				0.884
				(174.5)				(0.749)
Twins pairs	58	45	45	45	58	45	45	45
Observations	116	90	90	90	116	90	90	90

Notes: Standard errors clustered at the family (twins pair) level in parentheses.

Table 10 Instrumental Variable within-MZ Estimates of the Effects of Parental Schooling on Children's Schooling: Married Male MZ Twins

	IVFE-1				IVFE-2			
Mothers schooling	0.418	−0.212	−0.142	0.446	−0.022	0.004	0.061	0.659
	(0.452)	(0.311)	(0.310)	(0.272)	(0.088)	(0.535)	(0.524)	(0.527)
Fathers schooling	−0.318	−0.136	−0.144	−0.326	−0.283	−0.174	−0.163	−0.213
	(0.434)	(0.232)	(0.233)	(0.282)	(0.246)	(0.224)	(0.234)	(0.351)
Mothers log earnings		1.576**				1.476*		
		(0.777)				(0.867)		
Mothers endowment with noise term			1.474*				1.399*	
			(0.754)				(0.840)	
Mothers endowment without noise term				0.835*				0.841
				(0.495)				(0.601)
Twin pairs	76	17	17	17	76	17	17	17
Observations	154	34	34	34	154	34	34	34

Notes: Standard errors clustered at the family (twins pair) level in parentheses.

* significant at the 10% level, ** significant at the 5% level, ***significant at the 1% level.

Table 11 Robustness Checks: Sample Size

	Permutation test		Twins(DZ+MZ)		Twins (DZ+MZ)	
	Male MZ Twins	Female MZ twins	Male (total)	Female (total)	Male (samples with earnings)	Female (samples with earnings)
	Within-MZ (1)	Within-MZ (2)	Within-twin (3)	Within-twin (4)	Within-twin (5)	Within-twin (6)
Father's schooling	−.322 (0.072)[a]		0.01 (0.133)[b]		−0.111 (0.121)[b]	
Mother's schooling		0.149 (0.274)[a]		0.095 (0.083)[b]		0.079 (0.103)[b]
Replications	1000	1000				
Number of twins pairs	77	59	140	183	123	143

Notes: a P-values in parentheses.

b Standard errors clustered at the family (twins pair) level in parentheses.

Table 12 Robustness Checks: the Timing of Parental Schooling

	Male MZ twins		Female MZ twins	Male twins (DZ+MZ)		Female twins (DZ+MZ)		Twins (DZ+MZ)	
	Within-MZ (CR cohort)	Within-MZ (non-CR cohort)	Within-MZ (CR cohort)	Within-twin (CR cohort)	Within-twin (non-CR cohort)	Within-twin (CR cohort)	Within-twin (non-CR cohort)	Within-twin Male	Within-twin Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Father's schooling	−0.300	−1.055		−0.076	0.108			−0.241	
	(0.197)	(0.473)		(0.153)	(0.240)			(0.319)	
Mother's schooling			0.158			0.083	0.191		0.130
			(0.161)			(0.098)	(0.168)		(0.176)
Sent down								−1.18	−0.053
								(1.920)	(1.363)
Schooling* Sent-down								0.157	−0.027
								(0.172)	(0.118)
Number of twins pairs	47	30	47	96	44	145	38	136	172

Notes: We define the Cultural Revolution (CR) cohort as those aged 7-19 in 1966-1976 or aged 33-55 in 2002. Because there are only 12 pairs of female MZ twins for non-CR cohort, an insufficient number of observations to perform a regression, results for the female non-CR cohort are not presented in the table. Standard errors clustered at the family (twins pair) level in parentheses.

Table 13 Robustness Checks: the Age of Children

	Male twins (MZ+DZ)		Female twins (MZ+DZ)	
	Within-twin (Child aged 18 or over)	Within-twin (Child having finished schooling)	Within-twin (Child aged 18 or over)	Within-twin (Child having finished schooling)
	(1)	(2)	(3)	(4)
Fathers schooling	0.098	0.083		
	(0.144)	(0.232)		
Mothers schooling			0.070	0.194
			(0.084)	(0.129)
Number of twins pairs	104	48	147	64

Notes: Standard errors clustered at the family (twins pair) level in parentheses.

Table 14 Estimates of the Effect of Other Relatives' Schooling: MZ and DZ Twins

	Male twins (MZ+DZ)		Female twins (MZ+DZ)		Male twins (MZ+DZ)		Female twins (MZ+DZ)	
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fathers	0.088	0.198**			0.178***	0.193***		
schooling	(0.088)	(0.076)			(0.064)	(0.061)		
Mothers			−0.016	0.180**			0.146***	0.133***
schooling			(0.076)	(0.079)			(0.052)	(0.051)
Uncles	0.222***		0.205**					
schooling	(0.075)		(0.088)					
Aunts		0.237***		0.002				
schooling		(0.074)		(0.090)				
Grandfathers					0.087*		−0.021	
Schooling					(0.047)		(0.050)	
Grandmothers						0.077*		0.044
schooling						(0.044)		(0.067)
Observations	144	124	172	166	278	278	364	363

Notes: Standard errors clustered at the family (twins pair) level in parentheses.

* significant at the 10% level, ** significant at the 5% level, ***significant at the 1% level.

Table 15 Estimates of the Effect of Uncles' or Aunts' Schooling: MZ and DZ Twins

	Male twins			Female twins		
	Within-twin	Within-twin	Within-twin	Within-twin	Within-twin	Within-twin
	(1)	(2)	(3)	(4)	(5)	(6)
Fathers schooling	−0.034	−0.051	0.020			
	(0.151)	(0.139)	(0.275)			
Mothers schooling				0.109	0.031	0.173
				(0.085)	(0.149)	(0.099)
Uncle/Aunts schooling	−0.182			0.067		
	(0.156)			(0.119)		
Uncles schooling		0.026			−0.129	
		(0.202)			(0.244)	
Aunts schooling			−0.301			0.178
			(0.217)			(0.137)
Number of twins pairs	140	76	64	183	62	121

Notes: Standard errors clustered at the family (twins pair) level in parentheses.

Table 16 Estimates of the Gross Effect of Parents' Schooling on Children's Schooling with Twin Parent's Birth Weight: MZ Twins

	Female MZ Twins		Male MZ Twins	
	OLS	Within-MZ	OLS	Within-MZ
	(1)	(2)	(3)	(4)
Mothers schooling	0.404***	0.028		
	(0.113)	(0.123)		
Fathers schooling			0.508***	-0.340
			(0.130)	(0.207)
Twin parent's BW	1.144	3.589***	-0.636	-0.289
	(0.587)	(1.076)	(0.557)	(1.229)
Observations	118	118	154	154

Notes: Standard errors clustered at the family (twins pair) level in parentheses.
* significant at 10% level, ** significant at 5% level, ***significant at 1% level.

Table 17 Relationship between Father's Schooling and Home Time

	Days living at home in the last six months	Days living at home in the last six months	Working within the local city	Working within the local city	Going out for dinner without kids	Going out for dinner without kids
	(1)	(2)	(5)	(6)	(3)	(4)
Years of Schooling	-0.543***	-0.217	-0.002***	-0.001*	0.033***	0.023***
	(0.190)	(0.231)	(0.001)	(0.001)	(0.002)	(0.003)
Han	-0.857	-0.889	-0.009	-0.009	-0.019	-0.018
	(1.820)	(1.818)	(0.007)	(0.007)	(0.021)	(0.021)
Age	0.158	0.174	-0.001	-0.001	-0.006***	-0.006***
	(0.116)	(0.116)	(0.000)	(0.000)	(0.001)	(0.001)
Employment	-4.812***	-3.885**	0.979***	0.983***	0.036*	0.007
	(1.625)	(1.666)	(0.006)	(0.006)	(0.019)	(0.019)
Skilled worker		-4.623**		-0.014**		0.144***
		(1.863)		(0.007)		(0.021)
Observations	3303	3303	3303	3303	3303	3303
Adj. R-squared	0.005	0.007	0.891	0.891	0.071	0.083

Notes: We construct three measurements of the home time of the fathers based on the survey questionnaire: days living at home during the last six months, a dummy variable indicating workplace (working in the local city=1, otherwise=0), and a dummy variable indicating social activities (going out for dinner without kids last month=1, otherwise=0). We define a dummy variable "working within the local city" based on the question in the questionnaire "where is your workplace?". There are four answers to the question including within the city, in other cities within the province, in other provinces, and abroad. The dummy variable "working within the local city" is equal to 1 if the respondents work in the local city, and zero otherwise. We define another dummy variable to indicate social activities based on the question "did you go out for dinner without your kids last month?". If the answer is yes, then the dummy variable "going out for dinner without kids" is equal to 1, and 0 otherwise. We create a dummy variable "skilled worker" to indicate respondents' occupation. The variable "skilled worker" takes the value 1 if the respondents are technical professionals/persons in charge of work place whether in public or private sector/clerks or managers, and 0 otherwise. "Employment" is a dummy variable indicating employment status (employed=1, otherwise=0). "Han" is a dummy variable indicating ethnicity (han=1, otherwise=0).

Standard errors in parentheses.

* significant at the 10% level, **significant at the 5% level, *** significant at the 1% level.