The Economic Returns to Schooling: Evidence from Chinese Twins

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ABSTRACT

This thesis attempts to estimate returns to education using a new data set of identical Chinese twins. To avoid constraints of conventional estimates, we utilize an exceptionally large sample of 914 pairs of identical twins to adjust our estimates for omitted ability variables, and also use instrumental variables to shed some light on the measurement error problem. The point estimates confirm the theoretical prediction that, measurement error biases estimated returns to education down, and omitted ability biases estimates up. In fact, omitted ability bias exceeds measurement error bias indicating private returns to education of 3.6 percent.

摘要

本文利用一組關于中國雙胞胎的新數據，來測算中國人口的教育回報值。這組包含了 488 對同卵雙胞胎的詳細數據，使我們能夠避免傳統測算的侷限，即解決了能力變量不可知和數據測量不準卻的問題。

我們得齣的點估計證實瞭傳統理論的假設，對于中國教育回報的測算，忽略能力變量將引起正偏差，而數據側量不準卻則帶來負偏差。事實上，正偏差的大小超過了負偏差的絕對值，從而最終達到 3.6 % 的教育回報率。

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1 Introduction

Every central or local government subsidizes primary and secondary education. People also tend to spend increasingly more money in their children's human capital investment. Therefore, the magnitude of returns to education is important for assessing the efficiency of both public and private investment in education. Although there have been papers looking at returns to education (Psacharopoulos 1992, 1994), most of them did not deal with the issue of ability, so that they cannot tell us whether the higher income-higher education relationship is due to the causal effect of schooling or due to unobserved ability that correlates with both educational experience and earnings. It has long been understood that application of least squares to a regression of the log wage rate on years of schooling and some control variables cannot generate coefficient estimate for years of schooling consistently. The problem is that the cross-sectional comparison of workers with different years of schooling does not generate consistent estimate even if the workers are identical with respect to the observed control variables. For example, if more educated workers tend to be more intelligent, motivated, or blessed with advantageous family backgrounds, and if these advantages are not completely accounted for by the measured control variables, then the more educated workers typically would have received higher wages even without their additional schooling. It therefore is difficult to ascertain how much of the empirical association between wages and schooling is due to the causal effect of schooling and how much is due to unobserved factors that influence both wages and schooling.

Over the last few years, several studies have pursued a strategy, which could date back to an Indianan University dissertation written 70 years ago by Gorseline (1932) (Bound and Solon 1999), of estimating the returns to schooling by comparing monozygotic (from the same egg, MZ) twins to solve this inconsistency problem. They solve the problem by contrasting the wage rates of identical twins with different schooling levels. The goal is to ensure that the correlation observed between schooling and wage rates is not due to a correlation between schooling and a worker's ability or other characteristics. In other words, they estimate the returns to education by taking advantage of the fact that MZ twins are genetically identical and have similar family backgrounds. Twins are likely to be more alike than a randomly selected pair of individuals on a variety of socioeconomic measurements. This correlation arises from many sources: common heredity, both physical and cultural; similar access to financial resources; exposure to similar influences of friends, neighbors, schools, and other aspects of their particular community; the likelihood, even in adulthood, of closer location in space and hence exposure to similar regional price differentials and common business-cycle effects; and more. Some of these effects are measurable, but many are not, or only imperfectly so. This leads then to the expectation that in models of socioeconomic achievement the disturbances, which represent the force of all 'other' unmeasured factors, will be correlated (positively) across twins. A major focus of their work has been the attempt to eliminate potential biases in estimates of the returns to schooling due to the presence of such unmeasured factors as 'ability' or 'family culture' by the use of differences between twins as the basic source of information. If MZ twins are identical with respect to these factors and if their schooling differences were randomly generated, this approach would generate consistent coefficient estimate that we desire.

For example, in a recent paper, Ashenfelter and Krueger (1994) have estimated the economic returns to education using data on a new sample of 149 pairs of identical twins which permitted them to adjust their estimate for omitted ability variables and measurement error. On this basis, their within-twin estimate of the returns to schooling is larger than the comparable Ordinary Least Squares (OLS) estimate, and their measurement error corrected estimates of the returns to schooling are also unusually large, with their most efficient estimate being approximately 13 percent. The findings of Ashenfelter and Krueger (1994) are very different from previous work in this field. And there is now some confusion as to what analysis of twin data really reveals about the economic returns to schooling. As Ashenfelter and Krueger (1994) remark at the end of their paper "Only additional data collection is likely to lead to better estimates of the returns to schooling."

Within the context of twins study in schooling returns, knowing the true returns is especially important for China, which is experiencing the transformation from a redistributive economy to a market economy. People would think that after the reform, returns to education in China should increase, since in market economies a large gradation in earnings by level of education reflects returns to individuals' investment in education (Mincer 1974; Becker 1993). In fact, this assertion has contributed to a lively debate among sociologists studying institutional transformation and social stratification in former state socialist societies (Rona-Tas 1994; Bian and Logan 1996; Parish and Michelson 1996; Szelenyi and Kostello 1996; Walder 1996; Xie and Hannum 1996; Gerber and Hout 1998; Zhou 2000). To answer this question, Zhang et al. (forthcoming) estimate the returns to schooling in urban China. They find a dramatic increase in the returns, from only 4.0 percent in 1988 -- right before a period of stalled reform following the inflationary episode of 1988 and the Tiananmen incident in 1989, to 10.2 percent in 2001 -- nine years after a period of rapid acceleration in the reforms following Deng Xiaoping's famous southeastern tour in early 1992 and Jiang Zemin's subsequent advocacy of a socialist market economy at the 14th Party Congress in October, 1992 (Myers 1995). However, one should be wary of the fact that their study does not take account of omitted ability variables or measurement error problem, so that true value of these returns still remains unavailable. Our study uses a new survey of Chinese identical twins to adjust our estimate for omitted ability bias, and also use instrumental variables to shed some light on the measurement error problem. Therefore we hope it could make some contribution to the literature dealing with ability bias and China issues.

Another concern in study of returns to education, especially twins study, is the impact of measurement error. Although the impact of measurement error probably is small for the cross-sectional estimator, Taubman (1976), Griliches (1979), and many subsequent researchers have explained that its impact may be much greater for the covariance estimator. Because MZ twins are highly correlated in their years of schooling, within-twin-pair differencing filters out most of the 'signal' component of schooling variation without a commensurate reduction in the 'noise' from measurement error. As a result, the covariance estimator probably is subject to a much more severe errors-in-variables inconsistency. An important innovation of the Ashenfelter and Krueger (1994) study is to ask each twin his/her own and his/her co-twin's education. If self-reported education is measured with error this provides a potential instrument since the report of the other twin should be correlated with the self-reported education level but uncorrelated with the equation regressed. This strategy was adopted in the subsequent Twinsburg studies, Miller et al. (1995), Behrman and Rosenzweig (1999) studies, and we use it too.

Owing to many good qualities of our data set, which enables us to avoid constraints of conventional estimates, we believe this study is of interest for three main reasons. First, this is the first study on China to present within-twin-pair estimates using identical twins. There have been many twins studies examining the estimated returns to education for developed countries and regions, but situation in developing countries may be a very different story. In fact, some researchers (e.g., Lam and Schoeni 1993) have suggested that omitted variables bias might be larger in less developed economies, where liquidity constraints and family background are likely to be important determinants of both education and earning. To date, however, there are few estimates of the returns to education based on twins sample from developing countries. The special interest in genetics and economic success could also be found in Herrnstein and Murray (1994) and Literature Review Section of this thesis. In a word, our data on genetically identical individuals are of particular value.

Second, we have followed Ashenfelter and Krueger's (1994) innovation of asking one twin to report on the schooling of the other, in order to examine possible measurement error, because the independence of measurement errors within the family is compromised by the fact that in this survey, as in many others (Hertz 2003), a single respondent provides information on the educational attainments of all family members: a respondent who is prone to exaggeration might well exaggerate consistently. Third, this study has more data on twins than other studies, i.e., smoking behavior. So we can shed some light on the smoking debate---whether smoking can be used as instruments for education.

The results of our study indicate that each year of school completed increases a worker's wage rate by 3.6 percent in China, very much lower than 10 percent in Rouse's (1999) MZ twins sample of U.S. This estimate is also much lower than the estimate we would have obtained from these data had we been unable to adjust for omitted ability variables and measurement error. We find strong evidence that unobserved ability is positively related to the schooling level completed. We also find significant evidence of measurement error in schooling levels. Our results indicate that measurement error may lead to considerable underestimate of the returns to schooling in studies based on identical twins.

The structure of the rest of this thesis is as follows: in the next section we review previous work in this field. In Section 3 we set out some simple theories. In Section 4 we describe the data and in Section 5 the results, including some further results. Section 6 contains concluding remarks.

2 Literature Review

The earliest attempt to look at siblings data in economics can be traced back to Gorseline's (1932) dissertation, which set the pattern for most of the work to come by asking whether education paid when one contrasted the different educational experiences of brothers (156 pairs from Indiana, with data on 1927 income, schooling, occupation, and age). His results were used by Becker (1964), and the data were reanalyzed by Chamberlain and Griliches (1975). Since then there has been a rash of publications analyzing different sibling samples: Various scattered pieces of data on siblings were synthesized and reviewed by Jencks et al. (1972). Jencks and his associates (Corcoran et al. 1976; Jencks and Brown 1977) have analyzed data on 99 pairs of brothers culled from the Talent Survey follow-up and on 150 pairs of brothers collected by the National Opinion Research Center (NORC) in 1973. Chamberlain and Griliches (1977) analyzed 292 pairs of brothers from the National Longitudinal Survey (NLS) of young men with data on expected schooling and occupation and 161 pairs with actual (1970) wage data. Brittain (1977) analyzed about 60 pairs of Cleveland brothers. Olneck (1976, 1977) collected and analyzed data on 346 pairs of brothers from Kalamazoo and Michigan. Table 1 summarizes these studies of estimated returns to schooling based on samples of brothers.

In a series of studies published between 1976 and 1980, Taubman and his associates (Behrman and Taubman 1976; Taubman 1976a, 1976b; Behrman et al. 1977) have been analyzing a set of about 1,000 MZ and 900 dizygotic (DZ) twin pairs based on the National Academy of Sciences-National Research Council (NAS-NRC) Twin Registry sample of white male army veterans. Behrman and Taubman's estimates stand as the only ones of their kind until Ashenfelter and Krueger's (1994) study of participants in the 1991 Annual Twins Day Festival in Twinsburg, Ohio. Ashenfelter and Rouse's study (1998) incorporates data from the 1992 and 1993 Twinsburg festivals, and Rouse (1999) further adds data from the 1995 festival. The Twinsburg studies have sparked interest in analyzing still other data on MZ twins. Behrman and Rosenzweig use data from the Minnesota Twin Registry. In addition to these studies based on U.S. samples, Miller et al. (1995) use the Australian Twin Register, Isacsson (1999) uses the Swedish Twin Registry, and Bonjour et al. (2003) use the London Twins Research Unit. Table 2 summarizes these studies of estimated returns to schooling based on samples of twins.

Concentrating on what they have to say about the income-schooling relationship, one can divide them roughly into two groups: those who find only minor biases in the estimated returns to schooling due to omitted family background (Gorseline, Chamberlain and Griliches, and Jencks and some of his associates), and those who find that family background accounts for a major portion of the observed income-schooling relationship and is a major source of income inequality over time (Brittain, Olneck, Taubman and his associates, Ashenfelter and his associates). This division is due in part to the way the question is phrased and in part to differences in methodology, but mostly to the fact that different samples appear to be telling different stories.

Jencks and his associates report (Corcoran et al. 1976; Jencks and Brown 1977) results for 99 pairs of brothers culled from the 11-year follow-up of the Talent Survey. The dependent variable is the logarithm of the wage rate at approximately age 28. The results are similar to those reported for NLS brothers--almost no decline in the schooling coefficient as one goes from total to within-family estimates, except that the estimated effect of IQ is higher. Since the sample is very small and heavily selected, it is probably not worth spending too much time on. Jencks and his associates also report the results for 150 pairs of brothers collected by the NORC (earnings as of 1973). No test scores were available. The schooling coefficients are rather high and do not decline when estimated from differences between brothers. In summarizing their work on both samples (Talent and NORC), Jencks et al. conclude that the unobservable components that are common to the earnings of brothers have little to do with measured parental characteristics and are only weakly related to the unobservable family components in test scores and in schooling.

Chamberlain and Griliches (1975) reanalyzed the Gorseline (1932) data from the late 1920s on 156 pairs of brothers in the state of Indiana. There was very little difference between the total and within-family estimates of returns to schooling. Since the data base contained no direct measures of 'ability' (e.g., IQ scores), an attempt was made to use reported occupation (scaled by the log average income in these occupations) as another indicator, and a maximum-likelihood procedure was developed to estimate the parameters of a model in which 'ability' is unobservable with both family and individual components of variance. This model is underidentified but yields bounds on β when other coefficients are constrained to lie within a reasonable range. Such bounds indicate that in these data there is little 'ability' bias in the usual estimates of β. Chamberlain and Griliches (1977) analyzed a more recent and more representative set of data on 292 pairs of brothers culled from the 1969 National Longitudinal Survey of Young Men, containing data on two types of test scores: IQ and a test of `Knowledge of the World of Work. A similar two-unobservable-factors model was estimated using both test scores as indicators of unmeasured ability. The results indicate little bias from the omission of such unobservable variables (maximum likelihood estimator [MLE] β=.064, versus an OLS β of .074 at the individual level). The implied unobservable factor which loads positively on the test scores and schooling appears to have no significant effect on expected occupational earnings net of expected schooling, while the unobservable that is correlated significantly with both expected schooling and earnings has opposite signs in the two equations. These results can be criticized on at least three grounds: (1) they use a rather dubious second indictor equation, occupational success, to identify the parameters of interest. It is not clear, however whether one can really treat income and occupation as two different measures of success. (2) Given the expectational nature of the data on schooling and income, it is not clear whether one can maintain the no-correlation-between-disturbances assumption even after the introduction of the two unobservable factors. A two-factor rational expectations type model implies the estimation of β from a smoothed between-families variance matrix. This yields a β of 0.061 with no significant change in the interpretation of the rest of the model. (3)

The question may be raised whether actual data are anything like the expectations. To check on this, Chamberlain and Griliches estimated their model also for 161 pairs of out-of-school brothers with good wage, IQ, and work-experience data as of 1970. The full two-factor model did not converge, but a 'one-and-a-half' factor model (second factor purely familiar with no direct effect on income) again implied that the unobservable factor that is positively connected to test scores and schooling has no significant independent effect in the wage-rate equation (net of schooling and work experience). In this data set, too, there seems to be little bias from family-type effects, and whatever bias there may be does not seem to arise from the omission of IQ-type variables. (4) The youngness of this sample may be the reason why IQ-type variables seem to matter so little. There is some evidence that the importance of IQ in wage determination increases with age (Hauser and Daymont 1977). Griliches and Stoker have been following the NLS brothers as they age. As of 1973 there were 247 pairs of brothers out of school with good wage data. In this sample there was again no significant change in the estimate of the schooling coefficient then going from individual to within-family data.

Brittain's (1977) study of 60 or so pairs of Cleveland, Ohio, brothers is based on a sample of 659 decedent estates closed in 1964-65 and subsequent interviews with the heirs of these estates. The sample is the smallest and the most peculiar of all those reported here. The correlation of income between brothers exceeds that reported for all other sibling sets except the Taubman-NRC MZ twins.

Olneck (1977) collected and analyzed data on 346 pairs of brothers from

Kalamazoo, Michigan, who were between 35 and 59 years old in 1973. Sixth-grade test scores were collected from school records, and data on current and past occupation, current earnings, and educational attainment were collected by phone interview. His estimate of the schooling coefficient is somewhat lower than in other studies, and its decline when estimated from the within data was somewhat larger than usual but not out of line with the results of other studies. The big difference occurs in the importance that is assigned to the IQ variable. Once the within estimates are computed, including IQ, its coefficient goes up and the schooling coefficient drops significantly, to about less than half of its original value. Olneck concludes that the combined ability-background bias in the income-schooling relationship is quite large. His results with respect to the IQ variable are not consistent, however, with the model outlined above or with most of the other models used in this area.

Taubman and his associates report (Behrman and Taubman 1976; Taubman 1976a, 1976b; Behrman et al. 1977) on a large study of U.S. white male veteran twins, age 46-56 in 1973, based on 1,022 pairs of MZ and 914 pairs of DZ twins. The emphasis in their studies is on estimating 'genetic' versus 'environmental' sources of variation in education, occupation, and earnings. In their model the right estimate of β is to be had from the within-twin-pair variance-covariance matrix. The resulting estimate of 0.027 is the lowest estimate in the whole of table of within-twin studies and also quite low relative to their estimate of the schooling coefficient at the individual level (0.077 for MZ and 0.080 for DZ twins). Their model also attributes about 45 percent of the observed variance in earnings to 'genetics', 12 percent to other family-environment sources, and the rest (43 percent) to individual differences.

Ashenfelter, Rouse and various colleagues (Ashenfelter and Krueger 1994; Ashenfelter and Rouse 1998) estimated the returns to schooling with a sample of the Annual Twins Days Festival in Twinsburg, Ohio, from 1991 to 1995. Their sample consists of 453 pairs of twins both of whom have held a job at some point in the previous two years and are not currently living outside of the United States. They contrast the wages of genetically identical twins with different schooling levels to investigate the contribution of genetic ability to the observed cross-sectional returns to schooling. They find that the within-twin regression estimate of the effect of schooling on the log wage is smaller than the cross-sectional estimate, implying a small upward bias in the cross-sectional estimate. There is also evidence of an important individual-specific component to the measurement error in schooling reports.

Table 2 lists the estimated coefficients of years of schooling in each of the above-described within-twin-pair studies. These estimates come from regressions of the wage measures listed in the table's third column on years of schooling and, in some cases, a few control variables. In particular, except as indicated in the table's footnotes, the OLS and Generalized Least Squares (GLS) estimates shown in the fourth and fifth columns come from regressions that control for age, gender (in those studies that include women as well as men), and race (in the U.S. studies that include nonwhites). The sixth column, labeled $\hat{\beta}_{fe}$ for 'covariance estimator', shows the results from applying least squares to the regression of the within-twin-pair difference in the wage measure on the within-twin-pair difference in years of schooling. The age, gender, and race controls drop out of this differenced regression because MZ twins share the same values of these regressors. For this same differenced regression, the last column reports results from instrumental-variable estimation.

Of course, the main motivation for the twins-based literature is a concern that the OLS and GLS estimators are inconsistent because schooling is correlated with factors-such as intelligence, motivation, and family background- that contribute to the error term in the wage equation. If these factors differed only between families and not at all between MZ twins, and if MZ twins nevertheless differed in their schooling for reasons completely unrelated to the within-twin-pair difference in error terms, then the covariance estimator, which applies least squares to the regression of the within-twin-pair difference in wage measures on the within-twin-pair difference in schooling, would be a perfect solution to the endogeneity problem of the conventional cross-sectional estimators. The sixth column of the table shows each study's covariance estimator of the returns to schooling. In Taubman's study, $\hat{\beta}_{fe}$=0.027 came out much smaller than $\hat{\beta}_{ols} = 0.079$. He therefore concluded that `it is very important to control for genetics and family environment when studying the effects of schooling on earnings' and that failing to do so 'may cause a large bias, up to two-thirds of the non controlled coefficient.' During the nearly two decades these results were the only ones available for a sample of MZ twins, it was presumed that the within-twin-pair variation told a very different story than the between-families variation. It therefore came as quite a surprise when Ashenfelter and Krueger reported that their $\widehat{\beta}_{fe}$=0.092 exceeded their $\widehat{\beta}_{ols}$=0.084. Based partly on this comparison, Ashenfelter and Krueger reached the provocative conclusions that `unobserved factors do not cause an upward bias in simple estimates of the economic returns to schooling' and that 'the economic returns to schooling may have been underestimated in the past.' However, Ashenfelter and Krueger's original results were at least partly an artifact of an odd sample. When additional waves of the Twinsburg survey were added in the Ashenfelter-Rouse and Rouse studies, the old result $\widehat{\beta}_{fe} < \widehat{\beta}_{ols}$ was restored. This result also was replicated in the Miller et al., Isacsson, and Bonjour et al. studies.

2.1 Problems about Using Sibling Samples

Twins samples' advantage of having the same intelligence, motivation, family background and so forth is the corner stone of all twins studies, but they are not flawless. There are several problems that plague sibling samples, and therefore twins samples, in general which should be mentioned here. (1) Most of the sibling samples are 'opportunity' samples. The data had been collected for other purposes and are usually quite unrepresentative of the population at large (e.g., men in Indiana, Kalamazoo, or Cleveland; white army veterans etc.). (2) Brothers' as such may not be fully representative of the population at large. For example, they exclude only children and mixed-sex pairs. (3) There is also a serious sample-selection problem. Missing data on one brother tend to eliminate the whole pair from the sample. To the extent that data are not missing entirely at random, it is quite likely that more of the discordant pairs are missing, overestimating the resemblance of brothers. Similarly, data are eliminated for nonresponse on the income or earnings question. To the extent that more low-income people are eliminated, this is likely to lead to an underestimate of the schooling coefficient. Methods are now available for tackling such problems explicitly (Heckman 1976; Griliches et al. 1978; Tunali 1986), and we will apply them in later section.

2.2 Difficulties with the Within-twin-pair Studies

Difficulties with the within-twin-pair studies also follow. (1). It was shown by Chamberlain (1977a, 1977b) that data on twins do not provide any more identifying restrictions than data on brothers, unless one makes very special additional assumptions. A similar point is also made by Goldberger (1977). Behrman et al. assume that the nongenetic factors are purely familial and that the role of 'environment' is the same for DZ and MZ twins. A basic asymmetry is postulated; MZ twins are assumed to be much 'closer' to each other as far as genes are concerned, but this is not reflected in any greater environmental closeness or interaction. This does not seem to be a very attractive assumption, and there is some evidence against it (Jencks and Brown 1977). One would assume that parents and society treat children more alike the closer they are to each other in time, space, character, and appearance. The failure of this assumption removes the special identifying power attributed to the within-twin data. (2). As has been shown earlier (Bishop 1974; Griliches 1979), twin data, because of the high intraclass correlation in schooling, are especially susceptible to errors-of-measurement problems. A relatively modest adjustment for such errors can account for most of their results. (3). As in most such studies, no allowance is made for the possible simultaneity between schooling and earnings. (4). The lack of test scores for most of their samples and the lack of a more explicit model of the sources of familial resemblance in the success patterns of twins make it difficult to interpret the latent variables that emerge and draw any clear policy conclusions from them. (This is not just a criticism of within-twin studies. It applies equally well to other studies which concentrate on the measurement of the 'role' of family background.)

To solve these problems, more specific models which take into account sample-selection, environmental interaction, simultaneity between schooling and earnings, and sources of familial resemblance are needed in the future.

3 Method

3.1 Omitted Variable Bias (Selection Effect)

3.1.1 OLS Model

Our study begins with OLS estimates as a way of replicating the conventional cross-sectional estimates. A general set up of recent applied work in the economics of schooling returns specifies wage rates as consisting of observable components and an unobservable component that varies by family, observable components that vary by individuals, and unobservable individual component. Following Ashenfelter and Krueger (1994), we suppose the wage of twin j in family i is determined by

$$(1)\ y_{1i} = \alpha X_i + \beta Z_{1i} + \mu_i + \varepsilon_{1i}$$
$$(2)\ y_{2i} = \alpha X_i + \beta Z_{2i} + \mu_i + \varepsilon_{2i}$$

where $y_{ji}\,(j=1,2)$ is the logarithms of the wage rates of the first and second twin in the pair. X_i is the set of observed variables that vary by family, but not across twins, which includes age, age-squared, gender, and city dummies. $Z_{ji}\,(j=1,2)$ is the set of variables that may vary across the twins. In our study these variables include the education levels, marital status and job tenure of each twin. μ_i represents a set of unobservable variables that also affect earnings, i.e., ability or family effect. And $\varepsilon_{ji}\,(j=1,2)$ is a disturbance, representing other not explicitly measured forces affecting earnings. It is assumed to be distributed independently of $Z_{ji}\,(j=1,2)$ and μ_i, and has zero mean and constant variance σ_i^2 conditional on $Z_{ji}\,(j=1,2)$ and μ_i.

As mentioned before, analysis of cross-sectional data alone can neither identify nor control for unobservable effects μ_i, and the regressed version of equations (1) and (2) becomes:

$$(3)\ \mathrm{y}_{\mathrm{ji}} = \alpha X_i + \beta Z_{ji} + \varepsilon_{ji},\, j=1,2$$

Least-squares estimates of β from equation (3) which ignore μ_i will be biased, picking up also some of the effects of μ_i and attributing them to Z_{ji}. The standard left-out variable bias formula gives the size of this bias as $\dfrac{\operatorname{cov}(Z_{ji},\mu_i)}{\operatorname{var}(Z_{ji})}$, which summarizes the relationship, in the sample, between the excluded μ_i and the included Z_{ji}.

3.1.2 Fixed-Effect Model

An approach to treat the bias and inconsistency of OLS estimate is to eliminate the unobservable ability and family effect μ_i by contrasting the wage rates of identical twins with different schooling levels. Our goal is to ensure that the correlation we observe between years of schooling and wage rates is not due to a correlation between schooling level and a worker's ability. We do this by taking advantage of the fact that MZ twins are genetically identical and have similar family backgrounds.

A within-twin-pair estimator of β for identical twins, β_{fe}, is based on first-difference of equations (1) and (2):

$$(4)\; y_{1i} - y_{2i} = \beta(Z_{1i} - Z_{2i}) + \varepsilon_{1i} - \varepsilon_{2i}$$

The family effect μ_i has been removed. Equation (4) can be fitted by least squares to get the Fixed-Effect estimator.

3.1.3 GLS Model

Ashenfelter and Krueger's (1994) another approach is based on an explicit expression of unobserved family effect μ_i. They assumed a general representation for the correlation between the unobserved family effect and the observables is:

$$(5)\; \mu_i = \gamma Z_{1i} + \gamma Z_{2i} + \delta X_i + \omega_i$$

where ω_i is uncorrelated with $Z_{ji}(j=1,2)$ or X_i. The coefficient γ measures the selection effect relating family earnings and schooling levels, while the schooling level element of coefficient β measures the rate of returns to schooling. For simplicity, contributions of schooling to family earnings are assumed to be identical across twins.

The reduced form for equations (1), (2) and (5) is obtained by substituting (5) into (1) and (2) and collecting terms:

$$(6)\ y_{1i} = (\alpha+\delta)X_i + (\beta+\gamma)Z_{1i} + \gamma Z_{2i} + \varepsilon_{1i}^{'}$$
$$(7)\ y_{2i} = (\alpha+\delta)X_i + (\beta+\gamma)Z_{2i} + \gamma Z_{1i} + \varepsilon_{2i}^{'}$$

where $\varepsilon_{ji}^{'} = \omega_i + \varepsilon_{ji} (j=1,2)$. Although equations (6) and (7) may be fitted by OLS, GLS is the optimal estimator for the equation because of the cross-equation restrictions on the coefficients. Both Fixed-Effect and GLS models control for ability, but GLS is better in that it also permits an assessment of selection effect γ. Regression results from both Fixed-Effect and GLS models will be displayed in section 5.

3.2 Measurement Error

Classical measurement error in schooling will lead to bias in the estimators of the effect of schooling on wage rates. If Z_{ji} is true schooling, and measured schooling is $z_{ji} = Z_{ji} + v_{ji}$ with $p\lim(Z_{ji}v_{ji}) = 0$, then the observed equation is:

$$(8)\ y_{ji} = \alpha X_i + \beta z_{ji} - \beta v_{ji} + \mu_i + \varepsilon_{ji}, (j=1,2)$$

The least squares regression coefficient in the presence of measurement error in schooling is attenuated by an amount equal to the reliability ratio; that is, plim $\hat{\beta}_{ols} = \beta_{ols}\left(1 - \frac{\mathrm{cov}(v,z)}{\mathrm{var}(z)}\right) + \frac{\mathrm{cov}(\mu,z)}{\mathrm{var}(z)} = \beta_{ols}\left(1 - \frac{\mathrm{var}(v)}{\mathrm{var}(z)}\right) + \frac{\mathrm{cov}(\mu,z)}{\mathrm{var}(z)}$, where β_{ols} is the population regression coefficient if schooling were perfectly measured. The Fixed-Effect estimator eliminates the omitted variable bias but it does so at the expense of introducing far greater measurement error bias. The plim of the

Fixed-Effect estimator, $\hat{\beta}_{fe}$, is:

$$\hat{\beta}_{fe} = \beta_{fe}\left(1 - \frac{\operatorname{cov}(\Delta\nu, \Delta z)}{\operatorname{var}(\Delta z)}\right) + \frac{\operatorname{cov}(\Delta\mu, \Delta z)}{\operatorname{var}(\Delta z)} = \beta_{fe}\left(1 - \frac{\operatorname{var}(\Delta\nu)}{\operatorname{var}(\Delta z)}\right) + \frac{\operatorname{cov}(\Delta\mu, \Delta z)}{\operatorname{var}(\Delta z)},$$

where β_{fe} is the population Fixed-Effect estimator that would be obtained in the absence of measurement error.

A straightforward consistent estimator for equations (6) and (7) or (4) may be obtained by the method of instrumental variables using the independent measure of the schooling variables as instruments. We have followed Ashenfelter and Krueger's (1994) innovation to ask each twin his/her own and his/her co-twin's education. In the survey, we asked each twin we interviewed to report on her/his own schooling level and on her/his sibling's. If self-reported education is measured with error this provides a potential instrument since the report of the other twin should be correlated with the self-reported education level but uncorrelated with the equation regressed. Writing $Z_{ji}^{\ k}$ for twin *k*'s report on twin *j*'s schooling implies that there are two different ways to use the auxiliary schooling information as an instrumental variable. There are four different ways to estimate the schooling difference ΔZ:

$$(9)\ \Delta Z' = Z_1^{\ 1} - Z_2^{\ 2}$$
$$(10)\ \Delta Z'' = Z_1^{\ 2} - Z_2^{\ 1}$$
$$(11)\ \Delta Z^{*} = Z_1^{\ 1} - Z_2^{\ 1}$$
$$(12)\ \Delta Z^{**} = Z_1^{\ 2} - Z_2^{\ 2}$$

$\Delta Z''$ will be uncorrelated with $\Delta Z'$ even if there is a family effect in the measurement error because the family effect is subtracted from both $\Delta Z''$ and $\Delta Z'$. However, $\Delta Z''$ and $\Delta Z'$ will be correlated if there is a person-specific component of the measurement error. To eliminate the person-specific component of the measurement error, it is sufficient to estimate the schooling differences using the definitions in equations (11) and (12), which amounts to calculating the schooling difference reported by each sibling and using one as an instrument for the other.

For example, we may fit

$$(13)\, \Delta y_i = \beta \Delta Z'_i + \Delta \varepsilon_i$$

using $\Delta Z''$ as an instrument for $\Delta Z'$. But in the presence of correlated measurement errors the instrumental-variables estimators of equation (4) will be inconsistent: $\hat{\beta}_{feIV} = \dfrac{\beta}{1 - s\rho_v \dfrac{\mathrm{var}(v)}{\mathrm{var}(\Delta Z)}}$, where ρ_v is the correlation between the measurement error of the twins. A straightforward consistent estimator of equation (4) may be obtained by instrumental-variables estimation of

$$(14)\, \Delta y_i = \beta \Delta Z^*_i + \Delta \varepsilon_i$$

in which ΔZ^{**}_i is used as an instrument for ΔZ^*_i. Note that ΔZ^*_i here differs from $(Z_{1i} - Z_{2i})$ in equation (4).

4 Data

The data we will use are derived from a survey carried out by the Urban Survey Unit of the National Bureau of Statistics during June and July, 2002, in five cities of China. Adult twins aged from 18 to 65 were identified by the local statistical bureau through various channels, including colleagues, friends, relatives, newspaper advertising, neighborhood notices, neighborhood management committees, and household records in the public security bureau. Overall, these channels are more or less equal in probability for all twins in a city, and in this sense, the twins sample obtained may be rather random. The survey was conducted with extraordinary care, including several site checks by experts from the National Bureau of Statistics and Junsen Zhang. Questionnaires were completed through household face-to-face personal interviews. With appropriate discussion with Mark Rosenzweig and other experts, data input was closely supervised and monitored by Junsen Zhang during the months of July and August, 2002.

In this data set, there is household economic information for respondents in 5 cities including Chengdu, Chongqing, Haerbin, Hefei and Wuhan. Altogether there are 4683 observations, in which 3012 observations are from twins households (i.e. 1510 households with twins). Figure 1 shows the distribution of the households from different cities.

Within twins, we can distinguish them between identical twins and non-identical twins. We consider a pair of twins identical if both twins respond that they have identical hair color, look, gender and age. We have completed questionnaires from 3,002 individuals, among which 2,996 are twin individuals aged 17 to 62, 6 are triplet individuals. In total, we have 914 complete pairs of identical twins, i.e., 1,828 individuals. For 488 of these pairs (976 individuals) we have complete wage information on both twins in the pair.

Table 4 sets out some descriptive statistics for our data along with comparative data from the non twins sample as a check on the representativeness of our sample. Column (1) sets out statistics for all identical twins. They average 12.22 years of schooling, are aged 34.78, and 66 percent are married. They earn, on average, RMB 887.85 per month, have worked for 15.03 years and 22 percent are China's Communist Party members. We define 'Tenure' as the number of years the interviewee has been working full time, starting from the age of sixteen. Foreign venture type is a dummy variable, which controls venture type of employment unit. Foreign venture type is 1, if the type of employment unit is either joint venture with foreign companies, or corporative venture with foreign companies, or foreign owned venture. If it is either state owned unit, or collective unit, or private business, then foreign venture type is 0. As more and more foreign investments making their way into China, foreign invested enterprises have been playing an influential power in China's social and economic life. Their impact on Chinese income may be important, with a payment system much more rewarding than Chinese traditional companies. Smoking is also a dummy variable which distinguishes whether the interviewee is a frequent smoker. Column (2) sets out data for non twins. They have 11.73 years of schooling, are aged 43.27, and 94 percent are married. Since our twins are slightly younger, they have less years of schooling, earn more income, and immature on personal covariates, such as tenure and Party member status, but larger on foreign venture type and spouse education. Their parents also appear to be more educated than non twins. Generally speaking, our data do not seem to be too far from the average for non twins.

5 Results

5.1 OLS, Fixed-Effect, GLS and IV estimates

Table 5 sets out our estimates. In columns (1), (2) and (3), we report the results of stacking equations (1) and (2) and fitting them by OLS. Column (1) and (2) show OLS regressions using both twins and non twins, entering schooling, age, age squared, gender, married status and tenure. The returns to education are quite precisely estimated at 6.7 percent. The rest of the columns are estimates for twins. Column (3) is an OLS pooled regression using all identical twins for whom we have complete wage information, 976 individuals, and schooling, age, age squared and gender as regressors. This gives returns to education of 8.2 percent, higher than the figure in column (1). Remember we have younger MZ twins than non twins, this may indicate that people's income level has been rising lately in China. .

A regression of the within-twin-pair difference in wage rates on the within-twin-pair difference in schooling levels (which is the Fixed-Effect estimate) is reported in column (4) of Table 5, which estimates the within-twin-pair equation (4). Since the pooled estimates do not control for ability bias, we would expect the within-twin-pair estimates to be less. As column (4) shows, the returns are indeed much less, at 2.5 percent. This result confirms that the OLS regression estimate is larger, not smaller, than the within---twin-pair regression estimate. This is consistent with results reported by Behrman et al. (1980) and Rouse (1999). But Rouse's within-twin-pair regressions indicate schooling returns are around 7.5 percent, much larger than ours.

This figure might however also reflect downward bias due to exacerbated measurement error in the differenced equation. To check this columns (1) and (2) in Table 5.1 report the instrumental-variable estimates which are intended to correct for measurement error in the education data. Here we use each sibling's report of his/her sibling's education level as an instrumental variable for his/her sibling's education level. Column (1) maintains a pooled specification, while column (2) uses within-twin-pair difference. As expected, measurement error in reported education does bias down the returns estimates in column (3) and column (4) of Table 5. As column (1) and (2) of Table 5.1 show, returns rise to 8.7 percent and 3.2 percent when this is done. Therefore, instrumental-variable estimate in Fixed-Effect model is larger than the least-squares estimate in Fixed-Effect model, and it is consistent with our finding above that a considerable fraction of the variability in reported differences in twins' education levels is due to measurement error. If we accept the sibling report as a valid instrument, it seems likely that conventional methods are producing serious underestimates of the economic returns to schooling.

The rest columns of Table 5 and 5.1 repeat the exercise controlling for years of job tenure and marital status. The pattern of point estimates on the regressors is similar. As before, measurement error biases returns down (OLS returns are less than IV returns), and the within-twin-pair IV estimates are lower than the pooled IV estimates suggesting positive ability bias. Wage rates are concave in age.

Table 6, 6.1, 7 and 7.1 repeat the same regressions as in Table 5 and 5.1 using only male twins and female twins. With our smaller sample size of females compared to males, the magnitude of ability bias resulting from columns (1) and (2) in Table 6.1

and 7.1 is 6.4 percent for males and 3.3 percent for females; the magnitude of measurement error resulting from column (4) in Table 6 and column (2) in Table 6.1 is 0.1 percent for males, but 3.0 percent for females from Table 7 and 7.1. Therefore, females in our sample tend to have a weaker correlation between education and ability, while report education with greater measurement error. Also, males earn more than females and the positive effect of years of job tenure increases females' wage rates more than males'.

Table 8 contains the same simple and augmented estimates of the effect of schooling on earnings as in Table 5, only here we fit models by GLS. The left panel controls only for demographic variables (that may be considered strictly exogenous). In column (1) we report the results of stacking equations (1) and (2) and fitting them by the seemingly unrelated regression method (S.U.R.E) due to Zellner (1962). The results in columns (1) are comparable to most of the estimates that have appeared in the literature which ignore the potential correlation between schooling level and family background. The results in column (2) of Table 8 correspond to stacking equations (6) and (7) and fitting them by S.U.R.E. These are the results that include the sibling's education level in each twin's wage equation. Coefficient of this variable is a measure of the selection effect, γ, in equation (5). As the table indicates, this effect is positive, indicating that the selection effect in these data is positive. In this sample the better-educated families are those who would be more highly compensated in the labor market. This result also implies that a regression estimator of the returns to schooling that does not adjust for the selection effect will be upward-biased.

The right panel of Table 8 contains an analysis that parallels the analysis in the left panel except that variables measuring marital status and years of job tenure have been added to the regressions. Many of the results in Table 8 are similar to those that have been reported elsewhere in the study of the determination of wage rates. Table 9 and 10 repeat the same regressions as in Table 8 using only male twins and female twins. Again, males earn more than females, with much larger constants in Table 8.

Table 11 reports the correlations among the (logarithmic) wages, (self-reported and sibling-reported) education levels, and father's and mother's education levels for our sample of twins. In all our analyses we have randomly selected one twin as the first in each pair. We write $Z_1^{\ 1}$ for the self-reported education level of the first twin, $Z_1^{\ 2}$ for the sibling-reported education level of the first twin, $Z_2^{\ 2}$ for the self-reported education level of the second twin, and $Z_2^{\ 1}$ for the sibling-reported education level of the second twin. (That is, $Z_n^{\ m}$, *n, m=1,2* refers to the education level of the *n*th twin as reported by the *m*th twin.) All six of the possible correlations are reported in the table. It is apparent that the independent measures of education levels are highly correlated. There are, of course, two measures of the father's and mother's education levels, and we have reported the correlations across both of these also. It is apparent from the table that the wage rates and education levels of identical twins are highly correlated.

It is possible to compare some of the correlations in Table 11 with other reports of sibling correlations. For identical twins, Ashenfelter and Krueger (1994) report intrapair correlations of 0.66 for years of schooling and 0.56 for (the logarithm of) earnings. These may be contrasted with our estimates of intrapair correlations for identical twins of 0.76 for self-reported schooling and 0.51 for (the logarithm of) wages rates. But our self-reported and sibling-reported education of the same twin is not so correlated as in Ashenfelter and Krueger (1994). The $\mathrm{cov}(Z_1^{\,1}, Z_1^{\,2})$ and $\mathrm{cov}(Z_2^{\,2}, Z_2^{\,1})$ in our sample are 0.743 and 0.723, while the same correlations in Ashenfelter and Krueger (1994) are 0.920 and 0.877. In other words, sibling-reported education is not so good an IV for self-reported education in our sample as in Ashenfelter and Krueger's (1994). Another thing is that our self-reported education and self-reported sibling's education are more correlated than Ashenfelter and Krueger (1994), which means our sample suffer from the correlated measurement errors problem even worse. To be specific, our $\mathrm{cov}(Z_1^{\,1}, Z_2^{\,1})$ and $\mathrm{cov}(Z_2^{\,2}, Z_1^{\,2})$ are 0.933 and 0.925, while the same correlations in Ashenfelter and Krueger (1994) are only 0.700 and 0.697. Thus, we implement an instrumental-variables approach that is consistent even in the presence of correlated measurement errors. Specifically, we include $\Delta Z^{*} = Z_1^{\,1} - Z_2^{\,1}$ in the first-differenced wage equations, and use $\Delta Z^{**} = Z_1^{\,2} - Z_2^{\,2}$ as an instrument for ΔZ^{*}. These instrumental-variables first-difference estimates, along with least-squares first-difference estimates, are reported in Table 12. When no other covariates are included, the instrumental-variable estimate that is robust to correlated measurement errors is 3.6 percent, which is almost 50 percent greater than the OLS estimate of 1.9 percent. Compared with column (2) in Table 5.1, the IV estimate in column (2) is much more significant, which validates the assumption of correlated measurement errors. Therefore, for our sample, conclusions about Fixed-Effect estimators should be mainly based on Table 12 column (2) and column (4), instead of Table 5.1 column (2) and column (4). Similar results hold when other variables are added to the regression. Table 13 and 14 repeat the same regressions as in Table 12 using only male twins and female twins. The pattern of point estimates remains the same.

5.2 Important findings

The results are comparable to most of the estimates that have appeared in the literature. However, compared with former researchers' results using developed countries' data, our Chinese twins sample still generates some differences. First, it seems to have much lower returns to schooling, 3.6 percent. For example, as Table 2 shows, Ashenfelter and Rouse's (1998) regressions fitted on data from the Twinsburg Twins Festival in U.S. with an identical specification as that in Table 5 give estimates of the effect of schooling on the wage of 8.8 percent per year completed, compared to 11.9 percent in Rouse (1999). These relative lower earnings returns to education in China may be attributed to the absence of markets in the past (Whyte and Parish 1984; Walder 1990; Peng 1992; Xie and Hannum 1996; Zhao and Zhou 2002). Scholars have long observed that before reform, economic resources in China were allocated primarily according to bureaucratic principles under redistributive economies, in which political loyalty rather than economic productivity was the basis of reward (Polanyi 1957; Szelenyi 1978, 1983). Second, the magnitude of ability bias could be generated by comparing the pooled IV and the within-twin-pair IV estimates as both controls for measurement error. Our ability bias 5.1 percent resulting from columns (1) and (2) in Table 5.1 far exceeds Rouse's (1999) result of 3 percent and Bonjour et al.'s (2003) report of 0.8 percent, which could be explained by a more intense correlation between people's unobserved ability and schooling level in China. As is known to all, developing countries' citizens usually do not have as much access to education as their developed counterparts. Therefore, the fierce competition for education opportunity is natural. As a result, in China only those who have distinct ability could get the chance to receive higher education. Third, comparison of the column (4) in Table 5 and (2) in Table 5.1 provides an estimate of the magnitude of measurement error as both control for ability bias. This magnitude is 1.1 percent in our sample, while Rouse (1999) reports 7.5 percent and Bonjour et al. (2003) report 3.8 percent. Therefore, our interviewees tend to report their education more accurately.

5.3 Further Results

5.3.1 Consistency of Fixed-Effect Estimate

There are two issues that arise with the within-twin-pair method. The first question is whether β_{fe} is less biased than β_{ols}, and therefore a better estimate. This question is also the major criticism of within-twin-pair estimates set out by Bound and Solon (1999) and Neumark (1999), building on earlier work by Griliches (1979). Bound and Solon (1999) examined the implications of endogenous determination of which twin goes to school longer, and concluded that twins-based estimation is vulnerable to the same sort of inconsistency that afflicts conventional cross-sectional estimation. They argued that while within-twin-pair differencing removes genetic variation, differences might still reflect ability bias to the extent that ability is affected by more than just genes. In other words, within-twin-pair estimation does not eliminate the inconsistency of the conventional cross-sectional estimator and can even aggravate it. The intuition is that, even though differencing between twins does difference out much of the endogenous variation in schooling, it does not eliminate it, and it also filters out much of the exogenous variation. If endogenous variation comprises as large a proportion of the remaining within-twin-pair variation as it does of the cross-sectional variation, then within-twin-pair estimation is subject to as large an endogeneity inconsistency as the cross-sectional estimator.

The key point is that even MZ twins are a little different, and their (often small) differences in abilities and temperament may contribute to their (often small) differences in earnings. What this implies is the inconsistency of within-twin-pair estimation of the returns to schooling depends on the extent to which the within-twin-pair differences that generate their schooling differences also contribute in other ways to their wage differences. As far as we can tell, there is no a priori basis for answering that question. It therefore is unclear whether endogenous variation comprises a small share of the within-twin-pair schooling variation than it does of the between-families variation, which means it is uncertain whether the covariance estimator based on within-twin-pair variation is subject to less inconsistency than the conventional OLS estimator.

So what, if anything, can be learned from twins-based estimates of the returns to schooling? One optimistic answer would be that, if endogeneity of schooling were the only problem with estimating the wage-schooling regression, and if we were confident that the schooling and the wage error term are positively correlated both in the cross-section and within-twin-pair regression, then both the OLS estimator and the fixed-effect estimator would be upward inconsistent. If one starts with the presumption that endogenous schooling induces upward inconsistency in the estimated returns to schooling, the new twins-based estimates may complement other approaches to tightening the upper bound on the returns to schooling. Ashenfelter and Rouse (1998), Bound and Solon (1999), and Neumark (1999), following earlier arguments due to Griliches (1979), debate this at length in recent papers. Using the framework of equations (1) and (2), we have the following OLS estimator: $E\beta_{ols} = \beta + \frac{\mathrm{cov}(Z,\mu)}{\mathrm{var}(Z)}$. Therefore, conventional OLS ability bias to β depends on the fraction of variance in schooling that is accounted for by variance in unobserved abilities that might also affect wages. Similarly, ability bias to β_{fe} depends on the fraction of within-twin-pair variance in schooling that is accounted for by within-twin-pair variance in unobserved abilities that also affect wages: $E\beta_{fe} = \beta + \frac{\mathrm{cov}(\Delta Z,\Delta\mu)}{\mathrm{var}(\Delta Z)}$. If the endogenous variation within family is smaller than the endogenous variation between families, then β_{fe} is less biased than β_{ols}. Hence even if there is ability bias in within-twin-pair regressions, β_{fe} might still be regarded as an upper bound on the returns to education (if schooling and ability are positively correlated). In that case, we could credit the twins-based literature for having tightened the upper bound on the returns to schooling. From this point of view, comparing MZ twins serves the purpose for reducing the endogeneity inconsistency in estimation of the returns to schooling.

There is no a prior reason to believe that β_{fe} is less biased than β_{ols}. Ultimately the matter is of course an empirical one. Its investigation is subject to the central problem that ability is not observed. To examine this, Ashenfelter and Rouse (1998) calculate the correlation of average family education over each twin pair with those average family characteristics that might plausibly be correlated with ability (e.g., employment status, tenure, and spouse's education). This indicates expected ability bias in a pooled regression. They then calculate the correlation of within-twin-pair differences in education with within-twin-pair differences in characteristics. This indicates expected ability bias in a within-twin-pair regression. Using a range of variables, they find significant correlations in the pooled case, but no significant correlation in the within-twin-pair case. This suggests that ability bias in pooled regressions is likely to be higher than that using within-twin-pair regressions, and that most of the variation in ability is between families and not between twins within a family. In this thesis, we follow and extend Ashenfelter and Rouse (1998), and get similar conclusions to theirs.

Table 15 shows results of these correlation analyses using our sample. Consider the first column, first row. This shows that the correlation between average family education and average family venture is 0.09. It suggests that families with low average foreign venture type have low average schooling; consistent with ability and family background affecting schooling choice. The second column shows a smaller correlation between differences in education within twin pair and differences in venture within twin pair. To the extent that venture measures ability, within-twin-pair differences in education are less affected by ability bias than the between-family education differences. The rest of the first column shows other family correlations. This shows strong correlations between average family education and average family marital status, Party member status, spouse education, smoking behavior and job tenure. The second column shows the correlations between within-twin-pair differences in education and within-twin-pair differences in other characteristics. All of them are not as large as in the first column. In sum, within-twin-pair education differences are uncorrelated with any other within-twin difference in observables. Of course, these characteristics are incomplete measures of ability, but the evidence is suggestive, especially as it mirrors that found by Ashenfelter and Rouse (1998) and Bonjour et. al (2003).

5.3.2 Smoking as an Instrument for Education

A strength of our data is that we have information on the smoking behavior of the twins. Smoking has been suggested as an instrument for education, since it might proxy discount rates (Fuchs, 1986), and subsequently been used by Evans and Montgomery (1994) for the United States and Chevalier and Walker (1999) for the United Kingdom. This was criticized by Hamermesh (2000) who suggests that a youth's smoking behavior is a measure of family background and thus not a valid instrument for education.

Evans and Montgomery (1994) show that smoking is highly correlated with educational outcomes and uses it as an instrument in estimating returns to education. Their IV estimate of the returns to education lies about 10 percent above the OLS estimate. This would indicate negative ability bias, unlike twins studies where ability bias is small or positive. Evans and Montgomery present indirect evidence that the correlation of smoking and educational attainments is due to differences in time preferences. However, they acknowledge that there is no possibility to test this directly against the alternative hypothesis that the observed correlation is due to unobserved 'ability' in a very broad sense including genes, family, and social background as well as peers.

While not able to perform a direct test, our twin data allow us to advance indirect evidence which relies on the correlation method used before. A significant negative correlation between average family smoking and average family education is consistent with either smoking reflecting individual's discount rates or family background. However, if smoking reflects individual's discount rates, differences in smoking between twins should be correlated with within-twin-pair differences in education. But the within-twin-pair correlation should be insignificant if the cross-sectional correlation between smoking and education is due to family background.

Table 15 lower panel shows the correlation results for smoking. There is a strong negative correlation between average family smoking and average family education.

However, there is no strong correlation between within-twin-pair smoking and within-twin-pair education. As discussed earlier, this suggests smoking is more likely to reflect family background than discount rates. In sum, evidence seems to suggest that smoking reflects family background rather than discount rates.

5.3.3 Symmetry Test

The second question for Fixed-Effect estimator is whether returns to education are symmetric within twins, $\beta_{1i} = \beta_{2i}$ (Hertz 2003). This equality may be tested empirically in OLS estimate of equations (1) and (2), or (6) and (7). If the test is not rejected then we have no evidence that is inconsistent with the proposition that the mean returns to education may be identified by the standard Fixed-Effect equation. However, if we reject the assumption, we may prefer the weaker claim: that the Fixed-Effect estimates serve as an upper bound on the true returns to schooling. And we should be wary of ignoring the information contained in the subscripts--we should not treat each pair of twins interchangeable.

Following Card (1999), Hertz (2003) assumes a similar model to solve this problem:

$$(15)\ y_{ji} = \alpha_j + \beta_j Z_{ji}$$
$$(16)\ \alpha_j = \overline{\alpha}_j + \lambda_{j1}\left(Z_{1i} - \overline{Z}_{1i}\right) + \lambda_{j2}\left(Z_{2i} - \overline{Z}_{2i}\right) + u_j, (j = 1,2)$$
$$(17)\ \beta_j = \overline{\beta}_j + \varphi_{j1}\left(Z_{1i} - \overline{Z}_{1i}\right) + \varphi_{j2}\left(Z_{2i} - \overline{Z}_{2i}\right) + v_j$$

where $\overline{\alpha}_j, \overline{\beta}_j, \overline{Z}_{ji}$ are expected values across families for all *j*th members. Combining (15)-(17) and taking the linear projection of the result onto Z_{1i}, Z_{2i} (gathering all constant terms into f_1, f_2) yields the following:

$$(18)\; y_{1i} = f_1 + \left(\overline{\beta}_1 + \lambda_{11} + \varphi_{11}\overline{Z}_1\right)Z_{1i} + \left(\lambda_{12} + \varphi_{12}\overline{Z}_1\right)Z_{2i} + e_{1i}$$
$$= f_1 + \tau_{11}Z_{1i} + \tau_{12}Z_{2i} + e_{1i}$$
$$(19)\; y_{2i} = f_2 + \left(\overline{\beta}_2 + \lambda_{22} + \varphi_{22}\overline{Z}_2\right)Z_{2i} + \left(\lambda_{21} + \varphi_{21}\overline{Z}_2\right)Z_{1i} + e_{2i}$$
$$= f_2 + \tau_{22}Z_{2i} + \tau_{21}Z_{1i} + e_{2i}$$

These will be estimated jointly as seemingly unrelated regression equations (S.U.R.E.).

Using this model we will first test for the presence of the family effect, by testing the hypothesis that τ_{12} and τ_{21} are zero. Then we will test whether returns to education, including any family effects, are symmetric for twins. A necessary and sufficient condition for symmetry is that all of the following are true: $\overline{\beta}_1 = \overline{\beta}_2, \overline{Z}_1 = \overline{Z}_2, \lambda_{11} = \lambda_{22}, \lambda_{12} = \lambda_{21}$ and likewise for φ. These assumptions are also sufficient, but not necessary, to imply $\tau_{11} = \tau_{22}, \tau_{12} = \tau_{21}$. This latter pair of equalities may be tested empirically; if it is rejected we should be wary of ignoring the information contained in the subscripts--we should not treat twins as interchangeable. However, if the hypothesis is not rejected we cannot be sure that symmetry in fact holds.

There are two sets of identifying restrictions that allow us to generate separate estimates of $\overline{\beta}_1, \overline{\beta}_2$ and to test for their equality. First, we may impose a restriction which is called vertical uniformity, namely, $\lambda_{11} = \lambda_{21}, \lambda_{12} = \lambda_{22}$, and similarly for φ. If we also observe that $\overline{Z}_1 = \overline{Z}_2$ then we may retrieve estimates of both slope coefficients $\overline{\beta}_1 = \tau_{11} - \tau_{21}, \overline{\beta}_2 = \tau_{22} - \tau_{12}$ and then test whether these are equal, which amounts to a conditional specification test of the family Fixed-Effect model, since if $\overline{\beta}_1 = \overline{\beta}_2 = \overline{\beta}$ (and assuming vertical uniformity) we may subtract (19) from (18), to arrive at the within-family equation:

$$(20)\ y_{1i} - y_{2i} = (f_1 - f_2) + \overline{\beta}(Z_{1i} - Z_{2i}) + e$$

Note that if the Fixed-Effect model is correct, then it again makes no difference whether we assign a given person to the first or second position.

The second identifying restriction is to assume horizontal uniformity (a stronger assumption than Card's horizontal inequality), i.e., that $\lambda_{11} = \lambda_{12}, \lambda_{21} = \lambda_{22}$ and likewise for φ. This approach has been used by Ashenfelter and Zimmerman (1997) in the analysis of parent-child pairs. As before, this assumption allows us to estimate $\overline{\beta}_1, \overline{\beta}_2$ and provides an independent (albeit conditional) test of their equality.

Table 16 presents the S.U.R.E. model for the set of twin pairs, to test the symmetry assumption of Fixed-Effect model. The first panel reports the results of the equations (18) and (19) model. The estimates of the cross effects $\hat{\tau}_{12}, \hat{\tau}_{21}$ are large and $\hat{\tau}_{12}$ is significantly different from zero, confirming that family effects are nontrivial. Below these is the test of the hypothesis of symmetry in own and cross effects, which is not rejected at customary significance levels. The next panel assumes vertical uniformity, resulting in a precisely estimated schooling coefficient for sibling No.1 of 0.025; for sibling No.2 the figure is 0.027. The difference between the twin pair's coefficients is not clearly significant. Under the assumption of horizontal uniformity, the sibling No.1's coefficient decreases to 0.019 while the sibling No.2's rises to 0.029. Again, the hypothesis of equal returns for twin pair cannot be rejected. This is a pattern that will repeat itself: the horizontal estimates are less precise and lead to an implausible divergence in the results for twin pair.

5.3.4 Hausman Test

As is evident in Table 5 and Table 8, OLS estimates of returns to schooling reduce sharply when we control for ability by using first-differenced equation in Fixed-Effect model or by including co-twin's education in GLS model. However, we have no evidence whether the differences among OLS, Fixed-Effect and GLS are significant. Therefore, we use three Hausman tests for differences between the OLS coefficient in Table 5 column (3) and Fixed-Effect coefficient in Table 5 column (5), the OLS coefficient in Table 5 column (3) and GLS coefficient in Table 8 column (3), Fixed-Effect coefficient in Table 5 column (5) and GLS coefficient in Table 8 column (3), respectively.

Hausman (1978) has devised a test for specification error, and here we use it too. Under the null hypothesis, two different estimates should not differ systematically, and a test can be based on the difference. The essential ingredient for the test is the covariance matrix of the difference vector, [*b-B*]:

$$(21)\operatorname{var}(b-B)=\operatorname{var}(b)+\operatorname{var}(B)-\operatorname{cov}(b,B)-\operatorname{cov}(b,B)'$$

where b and B stand for estimates from two different specifications. Hausman's essential result is that the covariance of an estimator with its difference from the other estimator is zero. This implies that

$$(22)\operatorname{cov}[(b-B),B]=\operatorname{cov}(b,B)-\operatorname{var}(B)=0\text{, or that, }\operatorname{cov}(b,B)=\operatorname{var}(B).$$

Inserting (22) in (21) produces the required covariance matrix for the test,

$$(23)\operatorname{var}(b-B)=\operatorname{var}(b)-\operatorname{var}(B)=\sum$$

The chi-squared test is based on the Wald criterion:

$$(24)\quad W = \chi^2(K) = (b - B)' \hat{\Sigma}^{-1} (b - B)$$

For $\hat{\Sigma}$, we use the estimated covariance matrices of the slope estimators. K equals to 1 in our thesis.

The results are shown in Table 17. Differences between OLS and Fixed-Effect, OLS and GLS are very significant (at 1%), and the difference between Fixed-Effect and GLS is insignificant. Therefore, we reject the null of no difference between OLS and Fixed-Effect, OLS and GLS.

5.3.5 Selection Bias

As in all studies that are concerned with wages there is the potential of selection bias due to the participation decision. As we have mentioned in Section 2 Literature Review, data are eliminated for no response on the income or earnings question. To the extent that more low-income people are eliminated, this is likely to lead to an underestimate of the schooling coefficient.

How are the returns to education estimates affected by possible selection bias? If returns to education are linear in schooling, then having a sample of highly paid individuals should not matter for estimates. However, if there are diminishing marginal returns then, since we have a slightly above average paid group, our estimates would understate the 'average' marginal returns.

To solve this problem, we first consider the effects on the pooled estimates. We experiment on the pooled regressions with traditional Heckman-correction model, using married status, number of children and age in the participation equation. But we find no evidence that selection affected our estimates significantly in Table 18 column (5) and (6).

Then, we follow Tunali's (1986) work on double selection, and estimate a bivariate probit model for participation of twin pairs in the within-twin-pair estimation. For the *i*th family in our random sample, we have

$$(25)\ y_{1i} = \beta_1 Z_{1i} + \varepsilon_{1i}, \text{ first selection rule}$$

$$(26)\ y_{2i} = \beta_2 Z_{2i} + \varepsilon_{2i}, \text{ second selection rule}$$

$$(27)\ \Delta y = \beta \Delta Z_i + \Delta\varepsilon_i, \text{ regression equation}$$

where $(\varepsilon_{1i}, \varepsilon_{2i}, \Delta\varepsilon_i) \sim N(0, \Pi)$, $\Pi = \begin{bmatrix} 1 & \rho & \rho_{13} \\ \rho & 1 & \rho_{23} \\ \rho_{13} & \rho_{23} & 1 \end{bmatrix}$. Using the dichotomous variables D_1, D_2 to indicate the outcomes of the two selection rules, we classify the individuals in the original sample as follows:

$$(28)\ D_1 = \begin{cases} 1 & \text{if } \mathrm{y}_{1\mathrm{i}} > 0 \\ 0 & \text{if } \mathrm{y}_{1\mathrm{i}} \leq 0 \text{ or} = . \end{cases}$$

and

$$(29)\ D_2 = \begin{cases} 1 & \text{if } \mathrm{y}_{2\mathrm{i}} > 0 \\ 0 & \text{if } \mathrm{y}_{2\mathrm{i}} \leq 0 \text{ or} = . \end{cases}$$

The regression function (27) for a subsample having complete observations may be written as

$$(30) E(\Delta y_i \mid \Delta Z_i, \xi) = \beta \Delta Z_i + E(\Delta\varepsilon_i \mid \Delta Z_i, \xi)$$

where the conditioning argument ζ denotes the joint outcome of the two selection rules, or the sample selection regime.

Regression equation (27) can be obtained only when

$$(31) P = \Pr(D_1 = 1, D_2 = 1) = \Pr(y_{1i} > 0, y_{2i} > 0) = \Pr(\varepsilon_{1i} > -\beta_1 Z_{1i}, \varepsilon_{2i} > -\beta_2 Z_{2i}) = G(C_1, C_2; \rho)$$

where G denotes the standard bivariate normal distribution function with correlation coefficient $\pm\rho$ and $C_j = \beta_j Z_{ji}\ (j = 1,2)$. That is, equation (27) may be rewritten as

$$(32) E(\Delta y_i \mid \varepsilon_{1i} > -C_1, \varepsilon_{2i} > -C_2) = \beta \Delta Z_i + E(\Delta\varepsilon_i \mid \varepsilon_{1i} > -C_1, \varepsilon_{2i} > -C_2)$$

Given the normal specification, the conditional expectation on the right-hand side is

$$(33) E(\Delta\varepsilon_i \mid \varepsilon_{1i} > -C_1, \varepsilon_{2i} > -C_2) = \rho_{13} \frac{f(C_1)F(C_2^*)}{P} + \rho_{23} \frac{f(C_2)F(C_1^*)}{P} = \rho_{13}\psi_1 + \rho_{23}\psi_2$$

where f and F respectively denote the standard univariate normal density and distribution functions, $C_1^* = \dfrac{C_1 - \rho C_2}{(1-\rho^2)^{1/2}}, C_2^* = \dfrac{C_2 - \rho C_1}{(1-\rho^2)^{1/2}}$, P is defined as in equation (31), and

$$(34) \psi_1 = \frac{f(C_1)F(C_2^*)}{P}, \psi_2 = \frac{f(C_2)F(C_1^*)}{P}$$

Using (33) in (30), the regression equation that takes explicit account of the fact that Δy_i is observed only for the subsample having complete observations becomes

$$(35)\ \Delta \mathrm{y}_i = \beta \Delta Z_i + \rho_{13}\psi_1 + \rho_{23}\psi_2 + v_3$$

where $v_3 = \Delta\varepsilon_i - \rho_{13}\psi_1 - \rho_{23}\psi_2$ with $\mathrm{E}(\mathrm{v}_3 \mid y_{1i} > 0, y_{2i} > 0) = 0$.

Bivariate probit estimation will yield consistent estimates $\hat\beta_1, \hat\beta_2, \hat\rho$, hence $\hat{\mathrm{C}}_1, \hat{\mathrm{C}}_2, \hat{\mathrm{C}}_1^*, \hat{\mathrm{C}}_2^*$, and $\hat{\mathrm{P}}$. Using these in (34), we obtain $\hat\psi_1, \hat\psi_2$ for each individual in the subsample. Insert $\hat\psi_1, \hat\psi_2$ into (35), and then fit (35) by linear regression of Δy_i on $\Delta Z_i, \hat\psi_1, \hat\psi_2$ for the individuals in subsample. Consistency of the coefficient estimates follows from consistency of $\hat\psi_1, \hat\psi_2$'s.

The two Heckman selection terms in the within-twin-pair regressions (ψ_1, ψ_2) were insignificant and the returns to education parameter were unaffected. Therefore, there is no evidence that selection affected our estimates significantly. Table 18 contains original estimations in Table 5 and their corresponding Heckman-correction estimations.

6 Conclusions

To estimate true value of returns to education in China after Reform, we have used a new sample of identical Chinese twins to apply the within-twin-pair method to correct for omitted ability bias, and IV method to correct for measurement error. Using Fixed-Effect and GLS models, we get very similar results about the contributions of education to the wage rate. Thus we can conclude the following. The estimates from our twins sample confirm the theoretical prediction that, first, measurement error biases estimated returns to education down and, second, omitted ability biases estimates up. Third, in fact omitted ability bias exceeds measurement error bias indicating private returns to education of 3.6 percent. Fourth, it is worth noting that our within-twin-pair results are sufficiently precise to state that omitted ability bias is statistically significant. Using Hausman tests for differences between the Fixed-Effect and OLS coefficients, and between the GLS and OLS coefficients, we reject the null of no difference between them. Fifth, there is no evidence that participation selection bias of wage equation affects our estimates significantly. Sixth, using similar correlates of ability to Ashenfelter and Krueger (1994), such as tenure, marital status, Party membership, etc., we find no correlation between within-twin-pair differences in these measures and within-twin-pair differences in their education, but a strong correlation between average family measures and average family education. We find no evidence that ability bias is likely to bias our within-twin-pair results by more than the pooled results. Thus we expect ability biases to be less for within-twin-pair estimates than for estimates not controlling for ability. Conditional on positive ability bias, which we find, our estimates at least tighten the upper bound for the returns to education. Seventh, our results suggest that smoking is more likely to reflect family background than individual discount rates. Eighth, returns to education are indeed symmetric within twins, therefore we have no evidence that is inconsistent with the proposition that the mean returns to education may be identified by the standard Fixed-Effect equation. We can treat each pair of twins interchangeable.

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Table 1--Studies of Estimated Returns to Schooling Based on Samples of Brothers

Study	Sample
Gorseline (1932)	156 pairs of brothers from Indiana with data on 1927 income, schooling, occupation, and age
Jencks et al. (1972)	99 pairs of brothers from the Talent Survey follow-up, 150 pairs of brothers from NORC in 1973
Chamberlain and Griliches (1977)	292 pairs from NLS of young men with data on expected schooling and occupation, and 161 pairs with actual 1970 wage rate
Brittain (1977)	60 pairs of Cleveland brothers
Olneck (1976, 1977)	346 pairs of brothers from Kalamazoo and Michigan
Ashenfelter and Zimmerman (1977)	143 pairs of brothers from NLS in 1978 and 1981

Table 3--Studies of Educational Attainments

Study	Sample	Wage Measure
Becker (1960)	U.S. Bureau of the Census for 1910,1940 and 1950	Income was represented by father's occupation, mother's education, or monthly rental
Behrman and his associates (1994; 1996; 1999)	NAS-NRC Twins sample, Minnesota male twins	Estimated earnings were converted from occupational data using the 1980 census for Minnesota sample
Bound and Solon (1999)		
Miller et al. (2001)	Australian Twin Registry in 1980-82, and 1988-89	Reported earnings
Black et al. (2004)	Entire population of Norway who were aged 16-74	Reported earnings
Chevalier et al. (2005)	LFS (a quarterly sample of households in the U.K.)	Reported earnings
Ota and Moffatt (2002)	101 households spread between 4 districts of rural Aundhra Pradesh, India in 1999	Reported earnings

Table 4--Descriptive Statistics

Variable	MZ twins (1)	Nontwins (2)
Education (years of schooling)	12.22	11.73
	(2.89)	(3.07)
Age	34.78	43.27
	(9.64)	(8.42)
Married	0.66	0.94
	(0.47)	(0.24)
Wage (Monthly wage rate)	887.85	845.84
	(517.91)	(549.08)
Tenure (The number of years the interviewee has been working full time, starting from the age of 16)	15.03	21.70
	(9.93)	(9.05)
Party membership	0.22	0.29
	(0.41)	(0.45)
Venture (1, if the type of employment unit is joint venture with foreign companies, corporative venture with foreign companies or foreign owned venture; 0 if state owned unit, collective unit or private business)	0.04	0.03
	(0.21)	(0.16)
Spouse education	11.64	11.49
	(3.11)	(3.49)
Smoking (1, if the interviewee smokes at least half a pack per day; 0, if the interviewee smokes infrequently or does not smoke)	0.31	0.28
	(0.46)	(0.45)
Birthweight	2.44	3.11
	(0.59)	(0.52)
Father's education	0.41	0.27
	(0.49)	(0.44)
Mother's education	0.31	0.17
	(0.46)	(0.38)
Chongqing	0.11	0.17
	(0.31)	(0.37)
Haerbin	0.16	0.21
	(0.37)	(0.41)
Hefei	0.19	0.12
	(0.39)	(0.33)
Wuhan	0.40	0.34
	(0.49)	(0.47)
Sample size(individuals)	976	1277

Note: We have completed questionnaires from 3,002 individuals, among which 2,996 are twin individuals aged 17 to 62 and 6 are triplet individuals. We consider a pair of twins identical if both twins respond that they have identical hair color, look, age and gender. We have 914 complete pairs of identical twins, i.e., 1,828 individuals. For 495 of these pairs (990 individuals) we have complete wage information on both twins in the pair.

Table 5—OLS and Fixed-Effect Estimates of the Returns to Education for Chinese MZ Twins and Nontwins

	MZ Twins & Nontwins		MZ Twins			
	Pooled		Pooled	Within pair	Pooled	Within pair
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Education	0.067***	0.067***	0.082***	0.025*	0.084***	0.027*
	(16.71)	(16.91)	(13.85)	(1.68)	(14.14)	(1.87)
Age	0.023**	0.011	0.041***		0.036*	
	(2.49)	(0.89)	(2.60)		(1.88)	
Age-squared	-0.020*	-0.023*	-0.045**		-0.052**	
	(1.68)	(1.67)	(1.99)		(2.13)	
Gender	0.217***	0.210***	0.205***		0.202***	
	(9.05)	(8.72)	(5.32)		(5.25)	
Married		-0.033			-0.027	-0.043
		(0.75)			(0.53)	(0.83)
Tenure		0.016***			0.011*	0.015
		(4.77)			(1.86)	(1.52)
Twin pairs				488		488
Observations	2253	2253	976	976	976	976
R-square	0.17	0.18	0.22	0.01	0.23	0.02

All pooled regressions include city dummies.

Robust t statistics in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5.1--IV Estimates of the Returns to Education for Chinese MZ Twins

Variable	Pooled (1)	Within pair (2)	Pooled (3)	Within pair (4)
Education	0.087***	0.032*	0.088***	0.033*
	(13.10)	(1.65)	(13.36)	(1.77)
Age	0.040**		0.035*	
	(2.53)		(1.78)	
Age-squared	-0.043*		-0.050**	
	(1.90)		(2.04)	
Gender	0.205***		0.203***	
	(5.35)		(5.27)	
Married			-0.026	-0.043
			(0.50)	(0.81)
Tenure			0.012**	0.016
			(1.97)	(1.60)
Twin pairs		488		488
Observations	976	976	976	976
R-square	0.22		0.22	0

All pooled regressions include city dummies.

Robust t statistics in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 6 (Male)—OLS and Fixed-Effect Estimates of the Returns to Education for Chinese MZ Twins and Nontwins

	MZ Twins & Nontwins		MZ Twins			
	Pooled		Pooled	Within pair	Pooled	Within pair
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Education	0.064***	0.066***	0.075***	0.012	0.076***	0.015
	(12.05)	(12.28)	(9.31)	(0.62)	(9.41)	(0.77)
Age	0.028**	0.007	0.048**		0.037	
	(2.28)	(0.43)	(2.50)		(1.62)	
Age-squared	-0.026*	-0.017	-0.053**		-0.048*	
	(1.68)	(0.99)	(1.99)		(1.69)	
Married		0.054			0.041	-0.005
		(0.94)			(0.59)	(0.08)
Tenure		0.013***			0.006	0.013
		(2.58)			(0.81)	(0.92)
Constant	5.367***	5.741***	4.902***		5.082***	
	(21.82)	(19.39)	(14.03)		(12.69)	
Twin pairs				291		291
Observations	1189	1189	582	582	582	582
R-square	0.14	0.15	0.18	0	0.19	0.01

All pooled regressions include city dummies.

Robust t statistics in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 6.1--IV Estimates of the Returns to Education for Chinese MZ Twins

Variable	Pooled (1)	Within pair (2)	Pooled (3)	Within pair (4)
Education	0.075***	0.011	0.076***	0.014
	(9.18)	(0.54)	(9.41)	(0.68)
Age	0.048**		0.037	
	(2.50)		(1.62)	
Age-squared	-0.053**		-0.048*	
	(1.99)		(1.69)	
Married			0.041	-0.005
			(0.59)	(0.08)
Tenure			0.006	0.013
			(0.81)	(0.92)
Constant	4.901***		5.081***	
	(14.01)		(12.69)	
Twin pairs		291		291
Observations	582	582	582	582
R-square	0.18		0.19	

All pooled regressions include city dummies.

Robust t statistics in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 7 (Female)—OLS and Fixed-Effect Estimates of the Returns to Education for Chinese MZ Twins and Nontwins

	MZ Twins & Nontwins		MZ Twins			
	Pooled		Pooled	Within pair	Pooled	Within pair
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Education	0.070***	0.069***	0.096***	0.045**	0.096***	0.045**
	(11.25)	(11.15)	(10.70)	(2.08)	(10.97)	(2.04)
Age	0.018	0.023	0.036		0.047	
	(1.07)	(1.05)	(1.23)		(1.30)	
Age-squared	-0.013	-0.038	-0.038		-0.070	
	(0.56)	(1.39)	(0.89)		(1.51)	
Married		-0.166**			-0.156*	-0.114
		(2.43)			(1.92)	(1.06)
Tenure		0.017***			0.017	0.019
		(3.90)			(1.57)	(1.24)
Constant	5.325***	5.375***	4.662***		4.585***	
	(17.31)	(14.08)	(9.26)		(7.53)	
Twin pairs				197		197
Observations	1064	1064	394	394	394	394
R-square	0.14	0.16	0.25	0.03	0.26	0.04

All pooled regressions include city dummies.

Robust t statistics in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 7.1--IV Estimates of the Returns to Education for Chinese MZ Twins

Variable	Pooled (1)	Within pair (2)	Pooled (3)	Within pair (4)
Education	0.108***	0.075**	0.108***	0.071*
	(9.38)	(1.99)	(9.29)	(1.90)
Age	0.035		0.045	
	(1.17)		(1.24)	
Age-squared	-0.034		-0.067	
	(0.80)		(1.42)	
Married			-0.155*	-0.100
			(1.88)	(0.87)
Tenure			0.017	0.020
			(1.60)	(1.32)
Constant	4.528***		4.455***	
	(8.90)		(7.33)	
Twin pairs		197		197
Observations	394	394	394	394
R-square	0.24		0.26	

All pooled regressions include city dummies.

Robust t statistics in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 8—GLS, IV and Fixed-Effect Estimates of the Returns to Education for Chinese MZ Twins

	Without other covariates			With other covariates		
	GLS	GLS	IV	GLS	GLS	IV
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Own education	0.076***	0.025**	0.022*	0.077***	0.027**	0.023**
	(12.27)	(2.06)	(1.89)	(12.43)	(2.22)	(1.98)
Sum of education		0.033***	0.036***		0.033***	0.036***
		(4.70)	(5.35)		(4.66)	(5.41)
Age	0.042***	0.039***	0.039***	0.035**	0.033**	0.032**
	(2.94)	(2.71)	(2.71)	(2.17)	(2.00)	(1.98)
Age-squared	-0.046**	-0.040**	-0.041**	-0.054**	-0.047**	-0.048**
	(2.36)	(2.04)	(2.08)	(2.56)	(2.26)	(2.30)
Gender	0.203***	0.206***	0.184***	0.201***	0.204***	0.182***
	(5.28)	(5.36)	(4.77)	(5.21)	(5.30)	(4.70)
Married				-0.025	-0.025	-0.025
				(0.58)	(0.58)	(0.58)
Tenure				0.013**	0.012**	0.013**
				(2.18)	(2.09)	(2.15)
Constant	4.799***	4.640***	4.636***	4.936***	4.769***	4.769***
	(18.23)	(17.50)	(17.39)	(16.83)	(16.19)	(16.14)
Twin pairs	488	488	488	488	488	488
Observations	976	976	976	976	976	976

Notes: Each equation also includes an intercept term. Own education and sibling's education are instrumented using sibling's report of the other sibling's education as instruments.

All regressions include city dummies.

Absolute value of z statistics in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 9 (Male)--GLS, IV, and Fixed-Effect Estimates of the Returns to Education for Chinese MZ Twins

	Without other covariates			With other covariates		
	GLS	GLS	IV	GLS	GLS	IV
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Own education	0.067***	0.012	0.010	0.069***	0.013	0.011
	(8.37)	(0.74)	(0.63)	(8.47)	(0.83)	(0.70)
Sum of education		0.037***	0.038***		0.037***	0.038***
		(4.05)	(4.12)		(4.08)	(4.17)
Age	0.049***	0.044**	0.047**	0.038*	0.032	0.034
	(2.69)	(2.43)	(2.56)	(1.83)	(1.54)	(1.64)
Age-squared	-0.055**	-0.047*	-0.050**	-0.053**	-0.043	-0.046*
	(2.23)	(1.89)	(2.03)	(2.03)	(1.64)	(1.76)
Married				0.029	0.041	0.043
				(0.53)	(0.75)	(0.77)
Tenure				0.009	0.008	0.008
				(1.12)	(1.09)	(1.10)
Constant	4.979***	4.811***	4.779***	5.161***	5.014***	4.984***
	(14.88)	(14.30)	(14.10)	(13.86)	(13.47)	(13.33)
Twin pairs	291	291	291	291	291	291
Observations	582	582	582	582	582	582

Notes: Each equation also includes an intercept term. Own education and sibling's education are instrumented using sibling's report of the other sibling's education as instruments.

All regressions include city dummies.

Absolute value of z statistics in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 10 (Female)--GLS, IV, and Fixed-Effect Estimates of the Returns to Education for Chinese MZ Twins

	Without other covariates			With other covariates		
	GLS	GLS	IV	GLS	GLS	IV
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Own education	0.090***	0.046**	0.038**	0.090***	0.044**	0.036**
	(9.33)	(2.37)	(2.05)	(9.44)	(2.27)	(1.97)
Sum of education		0.028**	0.038***		0.030***	0.039***
		(2.55)	(3.70)		(2.67)	(3.86)
Age	0.036	0.035	0.031	0.043	0.045*	0.040
	(1.52)	(1.47)	(1.30)	(1.59)	(1.67)	(1.46)
Age-squared	-0.038	-0.035	-0.031	-0.067*	-0.067*	-0.063*
	(1.15)	(1.06)	(0.95)	(1.92)	(1.91)	(1.80)
Married				-0.134*	-0.149**	-0.152**
				(1.86)	(2.09)	(2.11)
Tenure				0.018*	0.017*	0.018**
				(1.95)	(1.87)	(2.04)
Constant	4.745***	4.588***	4.571***	4.730***	4.516***	4.520***
	(11.16)	(10.69)	(10.56)	(9.92)	(9.38)	(9.37)
Twin pairs	197	197	197	197	197	197
Observations	394	394	394	394	394	394

Notes: Each equation also includes an intercept term. Own education and sibling's education are instrumented using sibling's report of the other sibling's education as instruments.

All regressions include city dummies.

Absolute value of z statistics in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 12--OLS and IV Fixed-Effect Estimates of the Returns to Education for Chinese MZ Twins, Assuming Correlated Measurement Errors

Variable	OLS (1)	IV (2)	OLS (3)	IV (4)
ΔZ^*	0.019	0.036**	0.020	0.038**
	(1.31)	(1.99)	(1.46)	(2.14)
Δmarried			-0.047	-0.048
			(0.89)	(0.91)
Δtenure			0.014	0.016
			(1.43)	(1.59)
Twin pairs	488	488	488	488
Observations	976	976	976	976
R-squared	0.01		0.01	

Note: ΔZ^* is the difference between sibling 1's report of her/his own education and her/his report of sibling 2's education. The instrument used for ΔZ^* is ΔZ^{**}, the difference between sibling 2's report of sibling 1's education and sibling 2's report of sibling 2's own education.

Robust t statistics in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 13 (Male)--OLS and IV Fixed-Effect Estimates of the Returns to Education for Chinese MZ Twins, Assuming Correlated Measurement Errors

Variable	OLS (1)	IV (2)	OLS (3)	IV (4)
ΔZ*	0.009	0.015	0.012	0.018
	(0.46)	(0.69)	(0.61)	(0.83)
Δmarried			-0.005	-0.006
			(0.08)	(0.09)
Δtenure			0.012	0.013
			(0.89)	(0.95)
Twin pairs	291	291	291	291
Observations	582	582	582	582
R-squared	0		0	

Note: ΔZ* is the difference between sibling 1's report of her/his own education and her/his report of sibling 2's education. The instrument used for ΔZ* is ΔZ**, the difference between sibling 2's report of sibling 1's education and sibling 2's report of sibling 2's own education.

Robust t statistics in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 14 (Female)--OLS and IV Fixed-Effect Estimates of the Returns to Education for Chinese MZ Twins, Assuming Correlated Measurement Errors

	OLS	IV	OLS	IV
Variable	(1)	(2)	(3)	(4)
ΔZ*	0.029	0.081**	0.029	0.077**
	(1.38)	(2.59)	(1.35)	(2.52)
Δmarried			-0.132	-0.125
			(1.29)	(1.19)
Δtenure			0.017	0.018
			(1.13)	(1.16)
Twin pairs	197	197	197	197
Observations	394	394	394	394
R-squared	0.02		0.03	

Note: ΔZ* is the difference between sibling 1's report of her/his own education and her/his report of sibling 2's education. The instrument used for ΔZ* is ΔZ**, the difference between sibling 2's report of sibling 1's education and sibling 2's report of sibling 2's own education.

Robust t statistics in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 15-Between-Families and Within-Family Twin-Pair Correlations of Education and Other Variables

Correlation of average family education with average family characteristics		Correlation of within-twin-pair differences in education with within-twin-pair differences in other characteristics	
	Education		ΔEducation
Venture	0.0937	ΔVenture	0.0073
	(0.0453)*		(0.8767)
High-level exam mark	0.4801	ΔHigh-level exam mark	0.3165
	(0.0000)**		(0.0001)**
Married	-0.1664	ΔMarried	0.0239
	(0.0000)**		(0.4709)
Party member	0.2456	ΔParty member	0.1550
	(0.0000)**		(0.0000)**
Spouse education	0.6118**	ΔSpouse education	0.1517
	(0.0000)**		(0.0003)**
Tenure	-0.2583	ΔTenure	-0.0234
	(0.0000)**		(0.4870)
Smoking	-0.1477	ΔSmoking	-0.0838
	(0.0000)**		(0.0114)*

Significant level in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 16--Simultaneous Equation Estimate of the Returns to Schooling for Chinese MZ Twins

Model	Statistic estimated	
S.U.R.E	$\tau11$	0.049(4.06)***
	$\tau21$	0.025(2.13)**
	$\tau12$	0.032(2.54)**
	$\tau22$	0.058(4.92)***
	χ-square for Ho:$\tau21$=0	4.52
	Prob>χ-square	0.03
	χ-square for Ho:$\tau12$=0	6.47
	Prob>χ-square	0.01
Symmetry	χ-square for Ho:$\tau11=\tau22$	0.22
	Prob>χ-square	0.64
	χ-square for Ho:$\tau12=\tau21$	0.13
	Prob>χ-square	0.72
	χ-square for joint test	4.76
	Prob>χ-square	0.19
Vertical uniformity	$\beta1=\tau11-\tau21$	0.025(1.68)*
	$\beta2=\tau22-\tau12$	0.027(1.79)*
	F for Ho:$\beta1=\beta2$	0.80
	Prob>F	0.37
Horizontal uniformity	$\beta1=\tau11-\tau12$	0.019(0.91)
	$\beta2=\tau22-\tau21$	0.029(1.3)
	F for Ho:$\beta1=\beta2$	0.21
	Prob>F	0.65

Notes: S.U.R.E. system predicts each sibling's earnings as a function of both siblings' education.
Absolute value of z statistics in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 17-Hausman Test for OLS, Fixed-Effect and GLS

	Difference between OLS and FE not systematic	Difference between OLS and GLS not systematic	Difference between FE and GLS not systematic
	(1)	(2)	(3)
chi2	7.40	28.86	0.46
Prob>chi2	0.0065	0.0000	0.4998

Table 18-Heckman Test for Selection Bias

Variable	Table 5&5.1				Heckman-correction			
	Pooled		Within pair		Pooled		Within pair	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education	0.082***	0.087***	0.025*	0.032*	0.080***	0.083***	0.025*	0.033*
	(13.85)	(13.10)	(1.68)	(1.65)	(15.05)	(14.16)	(1.71)	(1.75)
Age	0.041***	0.040**			0.034**	0.033**		
	(2.60)	(2.53)			(2.26)	(2.21)		
Age-squared	-0.045**	-0.043*			-0.036*	-0.035*		
	(1.99)	(1.90)			(1.72)	(1.65)		
Gender	0.205***	0.205***			0.210***	0.211***		
	(5.32)	(5.35)			(6.00)	(6.04)		
Heckman selection term ψ					-0.384	-0.351		
					(0.78)	(0.71)		
Heckman selection term 1							-0.404	-0.387
							(0.94)	(0.90)
Heckman selection term 2							0.330	0.311
							(0.67)	(0.63)
Twin pairs			488	488			494	494
Observations	976	976	976	976	1209	1209	988	988
R-square	0.22	0.22	0.01		0.20	0.20	0.01	

All pooled regressions include city dummies

Robust t statistics in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%