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ENDOGENEITY OF SCHOOLING IN THE WAGE FUNCTION: EVIDENCE FROM THE RURAL PHILIPPINES

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ABSTRACT

This paper evaluates the effect (in terms of private returns) of investment in education on wages in the rural Philippines. Statistical endogeneity of education in the wage function may result from (1) unobserved determinants of education that also influence wages and/or (2) measurement error. Panel data are used that provide relevant instruments, particularly distance to schools and measures of household resources, *at the time of schooling*, to endogenize investments in education while estimating wage functions. The estimated return to education increases more than 60 percent when education is endogenized. This increase is robust to the inclusion of a measure of health, models of selection into the sample, and measurement error. The paper suggests how heterogeneous returns to education might account for the magnitude of the downward bias in returns to schooling.

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1. INTRODUCTION

Economic development typically leads to increased average levels of human capital for individuals, as measured by education levels and health outcomes. These increases reflect, in part, rising consumption. But what makes increases in human capital particularly important to economists is that they represent productive investments, undertaken by individuals, that yield returns. A thorough understanding of these returns is crucial in directing scarce (public and private) resources.

This paper estimates the private returns to investment in human capital and, in particular, education, in the rural Philippines. Within the commonly used logarithmic wage function framework, the paper focuses on the endogeneity of education, which is typically ignored for developing countries. Statistical endogeneity results from (1) unobserved determinants of education that also influence wages and (2) unobserved measurement error in education, or both. It biases estimated returns to education, thereby calling into question the interpretation of these returns in understanding the effect of education on earnings, an issue of significant concern to policymakers. This paper exploits community and household panel data that provide relevant instruments to endogenize investments in education while estimating wage functions. The key instruments are distance to schools and household resources measured *at the time of the schooling*. In addition to labor force participation, the analysis controls for the selectivity inherent in a random sample of nonmigrants from a relatively poor, high outmigration region. One drawback with this strategy is that it requires a long panel and thus yields a small sample.

The results indicate that the return to education increases more than 65 percent when education is treated as endogenous. This finding is robust when measures of health (or nutritional) status and controls for selection into the wage-earning group are included. Although important, conventional measurement error alone, for which there are independent measures in the data, accounts for only one-third of the bias. An interpretation of the results based on heterogeneous returns to education is suggested.

The following section of this paper presents the empirical specification of the model. Section 3 describes the data and selects the instruments for education. The fourth section presents the results and the final section offers a conclusion.

2. A SIMPLE MODEL OF WAGE FORMATION

A common strategy employed in measuring the results of human capital investments is to consider how they affect individual productivity.[1] Typically, a variant of the standard logarithmic wage equation is estimated where completed education is treated as the level of human capital and used as a regressor (Mincer 1974). A maintained hypothesis is that

[1] Another common method used for education is to equate the costs (including the opportunity costs of foregone earnings) to the benefits of additional schooling (measured by increased earnings) and then calculate an internal rate of return (Hossain and Psacharopoulos 1994). This approach also ignores the endogeneity of education.

wages serve as a proxy for productivity (as under perfect competition, for example). The basic (linear) model is

$$\omega_i = x_i'\gamma + S_i\delta + \epsilon_i, \quad \textbf{(1a)}$$

where ω_i is the logarithmic wage, x_i is a set of conditioning variables including, for example, sex and experience, S_i is the number of completed years of education, and ϵ_i is a stochastic error. By construction, the model ignores the underlying accumulation process for education; education is assumed to be exogenous.

Many authors have estimated this model, finding that returns to education, or δ in equation (1a), in developing countries are substantial (Psacharopoulos 1985; 1994). For example, Hossain and Psacharopoulos (1994) estimate a return of 11.9 percent for the Philippines in 1988. Since they use earnings data rather than wages, their estimates are not directly comparable to those reported below. Wages, used in this study, are generally considered a superior proxy for productivity because they are less likely to reflect labor supply decisions.

A strong assumption implicit in the model is the linearity of the wage-education profile. In the Philippines, calculating a single (or average) return to education as above may mask differences among primary, secondary, and higher education. When broken down by schooling level, Hossain and Psacharopoulos (1994) estimate 18.6 percent returns to an additional year of primary school, 10.2 percent to secondary, and 11.0 percent to higher education. Similarly, Strauss and Thomas (1991) estimate the returns to education, in Brazil, semi-parametrically (including a dummy variable for each year of completed education) and reject the linearity assumption. In particular, they note a significant decline in the return to education going from six (completed primary school) to seven years of schooling. The linearity assumption of the education-wage profile should be carefully evaluated.

Building on the basic model in equation (1a), attempts to control for a variety of factors have been made (Behrman 1990 and Strauss and Thomas 1995 review the literature). These include quality of schooling (Behrman and Birdsall 1983), gender and family background (Behrman and Wolfe 1984; Lam and Schoeni 1993), cohort effects, region, occupation (including selection into the labor market), and unobservables such as ability (Griliches 1977). Dimensions of human capital other than education (for example, skills, nutrition, health, and labor mobility) have been considered as well; typically, these are treated as endogenous.[2] After controlling for several of these factors, this study focuses on the endogeneity of one type of human capital, schooling.

THE ENDOGENEITY OF SCHOOLING

To understand more clearly the sources of schooling endogeneity, it is helpful to posit a simple structural model of education and wages. In addition to (1a), consider

$$S_i = x_i'\alpha + z_i'\beta + \nu_i, \qquad \textbf{(1b)}$$

[2] Examples include Glewwe (1996), Sahn and Alderman (1988), Schultz (1996), and Thomas and Strauss (1997). See Schultz (1997) for a general framework.

where x_i includes conditioning variables that directly influence both education and wages, $z_{i,}$ includes variables that directly influence education but only influence wages indirectly via their impact on education (for example, price of schooling), and ν_i is a stochastic error. Estimation of equation (1a) alone, the standard approach, yields consistent estimates (of δ), provided the two error terms, ϵ_i and ν_i, are uncorrelated.

There are at least two possible reasons why this necessary condition, uncorrelated errors, might fail in practice. First, education (and indeed all forms of human capital) is in part the result of constrained optimizing decisions made within the household (Becker 1993; Glewwe and Jacoby 1993; Griliches 1977; Jacoby 1994; King 1982; King and Lillard 1987). As such, the decision to attend school depends on market conditions and public services outside the household, as well as preferences and resource constraints within the household. To the extent that there exist unobservables that influence both education and the wage, the error terms will be correlated. Possible confounding variables include ability, health, and family background characteristics. Any resultant correlation between education and the wage, then, can no longer be reliably interpreted as a causal effect (that is, an economic return). After considering sampling error, credentialism, and the possibility that some years of education are unproductive, Strauss and Thomas (1991) argue that the decline in returns after grade six that they observe in Brazil is most likely the result of endogeneity. There seems to be unobserved heterogeneity between the types of people who leave school prior to (or just after) completing a level such as primary.

A second possible reason is that education, S_i, may be measured with error. In the case of random measurement error, this will lead to downward bias in the estimates of the return to education. When there is more than one mismeasured regressor, unambiguous downward bias also requires that any measurement error for the other regressors be uncorrelated across regressors. Measurement error may explain in part the Strauss and Thomas (1991) results. If it is greater for grade 7 (just after completion of a level), for example, the returns to this grade will be biased downward. Of course, one possible cause of more measurement error is the unobserved heterogeneity they suggest.

In general, it is not possible to sign theoretically the bias that arises when one ignores the endogeneity of schooling and it is, therefore, an empirical issue. A variety of strategies have been employed to determine the importance of the different sources of bias (Willis 1986). Often, one is able to address both problems within a single technique. This is critical because it is possible that they have offsetting effects and controlling for one but not the other could be worse than ignoring both.[3] A small sample of the related literature is reviewed below to demonstrate the strategies used and provide evidence that bias does exist. Much of the pioneering work has been done for the United States.

One common approach is to include proxy measures of important unobservables in the wage equation. On its own, this technique does not address measurement error. Griliches (1977) examines the impact of ability bias on the education coefficient by putting

[3] For example, Ashenfelter and Zimmerman (1997) present evidence of opposing biases that almost exactly offset one another for a sample of brothers in the National Longitudinal Study of Youth (NLSY).

IQ test scores in the wage function to control for ability. Lam and Schoeni (1993) control for family background with education of parents, spouses, and spouse's parents; in doing so, returns to own education decrease around 25 percent. They interpret the direct impact of these parental controls as reflecting unobservables. It turns out that plausible amounts of random measurement error could also explain most of the decline in returns they find. Also, conditioning on marriage opens the door to other unknown biases from marriage matching. Another important unobservable, particularly for developing countries, where much of the labor is physical, is health status. Estimates conditioned on measures of nutrition and health status (Sahn and Alderman 1988; Schultz 1996; Thomas and Strauss 1997) address this. In most cases, the health measures are treated as endogenous themselves. These are discussed further in the following subsection.

Perhaps the dominant solution to the problem of endogeneity of education, however, is to make identification assumptions for system (1) (equations [1a] and [1b]), allowing estimation of equation (1a) alone.[4] These assumptions usually lead to fixed effects and/or instrumental variables techniques. Whereas the latter strategy also addresses the problem of measurement error, the former typically requires additional methods to do so. The instrumental variable strategy typically looks to the educational

[4] More structural approaches, for example, the estimation of the system (1), will not be discussed. See Willis and Rosen (1979) for an interesting example. The identification assumptions for these systems are the same ones discussed here; for example, Willis and Rosen assume that parental education only affects the child's wage through education.

attainment literature for appropriate instruments, as this paper does, or exploits some "natural experiment."

When there is reason to believe that the important unobservables are at the household (or family) level, an appropriate strategy is to use household fixed effects (Ashenfelter and Zimmerman 1997; Behrman and Wolfe 1984). If they are at the individual level, however, then individual or twin fixed effects are better.[5] Ashenfelter and Krueger (1994) is an example of the twins fixed effects approach that carefully evaluates measurement error. Using a sample of U.S. identical male twins, each reporting his own and his brother's education, Ashenfelter and Krueger use a twin fixed effects framework to control for ability, and then instrument for education with the brother's reported measure to control for measurement error. Their results suggest that though there is little ability bias, there is substantial measurement error. Returns increase from 9 percent to 12–16 percent after instrumentation, an amount that cannot be fully explained by random measurement error alone. They go on to evaluate some correlated forms of measurement error but, even with these, returns based on instrumental variables are 20 percent higher than ordinary least squares estimates.

After finding little ability bias in the United States, Griliches (1977) goes on to evaluate other sources of endogeneity of schooling, as well as measurement error, using

[5] Angrist and Newey (1991) estimate fixed effects at the individual level with multiple observations over time. Identification comes from those whose education changes while they are in the labor force. This strategy on its own might exacerbate measurement error problems since reported changes in education might be erroneous.

two-stage least squares. Assuming that family background characteristics (including parental education, occupation, and family size) influence wages only through schooling and ability (that is, these characteristics are in z_i of equation [1b]), Griliches concludes that the ordinary least squares estimates of the return to education are probably unbiased or even downward biased, contrary to a conventional pure ability bias story. If parental characteristics affect wages through some other channels (for example, via some other aspect of human capital like health), then such an identifying assumption would be invalid (Strauss and Thomas 1995).

Card (1993) uses variation in geographic proximity to four-year colleges in the United States for identification. Estimating a wage equation on 1976 wages, he uses the presence (dummy variable) of a four-year college ten years earlier as an instrument for education. He also treats potential experience as endogenous and instruments for it with age. He finds that return to education increases more than would be expected from random measurement error alone.

Angrist and Krueger (1991) employ a "natural experiment" instrument strategy, assuming that quarter of birth is uncorrelated with wages except through their effect on education via school-start age policy and compulsory school attendance laws.[6] They find little difference between the two-stage and ordinary least squares estimates for the United States, suggesting that schooling endogeneity is not empirically an important source of

[6] Harmon and Walker 1995 conduct a related analysis for the United Kingdom, exploiting changes in compulsory schooling laws.

parameter bias. Although their assumption seems plausible, their work has been called into question on different grounds; the correlation between the instruments (quarter of birth) and completed education is very low. In this instance, even a *weak* correlation between the instruments and the errors will lead the technique of instrumental variables to be biased toward ordinary least squares (Bound, Jaeger, and Baker 1995).[7] It is necessary to consider the explanatory power of the instruments.

The above studies and others (Angrist and Krueger 1992; Butcher and Case 1994; Kane and Rouse 1995) suggest that the returns to education in the U.S. are, if anything, biased downward in an ordinary least squares regression, though it is unclear exactly how to account for those results. For developing countries, the evidence is more mixed, partly due to incomparability across studies. Behrman (1990) concludes that most standard estimates overstate the return to education and cites several studies supporting this finding. Few of these, however, simultaneously control for both endogeneity and measurement error and several of them use earnings rather than wage data. Strauss and Thomas (1995) review several additional articles and suggest the evidence is inconclusive and warrants further study. This author proposes using an expanded set of instruments, including indicators of the availability of schooling and household resources measured contemporaneously with an individual's schooling decisions. These instruments are

[7] Angrist and Krueger (1992) use another "natural experiment," the Vietnam-era draft lottery, in conjunction with educational deferments, to provide instruments for education. After showing that the instruments do have strong explanatory power, they again find only slight upward bias in the ordinary least squares estimated returns.

discussed more thoroughly in the data section below. They will be combined with more commonly available instruments, including parental education. Before proceeding, however, two additional specification issues that will be considered in the analysis require mention.

MULTIPLE DIMENSIONS OF HUMAN CAPITAL

Investment in schooling is not the only form of human capital investment that individuals undertake. Thomas and Strauss (1997) argue that health human capital is also important in wage determination. Thus to the extent health is omitted it may lead to biased estimates. Measuring health status, however, is rather difficult. Thomas and Strauss proxy for it with height, body mass index (BMI),[8] and various calorie intake measures. Height, the result of a cumulative process, is considered a good indicator of long term health status, while the others measure shorter term dimensions of health. As mentioned above, it is possible that some instruments (for example, parental education) will be correlated with these health measures. These issues will be explored with a subsample for which there is anthropometric data in order to control directly for measures of health.

As with schooling decisions, investments in health and their subsequent outcomes will also be endogenous and subject to many of the problems mentioned in the previous subsection. This is particularly likely for short term measures, such as BMI. While it is

[8] Weight/height^2 measured in kg/m^2.

possible that current health influences wages earned in the labor market, there may also be a feedback effect whereby current wages affect the consumption patterns, nutritional intakes, and health of laborers. Thomas and Strauss (1997) use food prices and nonlabor income of workers and other household members to instrument all their health measures except height (and they do not instrument for education).

Schultz (1996) estimates logarithmic wage equations in Ghana and Côte d'Ivoire with four measures of human capital: education, height, BMI, and labor migration (measured by a dummy variable indicating those who have migrated). Initially, he treats all four of them as endogenous. His instrument set includes parental education and occupation, current food prices, and indicators of local health infrastructure. An implicit assumption is that the relative distribution of available services has not changed much over time. Individual Hausman tests reject the exogeneity of height and BMI but not of education and labor migration. Apparently, conditional on the other human capital variables, the two-stage least squares estimate of returns to education does not differ significantly from the ordinary least squares estimate in these samples.

SELECTION INTO THE WAGE LABOR MARKET

Lastly, selection into the labor market is almost certainly nonrandom. In a developing country, such a selection process is complicated by the large number of self-employed workers, typically farmers. For these workers, there is no observed wage or productivity measure and therefore they cannot be directly included in standard wage function estimation.

An additional selection issue arises in a region of high out-migration. Higher education might affect the probability of migration and consequently the returns to education, particularly if there is an urban-rural earnings gap (Schultz 1988). Also, migrants might be a select group in other ways (for example, ability). Consequently, estimates based only on those remaining in an area may be misleading; this potential bias is typically ignored (for an exception, see Lanzona 1995). Finally, a large number of grown children have left the parental home but settled locally. Given the panel nature of the data, this analysis can partly address these problems because there is basic information on respondents who migrated out before the final survey. All of these factors suggest a complicated selection process, closely linked to the life cycle of the household. Hence, a selection equation will be identified and estimated.

3. THE SETTING, DATA, AND SELECTION OF INSTRUMENTAL VARIABLES

THE PHILIPPINES

The Philippines provides an interesting setting for studying household decision making and rural labor markets. Often described as a "Latin American" country in Asia due to its colonial history, development strategies, and progress, the Philippines is classified by the World Bank as a lower middle income country given that its annual per capita GNP was $1,050 in 1995.[9] Much of the economy (22 percent overall) remains in agriculture, particularly outside Metropolitan Manila. Despite its low income, however, development of human capital has been strong. In 1995, adult life expectancy at birth was 66 years and adult literacy rates were 95 percent, better than most other countries with similar levels of income per capita. Despite strong, though declining, population growth (2.2 percent annually from 1990 to 1995), the younger generations are also benefitting. Primary school enrollment ratios were 109 percent, secondary, 74 percent, and tertiary 28 percent in 1993; these are comparable to many developed countries. Some commentators have even referred to the Philippines as being "over-educated." Evidence for such a claim is unclear, however, and the high returns to education estimated in this paper do not support it.

BICOL MULTIPURPOSE SURVEY

The Bicol Multipurpose Surveys (BMS) are used in this study.[10] Together, they provide a panel data set with three observations (1978, 1983, and 1994) for Camarines Sur, the main province of the Bicol region of the Philippines. Initiated as part of a major development scheme for the region, the surveys provide comprehensive information at the

[9] Statistics in this subsection are from the 1997 *World Development Report*.

[10] The author participated in the planning and collection of the 1994 BMS. See Popkin and Roco (1978), Research and Service Center (1983), Lanzona (1994), and Maluccio (1997) for further description of the data.

individual, household, and community levels. This study draws information mainly from the most recent (1994) survey, using data from the earlier surveys to a lesser extent.

The Bicol region has been, and remains, one of the poorest in the Philippines. In 1974, its per capita income was 50 percent of the national average. In 1988, it had the highest incidence of rural poverty[11] for the 13 regions in the country (Balisacan 1993). Located at the southern tip of the island of Luzon, Camarines Sur is the most populated and well off province in Bicol and contains the region's major city. Reflecting its name, which means granary or storehouse, a large proportion of the 1.3 million (2.2 percent of the Philippine population) residents of Camarines Sur are engaged in agricultural activities (National Statistics Office 1990). Its relative poverty suggests that the Bicol region is not representative of the Philippines as a whole and all results should be interpreted with this in mind.

In 1978, the BMS interviewed 1,136 household in Camarines Sur chosen from a stratified random sample. Of those, 691 households were interviewed again 16 years later in 1994, forming the base estimating sample for this study.

To guide the selection process of instruments for education, the Philippine educational system is described first. Patterned after the U. S. system, it consists of six years of primary school,[12] four years of secondary (or high) school, and four or five years

[11] As measured by either the head count (80 percent) or poverty gap indices.

[12] Some private primary schools offer seven years but less than 5 percent of sample attended these.

of college.[13] After mandating that every child must remain in school until completion of primary (which is tuition free at public schools), the government undertook extensive school building between 1960 and 1975 (King and Lillard 1987). As a result, primary enrollment ratios (those attending primary school as a percent of the population in the relevant age group) have been around 100 percent since the late 1970s and completion of primary school is common. Enrollment ratios at the secondary and tertiary levels were 65 and 38 percent, respectively, in 1985, near the levels of many developed countries (Tan and Paqueo 1989). Finally, there are no significant sex differentials. The educational system of the Philippines is quite advanced for a lower middle income country.

While most primary schools are public, private education is common at postprimary levels. Among those enrolled in secondary school nationwide, 43 percent attend private schools; the corresponding figure for college is 85 percent. Fees are typically higher for private institutions, largely because they receive little public support, while quality is perceived to be lower (Tan and Paqueo 1989). Both types of schools will be considered in the analysis.

How can the BMS data be brought to bear on the question of endogenous schooling? Consider respondents for whom there are wage observations in 1994 and who were of school age during (or just prior to) one or both of the first two surveys.[14] This

[13] Any postcollege schooling is coded at 15 years; less than 10 percent of the population have this much schooling.

[14] For this subsample, only a very small number of individuals report earnings in more than one survey. Furthermore, the majority of those reporting a wage in 1994 have no siblings doing so. Therefore, standard panel data analysis is not possible.

limits the sample to children of 1978 household heads, between the ages of 20 and 44 in 1994.[15] The challenge is to find some valid instruments for completed education in a wage function fit to wage data drawn from the 1994 resurvey. By construction, the first two surveys include household and community level variables measured contemporaneously with respondents' schooling decisions.

Among the relevant variables are distances to different types of schools (primary and secondary, public and private) from the household, which can be used as proxies for the price of schooling (King and Lillard 1987).[16] Since only 10 percent of the sample has less than a primary school education *and* the average distance to primary school is less than 1 kilometer, only distances to secondary schools are considered. As noted above, private education at the secondary level is common. In the sample, the percentage having attended private secondary school (46 percent) or private university (86 percent) are remarkably similar to the national averages. Since an individual's decision to attend public or private school is endogenous, the distance (and its square) to the nearest secondary school of *either* type, regardless of which type of school was attended, is used here. To partially account for the difference in private schools, an indicator of whether a private secondary school was in the village in 1983 is included.

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[15] The attrition rate for households was 40 percent between 1978 and 1994.

[16] King (1982), using the 1983 BMS, uses distance to school × village-level average child wages as a proxy for the price of schooling.

Distance to school and other community characteristics would not be valid instruments in a setting where families move to certain communities because of their characteristics. Such behavior could introduce choice-based correlation between community and household level variables. Migration in the Bicol region, however, does not appear to be driven by these factors; most of the survey respondents were long time residents in their communities in 1978 when the first round of the survey was implemented.[17] Settlement patterns are more likely to be related to land ownership and the government land reform of the early 1970s. In addition to the distance between household and school, the survey also provides a measure of distance between the village center and the nearest school. As an instrument, this measure has the advantage of being exogenous to intracommunity settlement patterns but it would be less efficient if individual household location were exogenous. Results using the more robust distance-to-village-center measure parallel those for the distance to household measure; only the latter are reported below.

Distance to school might also be an invalid instrument for education if school placement is nonrandom (Rosenzweig and Wolpin 1986; Pitt, Rosenzweig, and Gibbons 1993). For example, if secondary schools were being built just prior to the 1983 survey in areas where average educational attainment was low, using observations on children who might have been in school several years earlier would induce a negative correlation

[17] In 1978, more than 80 percent of the respondents had been in the same village 5 or more years and 60 percent 10 or more years.

between schooling and the presence of a secondary school. A more subtle possibility is that other types of public programs (such as health facilities) are highly correlated with schooling placement and may have effects on education (Rosenzweig and Wolpin 1982). These concerns are examined to the extent possible in the empirical analysis.

Several 1978 household level characteristics are also used as instruments for completed education. First, two indicators of household wealth are used, the value of the home[18] and a binary indicator of whether or not agricultural land is owned. A variety of other assets (including ownership of durable goods), all of which are highly correlated with these two, have also been considered and do not change the results substantively. They are excluded in order to keep the analysis parsimonious. The theoretical impact of these instruments is ambiguous. If schooling is a normal consumption good and these instruments measure wealth, they should increase completed schooling. Owning agricultural land may increase the marginal product of child labor at home, raising its opportunity cost, thereby decreasing completed schooling. This effect would be mitigated, however, if schooling also raised the marginal productivity of labor on the farm.

Lastly, both mother's and father's completed education are used. These can influence child schooling in a variety of ways. First, they serve as a proxy for permanent income (especially paternal education). Second, they may reflect parental preferences.

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[18] Note that although value of the home is likely to be measured with error if the housing market is thin, this is not a problem for instrumental variables estimation unless this error is correlated with the error in equation (1a).

Third, they may affect the education production process. Care must be taken when using these, and the extent to which they reflect other factors, such as health, are evaluated.

For a select subsample, essentially those who were at home or nearby during the interview, there is information on height and weight in 1994. These are combined to construct BMI as a proxy for health as human capital. Instruments for this short-term health measure include community level food prices, indicators of sanitation, and the availability of and distance to health care facilities, all measured in 1994.

Over half of the households in the sample are agricultural households. Therefore, many workers are self-employed farmers who choose not to enter the wage labor market. Also, the panel data reveal that a substantial number of individuals who were of school age during the first and second survey periods have left the household. Thus selection of observed wage earners into the sample requires both working in the wage labor market *and* remaining in the parental home.

This is a complex selection process involving the household's life cycle as well as (individual) migration decisions. Given the focus of this paper on returns to education, a simple selection-correction framework is implemented and its impact on returns to education evaluated. The identifying instruments include a measure of the roots in the community (logarithm of the length of residence in the community in 1978) and a binary indicator of whether the individual's maternal grandfather worked in agriculture. The former should help identify the migration decision and the latter, along with the wealth measures mentioned above, the wage labor market participation decision. Since most of the children were in school in 1978, it is plausible that the 1978 measures of wealth are less contaminated by their individual labor supply decisions.

Appendix Table 6 provides summary statistics for the sample of respondents aged 20–44 in 1994 categorized into three groups: wage earners remaining in the parental home (hereafter, wage earner-stayers), nonwage earners remaining in the parental home (nonwage-stayers), and migrants. The first feature of the sample to note is that only 10 percent of the sample are wage earner-stayers. Reporting a daily wage for the previous week, they represent a variety of occupations from farm laborers to government clerks. Nearly three-fourths of the sample have left the parental home. Thirty percent of these, however, have moved only a short distance, remaining in the same village.

Comparing these three groups along observable dimensions, they appear somewhat different. For example, average education for those remaining at home is similar (9.0 years) but migrants have significantly less (8.5 years), even after controlling for age, sex, and rural region in a regression with education as the dependent variable. Average years of education hide important spikes in the distribution (for all three groups) of around 30, 20, and 10 percent at 6 years (completed primary), 10 years (completed secondary), and 15 years (completed college), respectively. In all the subsamples, average female education is higher than that of males. Not surprisingly, the migrants are five years older, on average, and more than twice as likely to be married. Finally, there are more men than women remaining in the household.

4. ESTIMATES OF THE RETURN TO EDUCATION

First the endogeneity of education is investigated, and selection issues set aside. Table 1 presents ordinary least squares estimates of the logarithmic daily[19] wage equation ignoring both selectivity and the endogeneity of education. The equations include the standard regressors with one substitution. Given the objectives of the study, age rather than the more commonly included potential experience, which is usually defined as age minus years of education, is used.[20]

In the first column, a linear specification for education indicates a return of 7.3 percent per year.[21] Standard F-tests fail to reject that the sample should be stratified by gender and/or region after controlling for these with dummy variables and interaction terms. In the second column, years of postsecondary education, or beyond ten years, are also included (a spline) showing that the linear specification may be somewhat misleading. The return to postsecondary years of education (the sum of the coefficients

[19] Although hours worked are reported, daily wages seem to be the relevant measure, especially within the rural labor markets where casual labor is often hired on a daily basis. Specifications using logarithmic hourly wages (calculated as daily wage divided by number of reported hours) provide essentially the same conclusions though they are less precisely estimated.

[20] An alternative is to use potential experience and instrument it with age as in Card (1993). Given the small sample size, age is included directly, thus providing more precise estimates albeit of a slightly different parameter (see footnote 18).

[21] Since age, and not age minus years of education, is used for experience, this formulation differs from the original Mincer (1974) specification and thus (even under his assumptions) one can no longer interpret the coefficient strictly as a return since it now directly incorporates the (negative) impact that additional schooling has in reducing experience. I refer to it as this for ease of exposition.

Table 1 Ordinary least squares estimates of return to education
Dependent variable: Log daily wage

	Measure of education used		
	Linear	Spline	Postsecondary
Intercept	3.30	3.61	3.75
	(14.0)	(14.4)	(17.1)
Years of completed education	**0.0730**	**0.0231**	-
	(6.41)	(1.22)	
Years completed postsecondary education (beyond 10 years)	-	**0.1215**	**0.1581**
		(3.27)	(7.20)
Age (in 1994) × 100	2.785	2.558	2.953
	(0.88)	(0.82)	(1.00)
$Age^2 \times 1{,}000$	–0.297	–0.249	–0.365
	(0.29)	(0.25)	(0.37)
Male	0.328	0.356	0.353
	(4.00)	(4.38)	(4.33)
Rural community	0.193	–0.170	–0.169
	(2.34)	(2.08)	(2.08)
Sample size	250	250	250
R^2	0.25	0.29	0.28
F-test all variables	16.6	16.0	19.0
F-test age variables	4.0	3.8	4.0
F-test education variables	-	26.7	-

Note: Absolute value of t-statistic in parentheses.

on years of education and years of postsecondary education) is over 14 percent.[22] This dramatic increase in private returns coincides with an approximate 500 percent increase in school fees from secondary to postsecondary institutions. It would be incorrect, however,

[22] If number of hours worked is positively correlated with postsecondary education, then it is possible that the high returns estimated here are an artifact of using daily rather than hourly wages. This turns out not to be the case since hours worked are not significantly correlated with education and the returns estimated using hourly wages are equally as high.

to conclude that the return to education for years 1 to 10 is not significantly different from zero for at least two reasons. First, years of schooling and years of postsecondary schooling are highly correlated (0.83) and are jointly significant as the F-test on the education variables indicates (p-value of 0.0001). Second, in a semi-parametric specification with each year of education represented by a dummy variable (not shown), there are positive and significant returns to all years beyond the fourth grade.

All previous studies evaluating the endogeneity of education use linear education and for the United States, this appears to be a reasonable assumption. To control for endogeneity of education *and* capture the shape of the logarithmic wage-education relationship in these data, one might want to instrument for a spline specification. Given the instrument sets described above and the cumulative nature of schooling, however, it is difficult to justify exclusion restrictions for a traditional instrumental variable approach. For example, because of the sequential nature of schooling, access to primary school influences postsecondary education achievement. Alternatively, factors directly affecting higher education could impact early education to the extent that individuals take them into account in present discounted value calculations. An alternative approach is to use two-stage least squares. Although perhaps theoretically more appealing, this approach turns out to be statistically infeasible (predicted years of education and predicted years of postsecondary have a correlation of 0.98), generating volatile point estimates and large standard errors.

Another alternative is to assume there is no return to the first ten years of education (despite the arguments against this presented above) and consider only years of postsecondary schooling. Ordinary least squares estimates using this specification are presented in column 3 of Table 1 and indicate returns of nearly 16 percent. A parallel analysis using only years of postsecondary schooling is carried out (but not reported) and its results discussed. They are qualitatively similar to (and indeed stronger than) those for the base specification presented below.

The other conditioning variables are very similar across all three specifications and are in accord with the usual findings in the literature. Men earn higher wages, wages are lower in rural regions, and the relationship between logarithmic wages and age is increasing and concave over the range of ages in the sample.

In Table 2, the first-stage regressions instrumenting for education are reported, including an F-test on the instruments (to be excluded in the second-stage logarithmic wage equation).[23,24] Distance from the house to the closest secondary school (public or private) has a significant negative impact on education, consistent with its interpretation as a proxy for price of schooling. This effect is attenuated at longer distances; since most urban areas have a secondary school within a few kilometers, this indicates a less negative impact of distance in the rural areas. Conditional on distance, if the closest secondary

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[23] Staiger and Stock (1994) show that 1/F is an approximation of the bias toward the OLS estimates and suggest a revised critical value of 10 for the F-test on the excluded instruments. This critical value obtains for the latter two instrument sets.

[24] Not surprisingly, given the instrument set, results predicting years of postsecondary education are even stronger with all F-tests on the excluded instruments greater than 15.

school is private, there is a large positive impact on completed schooling (more than two years). Presence of a private secondary school may be picking up some

Table 2 First-stage ordinary least squares estimates of education

Dependent variable: years completed education

	Instrument set		
	IV-1	IV-2	IV-3
Wage equation regressors			
Intercept	6.45	2.58	2.52
	(5.27)	(2.04)	(2.01)
Age (in 1994) × 10	5.599	5.074	4.111
	(3.35)	(3.30)	(2.79)
Age^2 × 10	–0.162	–0.140	–0.120
	(3.00)	(2.81)	(2.52)
Male	–1.477	–1.000	–1.007
	(3.38)	(2.40)	(2.59)
Rural community	–0.285	0.528	0.271
	(0.61)	(1.17)	(0.62)
Instruments			
Distance to secondary school (kilometers)	–0.280	–0.205	–0.222
	(2.20)	(1.73)	(1.97)
Distance to secondary $school^2$ × 100	0.908	0.675	0.791
	(1.48)	(1.19)	(1.47)
Private secondary school in village	2.348	1.147	1.144
	(3.75)	(1.89)	(1.97)
Mother's years completed education	-	0.254	0.252
		(2.88)	(3.00)
Father's years completed education	-	0.250	0.167
		(3.00)	(2.06)
Logarithm of value of house	-	-	1.800
			(4.26)
Own agricultural land	-	-	0.190
			(2.51)
Sample size	250	250	250
R^2	0.18	0.31	0.38
F-test all variables	7.8	12.2	13.5
F-test on excluded instruments	8.2	14.9	15.6

Note: Absolute value of t-statistic in parentheses.

community wealth effects, a hypothesis that is supported by a 50 percent decrease in the coefficient when household-level instruments are added in the latter two specifications.

The parental education and household wealth instruments are all significant at a 5 percent level and raise completed education. When wealth measures are added, the coefficient on father's (but not mother's) education declines, suggesting that it is correlated with household wealth.

During this period, the Philippines was investing heavily in education and school building. As discussed earlier, if school placement was purposive, measures of schooling availability may no longer be valid instruments. For example, (endogenous) placement might reflect a convergence in educational service availability, with new schools being built in areas with lower educational attainment. Alternatively, divergence might occur if new schools were built mainly in wealthy communities. Is endogenous placement an empirical concern in these data? This possibility is assessed by restricting the sample to individuals born before 1960 (and thus likely to have completed any secondary schooling by 1978) and then regressing an indicator of whether a new secondary school was built in the community between 1978 and 1983 on their completed education and the other covariates from the wage function.[25] A negative coefficient on the education variable would be consistent with secondary schools being (endogenously) placed in areas with low average levels of education. In rural areas, where program placement is more of a

[25] Thomas and Maluccio (1996) employ a similar strategy examining the placement of family planning centers.

concern, the estimated coefficient is negative but small and insignificant. While far from proving that schools are being randomly placed across communities, this evidence does suggest that the schooling instruments are not obviously invalid.

Table 3 presents the results from two-stage least squares estimation of the logarithmic wage equation. In all three specifications (using different instrument sets as defined in Table 2), the returns to education increase 65 percent or more over the original ordinary least squares estimate of 0.073 (from Table 1, column 1). As the parental characteristic instruments are added, the point estimate on education declines, suggesting that there is not a problem with those instruments being correlated with other unobservables, like health, that are also positively correlated with wages. For all three two specifications, exogeneity tests (Hausman 1978) reject at a 5 percent significance level the null hypothesis that the ordinary and two-stage least squares estimates of the coefficient on education are the same. Returns to postsecondary education (not shown) increase nearly 50 percent and are significantly different in specifications using instrument sets IV-2 and IV-3 (Table 3). Therefore, it is not the case that the instrumental variables estimates are merely reflecting the higher returns to later years of education. Returns to experience are no longer precisely estimated and the coefficients for male and rural both become more positive relative to the ordinary least squares estimates.

Overidentification tests fail to reject the null hypothesis that the model is correctly specified and the exclusion restrictions on the instruments are valid. The first of these tests all the exclusion restrictions jointly (Davidson and MacKinnon 1993). As an

Table 3 Two-stage least squares estimates of return to education

Dependent variable: Log daily wage

	Instrument set		
	IV-1	IV-2	IV-3
Intercept	2.84	2.96	2.97
	(8.01)	(10.6)	(11.1)
Years completed education[a]	**0.1451**	**0.1253**	**0.1233**
	(3.58)	(5.14)	(5.84)
Age (in 1994) × 100	–1.355	–0.218	–0.105
	(0.33)	(0.06)	(0.03)
$Age^2 \times 1{,}000$	0.870	0.549	0.517
	(0.67)	(0.49)	(0.47)
Male	0.431	0.403	0.400
	(4.12)	(4.41)	(4.48)
Rural community	–0.132	–0.149	–0.150
	(1.39)	(1.69)	(1.72)
Sample size	250	250	250
pseudo R^2	0.17	0.21	0.23
F-test all variables	9.8	13.0	14.6
F-test age variables	1.9	2.3	2.3
Overidentification test	0.7	3.4	5.8
p-value	[0.78]	[0.50]	[0.44]
Hausman Test on education	3.4	5.9	8.0
p-value	[0.06]	[0.02]	[0.01]

Notes: Absolute value of asymptotic t-statistic in parentheses and p-values in square brackets.

[a] Education estimated as endogenous, based on instrument sets IV-1, IV-2, and IV-3 in columns 1, 2, and 3, respectively (see Table 2).

additional test for overidentification, each of the parental education and household wealth variables is put directly into the wage equation one at a time (while instrumenting for education with only the schooling variables). In no case are they significant, providing further evidence that they do not have a direct impact on wages and that their exclusion from the wage equation is valid.

The methodology proposed in this paper relies on using instruments for education measured at the time schooling decisions were being made, and thus requires a long panel of data. (Alternatively, it is possible to design surveys to ask retrospective questions though their accuracy would be less certain, especially for wealth measures.) What about simply using current measures of these resource constraints as instruments? Doing so assumes, for example, that the relative distribution of schools has not changed very much during the relevant period. In the Philippines, where the burst of school building occurred before the first survey in 1978, this turns out not to be a bad assumption; instrumenting with 1994 measures does not dramatically alter the results. Of course, without the earlier measures, such an assumption would remain untestable. Furthermore, an assessment of the endogeneity of school placement would not have been possible.

The results discussed thus far do not control for other dimensions of human capital. Separate regressions controlling for current health as measured by BMI are presented in Appendix Table 7.[26] Note the sample size is nearly halved due to missing anthropometric

[26] Specifications with height (alone and with BMI) were also considered; height does not have a significant impact on wages in these data though this may be due in part to the small sample size.

observations. In the ordinary least squares regression in column 2, the logarithm of BMI is significant but its inclusion in the regression does not change the coefficient on education (compare with column 1). The two-stage least squares estimate of the return to education, conditional on the logarithm of BMI, increases nearly two-fold (column 4 versus column 2) and a Hausman test indicates it is significantly different from its ordinary least squares counterpart.[27] Controlling for health leads to the same conclusions as the unconditional estimates alleviating concerns that certain instruments, like parental education, are invalid.

Turning to selection into the wage earning group, recall that only 10 percent of the total sample remain in the parental home and are in the wage labor market (wage earner-stayers). Does this introduce selection bias in the relationships estimated above? To answer this question, a multinomial logit is estimated and then the predicted hazard rate is used as a selection-correction factor in the wage equation (Lee 1983; Maddala 1983).[28] This procedure models the selection process more flexibly than a simple dichotomous framework.[29]

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[27] The additional instruments for the logarithm of BMI include indicators for the presence of a doctor and city health office in the community in 1994, as well as current prices of rice and milk.

[28] Using the predicted probability of being a wage earner-stayer, P_j, the selection correction factor $\lambda_j = (\phi(H_j)/P_j)$, where $H_j = \Phi^{-1}(P_j)$ and ϕ and Φ, represent the normal density and cumulative density functions respectively (Lee 1983).

[29] The multinomial logit implies independence of irrelevant alternatives which, while perhaps unrealistic in this setting, is assumed for tractability. Another possible approach would be to treat the decision to stay or migrate as distinct from the labor market decision and introduce two selection correction factors into the wage equation. This would require the errors in the two selection equations be uncorrelated, another unrealistic assumption.

Even though the majority of the sample has left the parental home, many have remained in the same or nearby villages. Hence, it is not only long distance migration that needs to be considered, but also the process of leaving the parental home for nearby destinations, often as a result of marriage. What, then, are the relevant choices for individuals? One obvious categorization of individuals in these data is depicted in Appendix Table 6. The three categories are (1) wage-earner-stayers, (2) nonwage-earner-stayers who may be self-employed, working in the home, or unemployed, and (3) migrants who have left the parental home. This is the first categorization considered below.

The above grouping, however, is somewhat unsatisfactory because distance of migration may be important. Of the three-fourths who have migrated, fully one-third still live within the same village. Therefore they effectively remain in the same labor market. In fact, a number of them continue to work in unpaid family agricultural occupations. Therefore, a second categorization splitting migrants by distance is considered. It includes (1) wage earner-stayers (as above), (2) nonwage earner-stayers (as above), (3) those remaining in the village but not in the parental home, and (4) long distance migrants. Separate selection models using both categorizations are estimated.

The selection model must be identified with instruments that directly influence participation, that is, being a wage earner-stayer, but only influence wages indirectly through participation. The instruments suggested above (length of residence in the community, occupation of the maternal grandfather, and 1978 wealth measures) can have ambiguous impacts. For example, under the first categorization, land ownership may raise the value of an individual remaining at home but it may also proxy the wealth necessary to finance a long distance migration (for example, if there are borrowing constraints). Of course, unambiguous sign predictions are not necessary for the instruments to be valid.

The results of the multinomial logit based on the first categorization are presented in Table 4 (those based on the second categorization are in Appendix Table 8). Since interpretation of the logit coefficients is not straightforward, the relative odds ratios (e^{β}) for the wage earner-stayers relative to nonwage earners at home (column 1) and relative to migrants (column 2) are reported. The coefficients represent the odds relative to the other category given a *one unit* change in the independent variable (Greene 1993). Thus a relative odds ratio of one indicates equal odds, while less than one means the wage earner-stayer state is less likely as that independent variable increases.

Measures of age, proxying life cycle considerations, positively impact the odds of working in the wage labor market and remaining in the household (hereafter just “odds”) relative to nonwage stayers, but negatively impact the odds relative to migration; older individuals are more likely to have left the household. Being male increases the odds relative to both alternatives. This in part reflects cultural norms since it is more common for females to move to their spouses' homes upon marriage than for males. Individuals living in rural regions are also more likely to have migrated, as indicated by the rural dummy and distance to school effects. More wealth, particularly as measured by the

Table 4 Multinomial choice: Participation and location First categorization

Dependent variable: Wage earning—migrant status

Category	Wage earner-stayers to nonwage-stayers	Wage earner-stayers to migrants
Wage equation regressors		
Years completed education	0.994	0.996
	(0.23)	(0.16)
Age (in 1994)	1.139	0.826
	(2.12)	(3.41)
Age^2	0.996	1.002
	(2.24)	(1.26)
Male	1.667	2.242
	(3.08)	(5.44)
Rural community	1.031	0.830
	(0.16)	(1.06)
Education instruments		
Distance to secondary school (kilometers)	1.030	0.916
	(0.63)	(2.09)
Distance to secondary school^2	0.998	1.002
	(0.92)	(1.40)
Private secondary school in village	0.904	1.130
	(0.39)	(0.52)
Mother's years completed education	1.054	1.039
	(1.42)	(1.14)
Father's years completed education	1.013	1.061
	(0.37)	(1.84)
Logarithm of value of house	0.908	0.925
	(2.68)	(2.48)
Own agricultural land	0.863	0.981
	(0.79)	(0.11)
Selection instruments		
Logarithm of length of residence in village	1.019	1.159
	(0.17)	(1.48)
Maternal grandfather worked in agriculture	1.213	0.717
	(1.02)	(1.93)
Chi-Square test all variables (df)	29 (14)	168 (14)
p-value	[0.0053]	[0.0000]
Chi-Square test selection instruments (df)	1.1 (2)	6.0 (2)
p-value	[0.5840]	[0.0505]

Notes: The model includes a constant. Reported coefficients are transformed into relative risk ratios (e^{β}) and represent the risk of being a wage earner-stayer relative to nonwage earner-stayers in column 1 and migrants in column 2, given a *one unit* change in the independent variable. Absolute value of asymptotic t-statistic in parentheses and p-values in square brackets.

logarithmic value of the household, is highly significant and decreases the odds of being a wage earner-stayer relative to both the other opportunities. While not individually significant, the point estimates for the measure of ties to the community, as proxied by logarithm of the length of residence, suggest that individuals from long term resident families are more likely to be wage earner-stayers.[30] Finally, the indicator of whether the maternal grandfather worked in agriculture significantly decreases the odds of being a wage earner-stayer relative to migration.[31]

Results for the second categorization described above are broadly similar with only a few exceptions (Appendix Table 8). In this specification, own education significantly increases the odds of being a wage earner-stayer relative to local migration but decreases the odds of distant migration. Males appear more likely to migrate longer distances and rural residents are more likely to migrate locally. The wealth measures, especially logarithm of household value, continue to help identify the wage earner-stayer relative to nonwage-stayers, while the "selection" instruments are strong predictors of migration behavior.

How do the implied selection terms change the wage equation and, in particular, the coefficient on education? In Table 5, four selection-corrected logarithmic wage equations are presented. Standard errors in all four specifications have been corrected

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[30] Other measures of ties to the community, including language (used by Bloom 1994) and birth places of parents, were considered, and these changed the results very little. All of them are highly correlated.

[31] As an exogeneity test, the residence and maternal grandparent occupation regressors are included in the wage equation, while instrumenting for education with distance to schooling variables only. The point estimates are close to zero and insignificant, justifying their exclusion from the education instrument sets.

Table 5 Ordinary least squares and two-stage least squares selection corrected estimates of return to education First categorization

Dependent variable: Log daily wage

	Estimation			
	OLS	Instrumental variables		
		IV-1	IV-2	IV-3
Intercept	4.08	3.22	3.59	3.43
	(15.5)	(3.99)	(5.24)	(9.38)
Years completed education[a]	**0.0654**	**0.1258**	**0.1000**	**0.1113**
	(11.7)	(3.160	(3.41)	(6.89)
Age × 100	5.309	0.486	2.550	1.644
	(4.34)	(0.130	(0.84)	(0.94)
$Age^2 \times 1{,}000$	–0.367	0.576	0.172	0.350
	(1.09)	(0.88)	(0.35)	(0.94)
Male	0.157	0.347	0.266	0.301
	(2.48)	(1.98)	(1.76)	(3.73)
Rural community	–0.100	–0.113	–0.107	–0.110
	(2.10)	(1.82)	(2.20)	(2.52)
Selection correction term (λ)	–0.589	–0.207	–0.370	–0.298
	(2.10)	(0.47)	(0.95)	(1.32)
Sample size	250	250	250	250
pseudo R^2	0.27	0.19	0.24	0.22
Hausman Test on education	-	2.4	1.4	9.2
p-value	-	[0.125]	[0.230]	[0.002]

Notes: Absolute value of asymptotic t-statistic in parentheses and p-values in square brackets. Standard errors estimated by the method of bootstrapping. Selection-Correction Term estimated using first categorization (Table 4).

[a] Education estimated as endogenous based on instrument sets IV-1, IV-2, IV-3 in columns 2, 3, and 4, respectively (see Table 2).

using the method of bootstrapping to account for the stochastic nature of the selection term and the instrumental variables estimation (Efron 1982).[32] The first column, ignoring the endogeneity of education, suggests selection is significant in levels terms and the return to education declines 10 percent (compared with 0.073 from Table 1, column 1). In the latter three specifications, which instrument for education, the selection term is no longer significant. The return to education increases more than 50 percent in these regressions. Only in the final column, with the full instrument set, are the ordinary least squares and the instrumental variables estimated returns to education, corrected for selection, significantly different (see Hausman test), and the exogeneity of education is rejected. The same conclusions follow when selection correction is based on the second categorization for the multinomial logit (see Appendix Table 9).

Taken together, all the above findings provide strong evidence that the return to education estimated with ordinary least squares is downward biased, though the extent of that bias depends on whether one controls for other types of human capital and for selection into the wage earner-stayer sample. How much of this difference can be attributed to measurement error? To explore this issue, the multiple measures of education available in the survey need to be examined. Using the correlation coefficient of mother's (father's) education between the 1983 and 1994 surveys and assuming

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[32] The first-stage multinomial logit is repeated 500 times based on random samples with replacement and the corresponding hazard rate calculated. Each time the second stage is estimated using the fixed sample of wage-earner stayers via two-stage least squares and the standard errors calculated. Tables report the average standard errors from this process.

measurement error is uncorrelated over time, the reliability ratio is estimated at 85 percent (83 percent). A similar exercise for those children who had completed schooling before 1983 (not in the sample used here) suggests an even higher reliability ratio of nearly 90 percent.[33] Adjusting the reliability ratio to account for the presence of right hand side regressors also correlated with education, conservative estimates of reliability ratios are closer to 80 percent, which would imply a 25 percent difference between ordinary least squares and instrumental variable estimates. Random measurement error alone does not fully explain the differences estimated above.

AN INTERPRETATION

Upon finding high returns using college proximity as an instrument for education in the wage equation, Card (1993) offers an explanation based on individual heterogeneity in the return to education (discussed further in Card 1994). The model is the following,

$$\omega_i = x_i' \gamma + S_i \delta_i + \epsilon_i, \qquad \textbf{(2a)}$$

which can be rewritten as

$$\omega_i = x_i' \gamma + S_i \delta + S_i (\delta_i - \delta) + \epsilon_i. \qquad \textbf{(2b)}$$

[33] In the presence of correlated measurement error, however, these estimates would be overstated.

Of course, as depicted, the model is inestimable. However, if one estimates equation (1a) while the underlying true model is (2a), the estimate of the return to education, δ, represents an average of the individual returns (see, also, Angrist and Imbens 1995).

How does this help explain the increase in the instrumental variable versus ordinary least squares estimates? Suppose that rather than individual heterogeneity, there is group heterogeneity of returns for groups 1,.. G, so that the return to education for group g is δ_g. If one uses a binary instrumental variable (as in Card 1993) and it affects only one group, then the probability limit of the instrumental variable estimator is δ_g. With multiple valued instruments, the story is more complicated but the intuitive results are similar. As an example, Card suggests discount rate heterogeneity that causes individuals to stop schooling at different points (according to their individual optimizing calculus). For example, if returns to schooling declined with more schooling, one would expect those with higher discount rates (due to credit constraints, for example) to stop schooling at an earlier point. The instrumental variable "experiment" of distance to schooling might pick up those individuals who stopped schooling earlier and had a higher than average marginal return. The results presented in this paper are consistent with such an interpretation.

5. CONCLUSIONS

The goal of this paper has been to understand the effect of investment in education as measured by private returns in the labor market. After presentation of evidence that distance to secondary school is appropriately considered exogenous to the wage equation and is therefore a valid instrument, the results indicate the estimated returns to education are significantly downward biased when the endogeneity of education is ignored. Returns increase more than 60 percent when education is endogenized. This finding is robust when health status and controls for the process of selecting individuals into the sample are included. Although important, conventional measurement error alone explains only one-third of the bias. In closing, the paper suggests how heterogeneous returns to education might account for the magnitude of the downward bias in returns to schooling.

To link these findings to policy prescriptions, a connection between private and social returns needs to be made. Standard procedures for calculating the latter typically take the cost-benefit approach (described in footnote 1) and include a measure of government expenditure (Psacharopoulos 1985 and 1994). They are therefore unambiguously lower than private returns calculated by the same method. Since the private returns estimated by these methods (which ignore the endogeneity of education) closely mimic those found in the wage equation literature, it stands to reason that estimates of social returns are also commensurately higher.

APPENDIX

Table 6 Sample means

	Category		
	Wage earner-stayers	Nonwage-stayers	Migrants
Individual characteristics			
Age (in 1994)	27.3	27.8	32.0
	(5.9)	(6.7)	(6.7)
Percent aged 20-24	40.0	42.8	15.9
25-29	32.4	22.0	23.0
30-34	14.0	17.4	23.7
35-39	8.0	9.4	21.0
40-44	5.6	8.4	16.4
Years completed education	8.9	9.0	8.5
	(3.5)	(3.5)	(3.2)
Years completed postsecondary education (beyond 10 years)	1.0	1.0	0.7
	(1.8)	(1.7)	(1.5)
Percent with some postsecondary education	25.2	29.4	19.8
Percent enrolled in school in 1982	57.2	56.0	28.7
Percent male	66.4	54.9	49.4
Percent married	25.2	35.5	79.3
Household and parental characteristics			
Mother's years completed education	6.4	6.0	5.2
	(3.1)	(3.2)	(2.8)
Father's years completed education	6.5	6.5	5.3
	(3.2)	(3.3)	(3.0)
Logarithm of value of house in 1978	6.0	6.5	6.4
	(2.4)	(2.3)	(2.0)
Percent with agricultural land	28.4	33.5	33.6
Percent with irrigation	7.2	11.7	9.6
Percent maternal grandfathers worked in agriculture	72.4	43.2	49.7
Years in residence in 1978	16.4	17.4	19.1
	(9.6)	(10.4)	(10.4)
Percent residing in rural community	66.8	66.7	76.1
Distance from house to nearest primary school in 1983 (kilometers)	0.5	0.6	0.7
	(0.8)	(0.7)	(1.0)
Distance from house to nearest secondary school in 1983 (kilometers)	2.5	2.8	3.3
	(3.4)	(4.4)	(4.3)
Percent with private secondary school in community in 1983	13.2	13.8	9.5
Sample size	250	477	1,925
Percent total sample	9.4	18.0	72.6

Note: Standard deviations in parentheses.

Table 7 Estimates of return to education, controlling for health

Dependent variable: Log daily wage

	Estimation/specification			
	Ordinary least squares		Instrumental variables	
	No BMI	BMI	No BMI[a]	BMI[b]
Intercept	3.16	0.52	2.69	5.55
	(9.28)	(0.46)	(6.68)	(1.06)
Years completed education	**0.0603**	**0.0626**	**0.1307**	**0.1434**
	(3.51)	(3.71)	(4.00)	(3.36)
Age × 100	5.457	4.349	1.278	1.613
	(1.22)	(0.98)	(0.25)	(0.29)
Age^2 × 1,000	-0.917	-0.636	0.211	0.141
	(0.64)	(0.45)	(0.13)	(0.08)
Male	0.264	0.260	0.356	0.380
	(2.17)	(2.18)	(2.66)	(2.48)
Rural community	-0.082	-0.074	0.021	0.035
	(0.68)	(0.63)	(0.15)	(0.23)
Logarithm of body mass index	-	0.891	-	-1.000
		(2.42)		(0.55)
Sample size	135	135	135	135
R^2 (pseudo R^2 in columns 3 & 4)	0.21	0.24	0.22	0.19
F-test all variables	6.9	7.9	7.1	5.0
F-test experience variables	4.0	3.2	1.6	1.5
Overidentification test	-	-	1.0	0.9
p-value			0.42	0.45
Hausman Test education	-	-	3.6	4.2
p-value			[0.06]	[0.04]
Hausman Test BMI	-	-	-	1.1
p-value				[0.29]
Hausman Test on education and BMI	-	-	-	4.3
p-value				[0.12]

Notes: Absolute value of t-statistic in parentheses and p-values in square brackets.

[a] Education estimated as endogenous based on instrument set IV-3 (see Table 2).

[b] Education and ln BMI estimated as endogenous, based on instrument set IV-3 (see Table 2), community prices for rice and milk, and indicators of the presence of a doctor and city health office in the community.

Table 8 Multinomial choice: Participation and location Second categorization

Dependent variable: Wage earning—Migrant status

	Wage earner-stayers to nonwage-stayers	Wage earner-stayers to local migrants	Wage earner-stayers to distant migrants
Wage equation regressors			
Years completed education	0.993	1.106	0.963
	(0.24)	(3.41)	(1.45)
Age (in 1994)	1.139	0.724	0.860
	(2.12)	(4.89)	(2.66)
Age^2	0.996	1.005	1.001
	(2.24)	(2.66)	(0.75)
Male	1.664	1.842	2.377
	(3.08)	(3.64)	(5.76)
Rural community	1.032	0.650	0.893
	(0.16)	(2.06)	(0.64)
Education instruments			
Distance to secondary school (kilometers)	1.030	0.925	0.914
	(0.63)	(1.68)	(2.10)
Distance to secondary school^2	0.998	1.002	1.003
	(0.92)	(1.11)	(1.43)
Private secondary school in village	0.905	1.474	1.071
	(0.39)	(1.330	(0.29)
Mother's years completed education	1.055	1.014	1.049
	(1.43)	(0.35)	(1.39)
Father's years completed education	1.013	1.082	1.054
	(0.37)	(2.14)	(1.62)
Logarithm of value of house	0.908	0.874	0.937
	(2.68)	(3.36)	(2.06)
Own agricultural land	0.863	0.824	1.045
	(0.79)	(1.03)	(0.26)
Selection instruments			
Log of length of residence in village	1.020	1.259	1.132
	(0.17)	(2.03)	(1.21)
Maternal grandfather worked in agriculture	1.215	0.732	0.717
	(1.03)	(1.530	(1.89)
Chi-square test all variables (df)	29 (14)	201 (14)	142 (14)
p-value	[0.0106]	[0.0000]	[0.0000]
Chi-square test selection instruments (df)	1.1 (2)	6.6 (2)	5.1 (3)
p-value	[0.5788]	[0.0365]	[0.0781]

Notes: The model includes a constant. Reported coefficients are transformed into relative ratios (e^{β}) and represent the risk of being a wage earner-stayer relative to nonwage earner-stayers in column 1, local migrants in column 2, and distant migrants in column 3, given a *one unit* change in the independent variable. Absolute value of asymptotic t-statistic in parentheses and p-values in square brackets.

Table 9 Ordinary least squares and two-stage least squares selection corrected estimates or return to education Second categorization

Dependent variable: Log daily wages

	Estimation			
-	OLS	Instrumental variables		
		IV-1	IV-2	IV-3
Intercept	4.12	3.34	3.70	3.50
	(15.6)	(4.140	(5.41)	(9.57)
Years completed education[a]	**0.0654**	**0.1213**	**0.0953**	**0.1099**
	(11.7)	(3.05)	(3.25)	(6.81)
Age × 100	5.403	1.010	3.057	1.911
	(4.420	(2.74)	(1.01)	(1.09)
Age^2 × 1,000	-0.367	0.509	0.101	0.329
	(1.09)	(0.78)	(0.20)	(0.86)
Male	0.148	0.320	0.240	0.284
	(2.340	(1.83)	(1.59)	(3.52)
Rural community	-0.096	-1.047	-0.101	-0.103
	(2.03)	(1.68)	(2.07)	(2.36)
Selection correction term (λ)	-0.615	-0.281	-0.437	-0.350
	(3.37)	(0.63)	(1.12)	(1.54)
Sample size	250	250	250	250
pseudo R^2	0.27	0.20	0.25	0.23
Hausman test on education	-	2.31	1.28	8.75
p-value		[0.128]	[0.258]	[0.003]

Notes: Absolute value of asymptotic t-statistic in parentheses and p-values in square brackets. Standard errors estimated by the method of bootstrap. Selection correction term estimated using second categorization (Table 8).

[a] Education estimated as endogenous based on instrument sets IV-1, IV-2, IV-3 in columns 2, 3, and 4, respectively (see Table 2).

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