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Household Income and Child Schooling in Vietnam

Jere R. Behrman and James C. Knowles

The stronger are the associations between household income and child schooling, the lower is intergenerational social mobility and the less equal is opportunity. This study estimates the associations between household income and children's school success in Vietnam. The estimates indicate that these associations are considerable. For example, the income elasticity of completed grades is five times the median estimate of earlier studies. Moreover, this association is strongest for grades completed per year of school, not *for completed grades, on which most of the previous literature has focused. There are some gender differences, the most important being a smaller association between income and grades completed per year of school for boys than for girls. This difference implies that schooling of girls is treated as more of a luxury (less of a necessity) than is schooling of boys.*

This article also investigates some ways in which policies relate to household incomes. School fees are progressive, but school fees are only about one-third of what households pay directly to schools and are a much smaller proportion of a household's total school-related expenditures. Total household expenditures paid directly to schools increase with household income less proportionately than do school fees alone, so the overall structure of such payments is less progressive than is the structure of school fees. Because school enrollment is positively related to household income, moreover, the structure of school fees is less progressive for the entire population than for the selected subset that has children enrolled in school. Further, the two school quality measures that have the strongest positive association with children's school success are much more available to higher-income households, meaning that higher-income households have greater school expenditures in part because they are obtaining higher-quality schooling and not because charges for the same quality schooling are progressive across income classes.

Schooling is widely seen as critical to the development process and poverty alleviation. Recent studies confirm that schooling is particularly important when complex new technologies and market options become available (for example, Rosenzweig 1995). Recently, many countries, including Vietnam, have under-

Jere R. Behrman is with the Department of Economics at the University of Pennsylvania, and James C. Knowles is an economic consultant based in Chapel Hill, North Carolina. The analysis that led to this paper was initiated with funding from the Asian Development Bank, through Contract COCS/95-000 under the Financing of Social Services Project: Vietnam (T.A. 2135-VIE) directed by Nguyen Van Tien of the General Statistical Office, Government of Vietnam. Behrman's subsequent input was funded by NIH 5-RO1-HD30907, NIH 1 RO1-AG-11725-01, and NSF SBR95-11955. The authors benefited from discussions with Benjamin E. Diokno from the University of the Philippines, who also worked on the project, and from the comments of Harold Alderman, three anonymous referees, and the editor.

gone considerable macroeconomic stabilization and market liberalization programs. The returns to schooling will probably increase following such programs. Therefore decisions about who is schooled now are likely to be critical in determining Vietnam's future economic growth and distribution of income.

A rising concern for many in Vietnam and other developing countries has been the possibility of greater inequality and reduced intergenerational social mobility under these economic reforms. Part of this concern is that family "dynasties" will be reinforced if children from higher-income households are more likely to receive more and better schooling, and thus reap greater gains from schooling in the future than children from lower-income households. Two different societies with the same income distribution at a point in time may be viewed as having different levels of social welfare if they have different degrees of social mobility. For example, Friedman (1962) argues that a given extent of income inequality that arises in a rigid system in which each family stays in the same position each period may be a cause for more concern than the same degree of income inequality that arises in a fluid system because of the great mobility and dynamic change associated with equality of opportunity.

Because of the concern that schooling could perpetuate social immobility and inequality, the recent policy-related literature has considered targeting public school resources toward children from poorer families (van de Walle and Nead 1995 provide examples and references). The concerns in Vietnam have been about whether family dynasties are becoming more powerful and whether schooling is targeted toward children from poorer households or if it is instead reinforcing the advantages of children from better-off households. Educational reforms have exacerbated these concerns (see World Bank 1996). The reforms are intended to make schools more efficient, but some of their components (such as the introduction of user charges) may affect children differently depending on their household income.

I. Indicators of School Progress

We examine four indicators of individual school progress for children ages 6–17 by income quintile in Vietnam: age when started school, grades passed per year of school, last completed grade, and exam score in last completed grade (table 1). Each of these indicators captures important and different dimensions of schooling from the point of view of individual children and their families.

Schooling in Vietnam

For a given extent of schooling the younger children are when they start school, the sooner they reap post-schooling returns and the longer they have to reap such returns. A few recent studies have emphasized the possible importance of the age when students start school (for example, Alderman, Behrman, Lavy, and Menon 1997; Glewwe and Jacoby 1995a; and Glewwe, Jacoby, and King 1998). In Vietnam this age is inversely associated with income: children from households in the

Table 1. *Means of Individual School Progress Indicators by Predicted Income per Household Member for Children Ages 6–17 in Vietnam, 1996*

	Income quintile				
Indicator	*1 (poorest)*	*2*	*3*	*4*	*5*
Age when started school	6.7	6.6	6.4	6.3	6.2
	(1.3)	(1.1)	(1.1)	(0.7)	(0.6)
	[2.8]*	[2.3]*		[1.3]	[6.1]*
Grades passed per year of school	0.81	0.85	0.87	0.90	0.95
	(0.20)	(0.20)	(0.19)	(0.19)	(0.12)
	[5.0]*	[1.7]**		[2.0]*	[8.8]*
Last completed grade	4.1	4.2	4.3	4.7	6.0
	(2.8)	(2.7)	(2.7)	(2.9)	(3.2)
	[4.1]*	[1.5]		[3.6]*	[11.5]*
Exam score in last completed grade	5.9	6.0	6.2	6.4	6.7
	(1.2)	(1.3)	(1.2)	(1.3)	(1.4)
	[2.6]*	[2.6]*		[2.2]*	[6.8]*

* Significant at the 5 percent level.

** Significant at the 10 percent level.

Note: Standard deviations are in parentheses, and absolute value of *t*-statistics for differences from the third quintile are in square brackets.

Source: Calculated from data from the 1996 Vietnam Social Sector Financing Survey.

first and second income quintiles (the poorest income quintiles) are significantly older, and children from households in the fifth quintile are significantly younger, than children in the third quintile (table 1). On average, children in the first quintile start school when they are half a year older than children in the fifth quintile.

For a given age at which a child starts school and a given extent of schooling, the more rapidly that child completes his or her schooling, the lower is the private cost of schooling in both direct monetary costs and opportunity costs, the sooner are post-schooling returns reaped, and the longer is the period in which to earn these returns. Grades passed per year of school are positively associated with income. The means for the first two quintiles are significantly below the mean for the third quintile, and the means for the fourth and fifth quintiles are significantly above that for the third quintile. On average, children from the first quintile pass about eight grades in a decade of attending school, while children from the fourth and fifth quintiles pass nine or more grades in a decade.

The last completed grade of schooling is the most emphasized indicator of individual school success in the literature—in fact, in most studies of the determinants and impact of schooling, it is the only indicator used. The last completed grade at the time of the survey is positively associated with income, with the mean for the first quintile significantly below, and the means for the fourth and fifth quintiles significantly above, the mean for the third quintile. On average, children from the fifth quintile had completed almost two more grades than children from the first quintile at the time of the survey.

Children who have completed the same number of grades may perform differently on examinations designed to measure how much they have learned. A few recent studies that have examined this question for developing countries report that cognitive test scores have an explanatory power beyond that of completed grades for estimated labor income relations (Alderman and others 1996b; Glewwe 1996; Knight and Sabot 1990; Lavy, Spratt, and Leboucher 1997; and Moll 1996). Examination scores conditional on the last completed grade of schooling are significantly and positively associated with household income in Vietnam; on average, children from the first two quintiles score significantly lower, and children from the top two quintiles score significantly higher, than children from the third quintile. Children from the fifth quintile score about 17 percent higher than children from the first quintile.

Thus the data suggest that in Vietnam there are systematic associations between important aspects of children's progress in school and household income: children from higher-income households do better in school according to each of the four indicators. These associations raise some important questions. Are such associations large or small? How do they differ among our four indicators? Are they largest for grades completed, as the emphasis in the literature might suggest? How sensitive are the estimated associations to details of their estimation, such as the definition of income and the treatment of students who are still in school? Do the associations differ for girls and boys? To what extent are the associations mitigated or reinforced by education policies? Is the structure of school fees progressive? If so, is this progressivity reinforced or offset by a household's other school-related expenditures? Is it reinforced or offset by the quality of schools attended by children from different segments of the income distribution?

Results of Past Research

Researchers have conducted numerous studies of associations between indicators of household income and schooling for other countries, although we are not aware of such studies for Vietnam. In Behrman and Knowles (1997) we review 42 studies, covering 21 countries (these are summarized in appendix A). Estimates for about three-fifths of the schooling indicators used in these studies show significant associations between household income and schooling. Of the cases for which we can estimate income elasticities, the median elasticity is 0.07. This number suggests that children from higher-income households do better in school than children from poorer households, although the magnitude of the effect is small. The estimates tend to be higher for samples with poorer households, and a number of the studies find small inverse associations between schooling and income. The largest elasticity estimates—those higher than 0.20—are for low-income regions (low-income during the period of the survey): Côte d'Ivoire, Ghana, Nepal, Taiwan (China) for the 1940–49 birth cohort, Northeastern Brazil, and rural Pakistan. But these are the only cases in which the estimates exceed 0.20.

It is not clear how informative these results are for Vietnam because they are from policy and market environments that are much different than those in Viet-

nam, where there has long been concern about equality of schooling. Moreover, most of this literature suffers from at least one of several limitations.

First, the income measures used in most of these studies are probably contaminated by measurement error as well as by the possibility that schooling is endogenous with other decision variables. If there is random measurement error in the income indicator, as would be the case if annual income is used when the relevant household resource constraint is really longer-run income, the estimated association between income and schooling would be biased toward zero. And if households make income and expenditure decisions simultaneously with schooling decisions, there may be a bias toward or away from zero, depending in part on which income measure is used. It may be away from zero, for example, if total income is used and households lower their total income (and possibly their expenditures) when they have school-age children by reducing child labor in order to increase schooling. But even if total income falls, expenditures may rise through dissaving to cover schooling costs, possibly generating the opposite bias. Likewise, the bias may be toward zero if adult (household head, father) income is the most important component of household income and if parents increase their work efforts and income to finance their children's schooling.

Second, studies may mask the true association between schooling and income because their estimations generally include a number of other household, community, and school controls that are correlated with income and may be proxying in part for the income association. To estimate multivariate causal relations and avoid omitted variable bias in the income coefficient estimate, it is important to control for all of the factors correlated with income that the underlying model suggests determine schooling and are predetermined with respect to income. Studies may claim to be estimating causal relations but in fact probably do not succeed in isolating the causal impact of income because of omitted variable biases and other problems. For example, innate ability may affect schooling investments in children, as is found in the studies summarized in Behrman and Rosenzweig (1999), may be correlated across generations through genetic endowments, and may be correlated with parental income, resulting in omitted variable bias. But to examine the extent of the associations between child schooling and parental household income, we do not want to control other characteristics that are correlated with income because the true association with income is likely to be misunderstood if such characteristics are included.

Third, most of the previous studies rely on only one schooling indicator, usually students' amount of time in school or their grade attainment. This narrow focus misses the possibility that there may be separate and important associations between income, on the one hand, and the age of starting school, grades passed per year in school, and performance on examinations in the last completed grade, on the other.

Fourth, in their estimates of associations between schooling and income, many studies that use data on school-age children do not control for the fact that, at the time the survey was conducted, some school-age children may not have started

school yet and others may not have completed school yet, or for other aspects of sample censoring. Because of sample censoring, data summaries, such as that in table 1, simple graphs, or other nonparametric summaries of the data may not reveal the extent of the associations with income.

Fifth, most of the existing studies do not explore whether there are important gender differences in associations between income and schooling, although other aspects of gender differences in schooling have received considerable attention. Finally, most studies do not address how education policies might be related to household income—for example, through targeted policies for school fees and a household's other school-related expenditures or through differences in school quality that may offset or reinforce differences in household income.

A few studies have addressed one or more of these limitations. For example, to address the limitation regarding the use of current annual income or expenditures, Glewwe and Jacoby (1995a, 1995b), Montgomery and Kouame (1993), and Tansel (1997) instrument household expenditures, and Alderman and others (1996a) instrument household income. With regard to the limitation of focusing only on completed schooling, Alderman Behrman, Lavy, and Menon (1997), Glewwe and Jacoby (1995a), and Glewwe, Jacoby, and King (1998) explore the relationship between income and the age of starting school, and Alderman and others (1996a, 1996b) and Alderman, Behrman, Khan, Rose, and Sabot (1997) investigate the relationship between income and cognitive achievement. With regard to the limitation of censored data, King and Lillard (1987) and Alderman and others (1996a, 1996b) control for right-censoring in completed years of schooling. Still, these examples and other studies that address one or a few of these limitations are a small proportion of the literature.

Our contribution in this paper is to explore the associations between household income and the four indicators of school progress for Vietnam. We use 1996 data from a cross-sectional household survey linked with community and schooling surveys that we conducted in collaboration with the Vietnamese General Statistical Office. We move beyond the previous literature by addressing the six limitations summarized above. Through this investigation we illuminate the extent and nature of associations between household income and schooling and related policies in the particular case of Vietnam, and we raise questions concerning what is known about such associations in other countries.

II. The Analytical Framework

We begin with a brief discussion of why schooling might be associated with household income. This discussion points to a number of possible reasons, as well as to the difficulty of disentangling association and causality from cross-sectional data and of determining whether such associations may reflect underlying inefficiencies.

If there were no unobserved differences between low- and high-income households, if schooling were purely an investment (with no current consumption as-

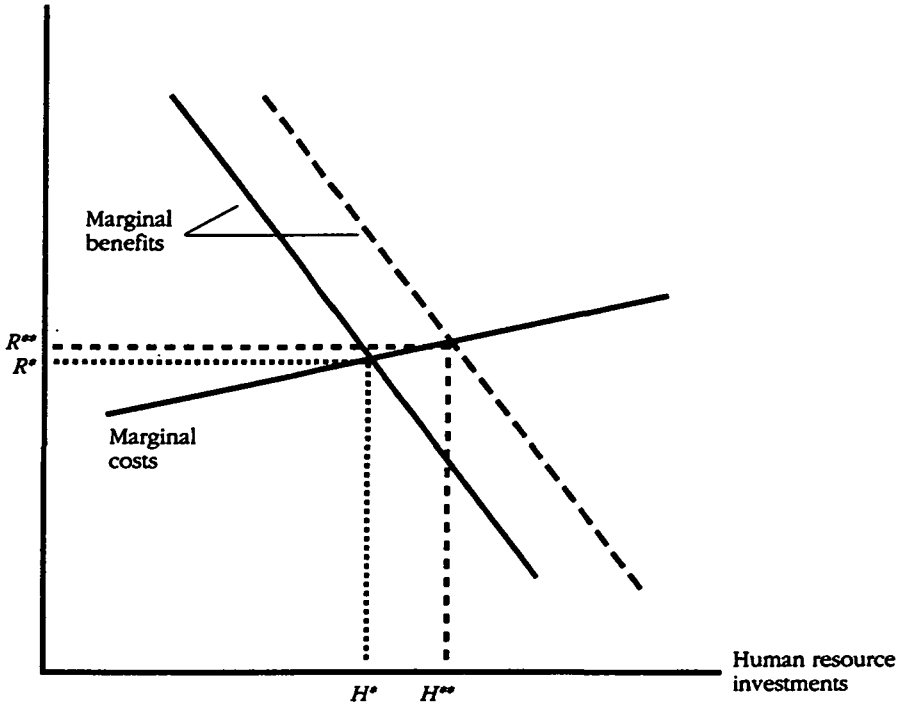
pects), if markets worked perfectly, and if the same prices prevailed in all markets, there would be no differences in schooling investments associated with income once controlling for any observed differences in household characteristics. Therefore it is useful to determine why there might be associations between household income and investments in schooling. The general reasons are that household income is proxying for correlated unobserved determinants of child schooling, such as innate ability, preferences, and family connections; household income is proxying for price variations in school inputs; and household income is playing a causal role in the presence of imperfect markets.

In addition to the investment aspect of schooling, spending time in school may be a current consumption activity that is associated with household income. We emphasize *current* consumption, such as spending time in school rather than spending time at home playing or watching television. Schooling may also affect future consumption (for example, by enriching reading as an adult), but because these effects are obtained in the future, current schooling for such purposes is an investment. If the current consumption of schooling has aspects that are normal goods, ceteris paribus, more household income leads to more schooling for that reason alone.

The relationship between schooling as an investment and household income is multifaceted and more complicated than the relationship between schooling as current consumption and household income. Becker's (1967) Woytinsky lecture on the determinants of human capital investments is a useful starting point for thinking in more detail about possible associations between parental household income and schooling investments. Within this framework schooling investments are made until the private marginal benefits of the investment equal its private marginal costs. Marginal private benefits (the solid benefits curve in figure 1) depend on the expected private gains (such as in wages or salaries or in enriched future consumption) from the human capital investment. The marginal private benefits curve is downward-sloping because of diminishing returns to schooling investments. We might expect diminishing marginal returns (at least at sufficiently high levels of investment) because a given individual has fixed genetic endowments (such as innate ability) and because human capital investments, such as those that extend the time in school, imply greater lags before obtaining postinvestment returns and a shorter postinvestment period in which to reap the returns. Marginal private costs may increase with human resource investments because of increasing opportunity costs of devoting more time to such investments and because of increasing marginal private costs of borrowing on financial markets. The private returns net of costs are maximized where private marginal benefits equal private marginal costs (H^* in figure 1).

If all markets function perfectly, there are no government interventions, and schooling is only an investment, then everyone invests in schooling until the expected rate of return from schooling equals the expected rate of return on alternative investments (at H^*), regardless of household income. In this case there are no or very few channels through which income may be associated with schooling

Figure 1. *Private Marginal Benefits and Private Marginal Costs of Human Resource Investments, with Marginal Benefits Dependent on Income*



(although there still may be some possibilities). But given the range of real-world market imperfections and government interventions, there are many reasons why household income may be associated with schooling, even if schooling is purely an investment.

To illustrate, consider what would happen in the presence of market imperfections. There are several explanations, originating in both policy and market failures (as well as reasons that would persist with perfect markets), why household income may be related to the marginal private benefits and costs of schooling investments and thus to schooling investments themselves. Current consumption effects could also generate associations between income and schooling (with the sign depending on the nature of the consumption effects). Some of these reasons reflect inefficiencies, such as those due to imperfect credit and information markets. Others reflect differing abilities that complement human capital investments or differing prices that are related to household income in different areas given positive transportation costs. Some reflect causal effects of income, such as current consumption demands. And some reflect associations with other variables, such as abilities that are correlated with income and transferred in part intergen-

erationally. With cross-sectional data of the types that are usually available, the relevance of many of these possibilities and the effect of causality compared with association cannot be sorted out conclusively.

A priori it would seem that market failures would be relatively common in Vietnam because of the country's low level of development, which was, until recently, exacerbated by the pervasiveness of relatively centralized command policies for many allocations. As a result, then, schooling investments may be more positively associated with household income in Vietnam than in most societies. However, at least the rhetoric of policy discussions in Vietnam has held that school policies should and do promote relative economic equality and reduce poverty. If the reality reflects the rhetoric, the policy effects would tend to work in the opposite direction.

Marginal Private Benefits Associated with Income

Figure 1 illustrates what happens when marginal private benefits for human capital are associated with household income. Each of the two curves depicted depends on a different income level ceteris paribus. If the (otherwise identical) individual is in the household whose income yields the dashed curve, the private incentives are to invest at H^{**}, which is higher than H^{*}. Why might marginal private benefits of schooling be associated with household income in the presence of government policies or market imperfections? There are several reasons.

First, public policies may affect households with different incomes differently. Policies may favor higher-income households by offering them higher-quality (or more accessible) schooling in response to their greater economic and political power or because prices of some important school inputs may be lower in areas where incomes are higher (for example, teachers may prefer to live and teach in high-income areas and be willing to do so at lower salaries than they would require in low-income areas). If school quality is positively associated with household schooling investments, the dashed marginal private benefits line would represent higher-income households. However, policies may favor poorer households if programs are designed to reduce inequality or to alleviate poverty by allocating better schooling to poorer households or if prices of some school inputs are lower in low-income areas (for example, land for schools). In such cases, if school quality is positively associated with household investments in education, the dashed marginal private benefits line would represent lower-income households.

Second, households may invest in children's education at home directly through tutoring or indirectly through improvements in their health and nutrition. If markets for these investments (or for financing these investments) are imperfect and the costs are lower for wealthier households, the marginal private benefits of schooling will be higher for wealthier households. For instance, the cost of helping with homework may be less for more-schooled parents than for less-schooled parents, and parental schooling is likely to be positively correlated with household income.

Third, children's genetic endowments, for which there are no perfect markets (marriage markets probably serve indirectly as imperfect markets for such endowments), may interact with schooling investments and be correlated with parental endowments that, in turn, are correlated with household income. These relationships arise because such endowments affect income directly and indirectly through parents' human capital stock, including their education. Behrman, Rosenzweig, and Taubman (1994, 1996) present evidence, using data on twins, that schooling investments respond positively to children's genetic endowments in the United States. Behrman and Taubman (1989) present estimates that variations in such endowments are consistent with most of the variance in child schooling for young adults in the United States. The enormous literature on the associations between adults' schooling and their household earnings is surveyed in Psacharopoulos (1994) and Rosenzweig (1995).

Fourth, households may make complementary investments in searching for a job and have contacts that affect their children's job search after completing schooling. If markets for financing such investments are imperfect and the costs are lower for higher-income households, in part because of more attractive possibilities for working in family enterprises and better connections for other employment opportunities, the marginal private benefits would again be higher for such households.

Fifth, higher-income households may have better information (in part because of better family enterprise options and better connections), given imperfect markets for information. As a result, they face less uncertainty about schooling investment decisions and, assuming constant risk aversion, therefore have higher expected marginal private benefits than poorer households.

Sixth, higher-income households may have lower risk aversion. Therefore in the presence of imperfect insurance markets or insurance with positive private costs, their private incentives would be to invest more in schooling than otherwise identical lower-income households.

And lastly, higher-income households may be better able to deal with stochastic events. For example, through their connections (perhaps facilitated by income transfers, including bribes), they may be better able to offset their children's bad performance on admissions examinations than poorer households can. They therefore have private incentives to invest more in schooling than otherwise identical lower-income households.

The first possibility (involving public policies) relates to endogenous policy choices, which, depending on the mechanism, could favor either higher- or lower-income households (see, for example, Rosenzweig and Wolpin 1986). In the other six cases higher-income households have private incentives to invest more in the schooling of otherwise equal children because they cope better with market imperfections, or higher-income households have unobserved characteristics that increase schooling investments and are associated with household income.

Marginal Private Costs Associated with Income

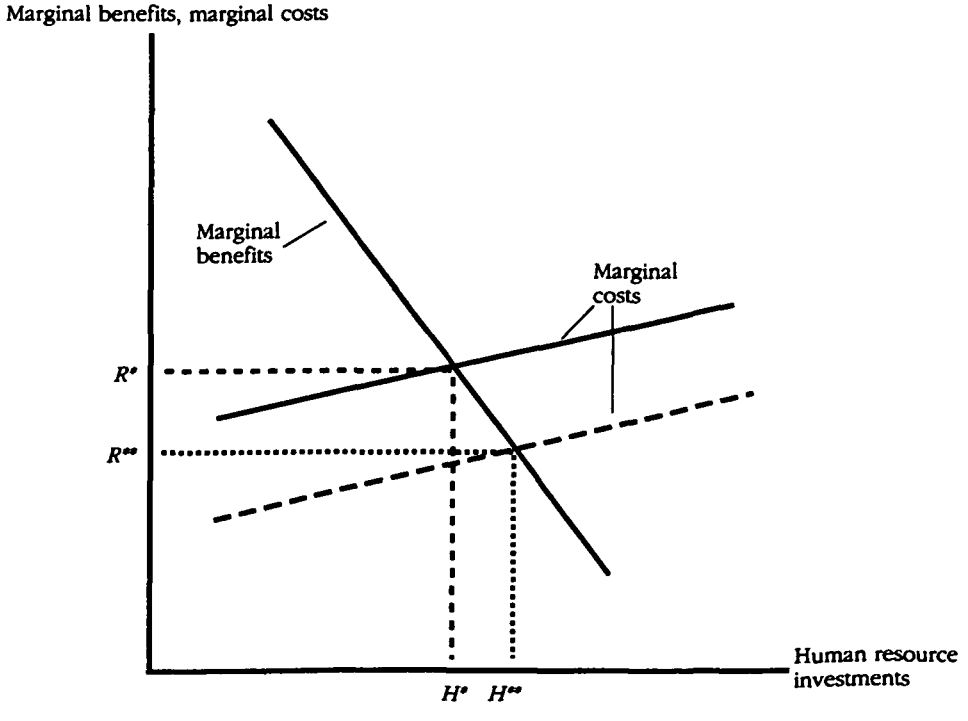
Figure 2 represents two different marginal private cost schedules for schooling investments that depend on household income. With the solid marginal cost curve, the private incentives are to invest at H^*, which is less than the privately optimal level of human capital investment for the dashed marginal cost curve, H^{**}. Why might marginal private costs for human capital investments be associated with household income in the presence of market imperfections? There are two reasons.

Because of capital market imperfections, particularly for human capital investments (in part because human capital is not recognized as collateral), the marginal private costs for such investments are particularly high for individuals from poorer families who cannot as easily finance these investments themselves. In this case the dashed line represents a household with higher income. In another case children from poorer households may be exempt from paying school fees, so that, ceteris paribus, the dashed line represents a poorer household.

III. DATA

Our principal data source is the Vietnam Social Sector Financing Survey (VNSSFS), which we and Vietnam's General Statistical Office conducted in 1996 with fund-

Figure 2. *Private Marginal Benefits and Private Marginal Costs of Human Resource Investments, with Marginal Costs Dependent on Income*



ing from the Asian Development Bank. We conducted the VNSSFS in seven provinces, one from each of the country's administrative regions. The data set includes a survey of 1,905 households and a series of commune and school surveys. The VNSSFS is one module within a larger multiround survey that the General Statistical Office is carrying out called the Multi-Objective Household Survey (MOHS). The MOHS is an ongoing survey covering 45,000 households in 1,500 communes located in all 53 provinces of Vietnam. It collects, among other data, information on income and expenditures. The income and expenditure data that we use were collected retrospectively for the previous year (subsequent data were not available to us).

We used a subsample of communes surveyed in the MOHS (we chose three communes from each of three districts in each of seven sample provinces—a total of 63 communes, 19 urban and 44 rural). We administered the VNSSFS to the same 30 households in each sample commune that participated in the MOHS. In addition, we administered a community questionnaire to the Commune People's Committee Chairman in each commune and facility questionnaires to schools (and to other social sector facilities). Households were surveyed by the commune-level MOHS interviewers, who were residents of each commune, and the facility interviews were conducted by commune-level supervisors who were part of the supervisory staff of the MOHS.

The VNSSFS household questionnaire collected information on the use and financing of schooling. For all children in the household questions on schooling included the age they started school, whether they were currently enrolled, the age they last attended school if they were not currently enrolled, the last grade they attended (current grade if currently enrolled), and their last comprehensive examination score conditional on the grade in which the examination was taken (see table 1 for individual school progress indicators). For children currently enrolled in school, we obtained data on the household's school-related expenditures (and exemptions). The units of observation used in this study are primarily the 2,789 children in the 6–17 age range from the 1,844 sample households on whom we had complete data. Usable income data were not available for 3.2 percent of the households. This is a small percentage compared with many samples (for example, in recent years item nonresponses on earnings in the U.S. Current Population Survey have exceeded 20 percent). Of the 2,789 children, 2,203 (79 percent) were enrolled in school at the time of the survey.

Within the 6–12 age range there is some age-related variation in school enrollment and thus in data related to enrollment: among the 1,373 children in the 6–11 age range, 94 percent were enrolled at the time of the survey, while among the 1,416 children in the 12–17 age range, 65 percent were enrolled. Therefore, in addition to estimates based on the full sample, we present estimates that allow the parameters to differ for these two age groups. Because some households in the sample have more than one child, the regression estimates of the associations between schooling and income use the Huber correction for clustering at the household level.

Information on annual household income, expenditures, and assets is available from the MOHS. We merged these data with the VNSSFS household data. To characterize income for this study, we considered four alternative measures: annual household income per household member, annual household expenditures per household member, predicted household income per household member, and predicted household expenditures per household member (table B-1 gives the relations used to construct predicted income and predicted expenditures on the basis of longer-run characteristics). The mean annual income per household member by the income measures is 2.076 million Vietnam dong (D), which translates into 188 U.S. dollars ($) at 1996 exchange rates (table 2). The mean annual expenditures are D1.728 million or $156. The standard deviations are about a third larger for the two income measures than for the parallel expenditure measures. This difference is consistent with the theory that transitory income fluctuations are smoothed somewhat over time so that there is greater measurement error in letting income, rather than expenditures, represent a longer-run household resource constraint.

These standard deviations imply that the distributions of income and expenditures are more equal in Vietnam than in many societies, as is also reported in other studies. World Bank (1995b, table 30), for example, gives the percentage share of consumption by quintile for 22 low-income countries, including Vietnam. The share for countries in the lowest quintile ranges from 2.1 to 9.7 percent. The share for Vietnam is reported to be 7.8 percent, which is ninth highest among the 22 countries. The share for countries in the highest quintile ranges from 38.6 to 63.5 percent. The share for Vietnam is reported to be 44.0 percent, which is ninth lowest. World Bank (1995a) also discusses in some detail income distribution and poverty in Vietnam.

The standard deviations are about twice as large for the actual than for the parallel predicted measures, which is consistent with the possibility that the actual values have considerable measurement error. All but one of the correlations are between 0.50 and 0.80, meaning that most of these measures share substantial variance, although each has some independent variation. The one exception is the correlation of 0.99 between predicted income and predicted expenditures—apparently the two predicted variables are similar linear combinations of the

Table 2. *Summary of Alternative Income Measures*
(thousands of dong per capita)

Alternative income indicators	*Mean*	*Standard deviation*	*Correlations*			
					Predicted	
			Income	*Expenditures*	*Income*	*Expenditures*
Income	2,076	1,740	1.00			
Expenditures	1,728	1,320	0.80	1.00		
Predicted income	2,076	924	0.53	0.51	1.00	
Predicted expenditures	1,728	684	0.52	0.51	0.99	1.00

Source: Calculated from data from the 1996 Vietnam Social Sector Financing Survey.

same underlying variables. Because the correlation is almost perfect, we have nothing to gain in presenting estimates for both predicted values because they imply virtually identical elasticities. This high correlation is also consistent with the possibility that both income and expenditures represent the same underlying longer-run household resource constraint, with random measurement errors drawn from differing distributions (with greater variance in the distribution for income).

The predicted measures are preferable to the others if there is random measurement error or if current income or expenditure decisions are made simultaneously with current schooling decisions and it is the longer-run household resource constraint that is relevant for schooling decisions. If the relations being estimated are causal, then, in addition, the instruments used in the first-stage relations cannot be correlated with the disturbance term in the relation of interest, which is a condition that often is difficult to satisfy (for example, demands for children's schooling probably respond to children's unobserved ability, which is likely to be correlated with first-stage variables, such as parents' schooling). For the present purpose of characterizing associations between schooling and household income, there is no such condition to satisfy, because we want to find the full association between longer-run income and schooling whether that association arises because of effects of income or because of correlated unobserved characteristics in the disturbance. If some consumption smoothing is possible, expenditures are likely to be a better measure than income. Therefore we use predicted expenditures per household member for all of our estimates.

In addition to the VNSSFS household questionnaire, the community questionnaire collected information on the location of schools used by the population in the commune. Most of these schools were in the commune, although some upper-secondary schools were located outside. We administered school questionnaires to heads of 209 schools. These questionnaires collected information on characteristics of the personnel, current inputs, physical structures, and finances. We merged these data with the data on children in the 6–17 age range to give the nature of the school options available to each child, depending on the commune of residence.

IV. Associations between Parental Household Income and Children's Progress in School

Here, we measure how strongly the four indicators of children's school progress are associated with parental household income in order to ascertain the schooling advantages that children from higher-income households have over children from lower-income households. We estimate elasticities of each of these four indicators with respect to income per household member.

Preferred Elasticity Estimates

Our preferred estimates use predicted income to represent the longer-run resource constraint. They control for right-censoring for children who have not yet

started school or have not yet completed school and control for the mass point at 1 for grades completed per year of school (table 3). Right-censoring is relatively uncommon for the age of starting school indicator (only 8 of 2,625 observations) but is quite common for the last completed grade indicator (2,173 of 2,615 observations). The mass point at 1 for grades completed per year of school is considerable (1,442 of 2,615 observations).

Two major points about these estimates merit emphasis. First, these income associations are large. The estimate for last completed grade alone—0.353—is seven times the median found for the countries surveyed in Behrman and Knowles (1997). Thus in Vietnam children from higher-income households have a considerable advantage in schooling over children from lower-income households. These elasticities imply, for example, that compared with a child from a household whose income is one standard deviation below the mean, a child from a household whose income is one standard deviation above the mean starts school a quarter of a year earlier, successfully passes 94 percent rather than 80 percent of her or his classes, completes 2.2 years more of schooling, and scores 7 percent higher on examinations, controlling for grades completed (or about a third of the standard deviation in such scores). The mean completed grades of schooling for this calculation is based on 17-year-olds, whose mean is 7.2 years and whose enrollment rate is 0.34, under the assumption that further schooling will be equivalent to one more completed year of schooling for everyone enrolled at the time of the survey. Under plausible assumptions and in combination with other estimates from the data (presented in appendix C), the combined implications of these differences is equal to 13.1 percent of the present discounted value of lifetime income.

Second, the association between income and last grade completed is an important part of the story, but only part of the story. Most of the literature focuses on grades completed, often exclusively. In this case, under the assumptions elaborated in appendix C, changes in grades completed for the two households have a significant association (3.4 percent) with a change in the present discounted value of lifetime income. But this amount is only about a quarter of the total association of 13.1 percent. The association between income and grades passed per year of school accounts for about three-fifths of the total, more than twice as much as the last completed grade. Although such a comparison is based on particular assumptions for a particular sample, it suggests that focusing only on the last completed grade or on years of schooling may substantially underestimate the association between children's success in school and parental household income and that other studies should give more attention to the grades passed per year of school.

Sensitivity to Selected Aspects of the Estimation

For some of the indicators of individual school progress, the use of annual income with no control for censoring results in much smaller estimates of the association with household income. In particular, our preferred estimates are

Table 3. *Elasticities of Individual School Progress Indicators with Respect to Predicted Income per Household Member*

	Elasticity with respect to income				*Basic estimate as a percentage of estimate with*		
Indicator	*Basic: predicted income, control for censoring*	*Annual income, no control for censoring*	*Predicted income, no control for censoring*	*School/ commune fixed effects*	*Annual income, no control for censoring*	*Predicted income, no control for censoring*	*School/ commune fixed effects*
Age when started school	–0.051	–0.051	–0.050	–0.040	100	102	128
	(9.6)	(6.1)	(5.5)	(7.0)			
Grades passed per year of school	0.193	0.070	0.118	0.134	276	164	144
	(12.1)	(5.7)	(9.0)	(6.3)			
Last completed grade	0.356	0.240	0.178	0.353	148	200	101
	(7.4)	(9.0)	(4.9)	(6.1)			
Exam score in last completed grade	0.092	0.085	0.092	0.087	108	100	106
	(6.1)	(6.2)	(6.1)	(3.7)			

Note: Absolute values for *t*-statistics are given in parentheses beneath point estimates. The estimates are all significant at the 5 percent level. The underlying estimates are given in table B-2. For the basic estimates, censored normal regressions are used for age when started school and for last completed grade because of right-censoring, and upper-limit tobits are used for grades passed per year of school because of the mass point at 1.0.

Source: Calculated from data from the 1996 Vietnam Social Sector Financing Survey.

176 percent greater for grades passed per year of school and 48 percent greater for last completed grade than those obtained with no control for censoring (table 3). The differences are very small for age when started school and for exam score in last completed grade, however. This result suggests that, at least for grades passed per year of school and for last completed grade, if the true resource constraint is the long-run constraint represented by predicted income, the use of uncensored annual income considerably underestimates the income association because of transitory fluctuations in income and endogenous aspects of income. Thus we obtain higher income associations than most of the previous literature partly because we use predicted income with control for censoring. This explanation might hold if the use of predicted income is preferable because of endogeneity, but it cannot be the full explanation if the use of predicted income is preferable only because of measurement error due to transitory fluctuations. In that case we would expect similar changes in the estimates for all four of our indicators.

Control for censoring (or for the mass point at 1 for the second indicator) by itself increases some of the estimated income associations considerably. For the first three indicators for which we control for such censoring, our preferred estimates are, respectively, 2, 64, and 100 percent higher than what we obtain with the same income variable but without such controls (table 3). Therefore, if we do not control for censoring, we underestimate considerably the extent of the association with income for the last two of these variables because censoring is selective with regard to income (for example, observations on children from relatively high-income households are more likely to be censored for last completed grade or for successfully passing one grade every year they have been in school). Controlling for censoring does not much affect the estimate for age when started school, probably because censoring is relatively limited for this variable.

Controlling for school and commune fixed effects lessens somewhat the estimated associations between school progress and income. As noted earlier, one channel through which income may be associated with schooling success is through the relationship between schools and communities and household incomes. Estimates that incorporate the full association between income and schooling (including factors related to local communities and school characteristics) are from 1 to 44 percent higher than estimates that control for school and commune fixed effects (table 3). Therefore part of the association between income and our indicators, particularly for age when started school and for grades passed per year of school, reflects differences among schools and communes that are associated with household income. Further, the direction of the changes in the income associations when controlling for school and commune fixed effects is consistent with school resources being allocated to reinforce income differentials rather than to compensate for them.

Sex and Age Differences

In alternative estimates we allowed the income elasticities and the constants to vary with sex and age group (6–11 and 12–17 age brackets). The age that boys

Table 4. *Estimated Differences by Sex and Age in Elasticities of Individual School Progress Indicators with Respect to Predicted Income per Household Member*

Indicator	Elasticity with respect to income: Female, ages 12–17	Elasticity with respect to income: Difference if Male	Elasticity with respect to income: Difference if Ages 6–11	F-test for significance of parameters being dependent on: Male	F-test for significance of parameters being dependent on: Ages 6–11
Age when started school					
Male and age parameter differences	–0.048	–0.021	0.020	7.7	50.8
	(5.8)*	(2.0)*	(1.9)**	[0.0005]	[0.0000]
Male and age parameter differences plus school/commune fixed effects	–0.046	–0.008	0.023	5.5	98.3
	(5.8)*	(1.0)	(2.7)*	[0.0043]	[0.0000]
Grades passed per year of school					
Male and age parameter differences	0.198	–0.011	0.007	8.0	59.4
	(7.9)*	(0.3)	(0.2)	[0.0003]	[0.0000]
Male and age parameter differences plus school/commune fixed effects	0.131	–0.023	0.024	7.9	69.8
	(4.7)*	(0.8)	(0.8)	[0.0004]	[0.0000]
Last completed grade					
Male and age parameter differences	0.426	–0.100	–0.122	0.4	20.9
	(5.8)*	(0.9)	(0.7)	[0.6502]	[0.0000]
Male and age parameter differences plus school/commune fixed effects	0.400	–0.056	–0.120	0.2	20.5
	(5.3)*	(0.6)	(0.7)	[0.8266]	[0.0000]
Exam score in last completed grade					
Male and age parameter differences	0.094	–0.020	0.024	16.2	22.6
	(5.0)*	(1.0)	(1.2)	[0.0000]	[0.0000]
Male and age parameter differences plus school/commune fixed effects	0.083	–0.018	0.037	14.3	26.3
	(3.6)*	(1.0)	(1.8)**	[0.0000]	[0.0000]

* Significant at the 5 percent level.

** Significant at the 10 percent level.

Note: Absolute values for *t*-statistics are given in parentheses beneath point estimates. Probabilities are given in square brackets. The underlying estimates are given in table B-2. Censored normal regressions are used for age when started school and for last completed grade because of right-censoring. Upper-limit tobits are used for grades passed per year of school because of the mass point at 1.0.

Source: Calculated from data from the 1996 Vietnam Social Sector Financing Survey.

start school is more income-responsive than is the age that girls start school (table 4). But otherwise, if anything, boys' school progress indicators have smaller associations with parental household income than do girls'. Although the individual point estimates are imprecisely estimated for both the income-interactive impact of sex and for the additive component, *F*-tests indicate that the sex differences are significantly nonzero at the 1 percent level for age when started school, grades passed per year of school, and exam score in last completed grade, although not for last completed grade. Therefore, once again, if we were to focus only on last completed grade, our inference would be misleading: we would infer that there are no significant gender differences, when in fact there are, according to the other three indicators of school progress.

The point estimates suggest, moreover, that these effects may be fairly large—the income elasticity for boys is more than 40 percent larger in absolute magnitude for age when started school and is 6 and 22 percent smaller, respectively, for grades passed per year of school and exam score in last completed grade (including the effects generated by school and commune characteristics). The income elasticity of total schooling expenditures that households pay directly to schools is also significantly greater for boys than for girls, even though the elasticities for school fees alone and households' school-related expenses not paid directly to schools do not differ significantly by sex (see table B-3).

Thus there seem to be some gender differences in income associations with three of the four indicators, although age when started school pushes in a different direction than grades completed per year of school and exam score in last completed grade. The net effect of these gender differences is to imply a smaller association between income and school progress for boys than for girls because the income effects are much larger for grades completed per year of school than for age when started school. The association between enrollment and parental household income is also significantly smaller for boys than for girls (compare elasticities of 0.092 and 0.196 in table B-4). Thus if income elasticities are higher for luxuries than for necessities, girls' schooling is treated as more of a luxury than is boys' schooling.

The absolute magnitude of the elasticities of income associated with age when started school and last completed grade are smaller for children in the age range 6–11 compared with children in the age range 12–17, but the elasticities of income associated with grades passed per year of school and exam score in last completed grade are larger. The individual income-multiplicative and additive age effects (similar to those for sex) tend to be imprecisely estimated, but *F*-tests indicate that the age effects are significantly nonzero at high levels of significance for all four indicators (table 4). Also, the point estimates suggest that there may be fairly large sex effects on the gross income elasticities for the 6–11 age group compared with the 12–17 age group. These effects reduce the absolute magnitude of the income elasticity by more than 40 percent for age when started school and by more than 25 percent for last completed grade, and they increase the income elasticity for exam score in last completed grade by more than 25 percent

(although they increase the income elasticity for grades passed per year of school only 4 percent).

The smaller absolute magnitude of the income elasticity of age when started school for younger children suggests a reduction in the importance of income in determining age of entry between these two cohorts. The larger elasticities of the older group for last completed grade and exam score are consistent with income being a less important constraint for children of primary school age, given that primary school is almost universal, but being a more important constraint for older children (12–17 years), for whom most decisions at the margin about continuing school are made. The pattern is the opposite for grades completed per year of school, although, as usual with cross-sectional data, it is difficult to disentangle life-cycle and cohort effects for such variables. Also, it should be noted that the interaction between age group and income is not significantly nonzero in the enrollment probits (table B-4).

V. Associations between Parental Household Income and School Policies

School policies may compensate for differences in household income or may reinforce them. Two major aspects of such policies are income-related school fees and school quality in terms of the quality of teachers, current inputs, and school structures, and the amount of congestion. Both school fees and school quality may affect which children enroll in school and how well they succeed once enrolled. Whatever distributional effect these policies have on children who enroll in school, they have a less equalizing effect on all children if enrollment itself (inclusive of the effects of these policies) is inversely associated with income. We find that, indeed, enrollment is positively associated with household income (table 5). Children in the first two quintiles have significantly lower enrollment rates, and those in the fifth quintile have significantly higher enrollment rates, than those in the third quintile. We return to this point at the end of this section.

School Fees and Other Household School-Related Expenditures

Actual school fees paid tend to be inversely associated with income: the means for the first and second quintiles are significantly below, and the mean for the fifth quintile is significantly above, the mean for the third quintile (table 5). The structure of school fees is progressive mainly because there are a primary school fee exemption and a higher concentration of children from lower-income households in primary school. Of the total number of children in the sample who receive exemptions from school fees, 80.3 percent receive them because they are in primary school, 8.0 percent because they live in mountainous regions, 4.3 percent because they are members of ethnic minorities, and only 1.0 percent because they are from poor households.

For this reason the income elasticities that depend on age are of primary interest. These estimates yield an income elasticity of 2.312 for the age range 12–17

Table 5. *Means of Enrollment, School Fees, and Other Household School-Related Expenditures Conditional on Enrollment by Predicted Income per Household Member*

Enrollment, fees, and school-related expenditures	*Income quintile*				
	1 (poorest)	*2*	*3*	*4*	*5*
Current enrollment	0.72	0.78	0.78	0.81	0.87
	(0.45)	(0.41)	(0.41)	(0.39)	(0.34)
	[2.8]*	[2.3]*		[1.3]	[6.1]*
Conditional on enrollment at time of survey					
School fees	15	12	21	22	36
	(35)	(32)	(52)	(48)	(68)
	[2.5]*	[2.3]*		[0.5]	[3.6]*
Total expenditures paid to schools[a]	38	41	55	67	102
	(45)	(42)	(65)	(59)	(88)
	[4.7]*	[3.1]*		[3.1]*	[8.6]*
School-related expenditures not paid to schools[b]	123	146	175	287	523
	(140)	(198)	(181)	(362)	(411)
	[4.8]*	[1.7]**		[5.4]*	[15.3]*
Total school-related expenditures	162	187	230	354	625
	(163)	(220)	(216)	(385)	(437)
	[5.5]*	[2.3]*		[5.7]*	[16.4]*

* Significant at the 5 percent level.

** Significant at the 10 percent level.

Note: Standard deviations are in parentheses, and absolute values of *t*-statistics for differences from the third quintile are given in square brackets. All fees and expenditures are measured in thousands of dong per year in 1996 prices.

a. Includes school fees, school improvement fees, parent association fees, and insurance.

b. Includes textbooks, supplies and materials, uniforms, tutoring fees, travel, meals, and miscellaneous.

Source: Calculated from data from the 1996 Vietnam Social Sector Financing Survey.

years, but close to zero for the primary school age group (6–11 years; table 6). The latter elasticity is close to zero because exemptions for primary school are widespread regardless of household income. That the income elasticity for the probability of fee exemptions is much larger in absolute magnitude for older children suggests that the income-related exemptions have a much greater effect. The estimated elasticity of school fees with respect to income in the simplest specification (with only income and not age) is smaller and more imprecise, whether or not we control for school and commune fixed effects (table B-3). If we do control for age, the estimated income elasticity does not change much for the age group 12–17 years if school and commune fixed effects are added, although the estimated change for the age group 6–11 years is much smaller in absolute magnitude and much more imprecisely estimated (table 6). Gender effects are not significantly nonzero in any of these specifications.

Table 6. *Estimated Elasticities of Household School-Related Expenditures with Respect to Predicted Income per Household Member*

Private school-related expenditures	*Base*	*Change for*	
		Ages 6–11	*Male*
School fees			
With all parameters dependent on age and sex[a]	2.312	–2.228	0.190
	(2.4)*	(1.6)	(0.2)
With school/commune fixed effects and age and sex[a]	2.123	–0.448	–0.729
	(2.1)*	(0.4)	(0.7)
Total expenditures paid to schools			
With all parameters dependent on age and sex[b]	1.063	–0.338	0.319
	(6.2)*	(1.7)*	(1.6)
With school/commune fixed effects and age and sex[b]	0.243	0.015	0.221
	(1.7)*	(0.1)	(1.5)

* Significant at the 5 percent level.

Note: The full lower-limit tobit estimates are given in table B-3. Absolute values for *t*-statistics are given in parentheses beneath point estimates.

a. *F*-tests indicate that parameter dependence on age is significant even though individual additive and multiplicative parameter estimates are imprecise ($F = 223.5$, probability 0.0000 for all parameters; $F = 267.0$, probability 0.0000 for fixed effects). *F*-tests indicate that parameter dependence on sex is insignificant ($F = 1.1$, probability 0.3458 for all parameters; $F = 0.9$, probability 0.4163 for fixed effects).

b. *F*-tests indicate that parameter dependence on age and sex is significant even though individual additive and multiplicative parameter estimates are imprecise ($F = 23.8$, probability 0.0000 for age, and $F = 4.5$, probability 0.0111 for sex in all parameters; $F = 55.9$, probability 0.0000 for age, and $F = 4.3$, probability 0.0130 for sex in fixed effects).

Source: Calculated from data from the 1996 Vietnam Social Sector Financing Survey.

Thus school fees tend to favor poorer households among those households that enroll children in school; however, they are not targeted only to such households because they work substantially through exemptions of primary school fees, which are given broadly regardless of household income. But this progressive fee structure may have a limited effect because school fees are a relatively small proportion of total household expenditures paid to schools, to say nothing of total school-related household expenditures. School fees average only 34 percent of total school-related household expenditures paid directly to schools (school improvement fees also average 34 percent, insurance averages 12 percent, and parent association fees average 10 percent).

Total expenditures paid to schools, like school fees alone, increase systematically with income (see table 5), but with an income elasticity that is much smaller than the 2.312 estimated for school fees—it is 1.063 with comparable controls for age and sex and 0.243 with additional controls for school and commune fixed effects (table 6). Therefore, the relatively limited response to income of household expenditures paid directly to schools (other than school fees) means that, despite fairly high income elasticities for school fees, total household expenditures paid directly to schools are much less income-responsive than are school fees alone. School fees, moreover, are an even smaller share of total school-related household expenditures—the mean for the full sample is only 6.9 percent because school-related household expenditures not paid directly to schools

are almost four times as large as those paid directly to schools (16.4 and 4.2 percent, respectively, of total household expenditures per household member). Thus even school fee exemptions that are much better targeted would have only a limited impact on the relationship between total household school-related expenditures and income and therefore, presumably, on poor households' decisions about schooling. To have more of an impact, policies would have to extend exemptions to household expenditures paid directly to schools beyond school fees or amend payment structures to make them much more strongly related to household income (possibly including negative fees or subsidies for children from poorer households).

School Quality

Governments also might alter schooling options for different households by affecting the quality of public schools (which dominate in Vietnam, accounting for 91.5 percent of students in the sample). Much of the recent economic literature on schooling focuses on the importance of school quality for school outcomes (see, for example, Card and Krueger 1996 and Moffitt 1996 for recent surveys on studies in the United States; Alderman and others 1996b, Behrman and Birdsall 1983, Behrman, Birdsall, and Kaplan 1996, and Hanushek 1995 for studies and surveys of developing countries).

We have constructed four indicators of school quality for school staff, current inputs, congestion, and facilities (see the note to table 7 for details). A priori and on the basis of other studies, congestion seems to be negatively associated with school success, and the other three quality indicators seem to be positively associated. We calculate simple log-linear estimates of the associations between our four indicators of school success and our four quality indicators. These estimates look like production functions, and similar relations have often been interpreted as production functions (see, for example, most of the studies surveyed in Hanushek 1995). But they can be interpreted as production functions only under the assumption that the right-side variables are predetermined in a statistical sense and therefore are independent of the disturbance term in the relation. This seems highly unlikely a priori and in light of the estimates in table 9, suggesting that the right-side variables are significantly correlated with household income (and therefore are most likely correlated with other determinants of school success, such as home learning environments, the effects of which are in the disturbance term of this relation). In our cross-sectional data set (as in most cross-sectional data sets), moreover, plausible instruments that would enable us to use some technique, such as instrumental variables, to control for determinants of school quality, are not available.

Our estimates suggest that the strongest associations are between the quality of school staff and children's success in school (table 8). All four elasticities are significantly nonzero with the a priori expected signs and with absolute magnitudes ranging from 0.176 for exam score in last completed grade to 1.094 for last completed grade. The quality of current inputs is significant and positively asso-

Table 7. *Mean School Quality Indicators by Predicted Income per Household Member*

	Income quintile				
Indicator	*1 (poorest)*	*2*	*3*	*4*	*5*
Staff	0.66	0.70	0.71	0.73	0.76
	(0.11)	(0.09)	(0.10)	(0.08)	(0.07)
	[8.0]*	[1.6]		[3.4]*	[8.3]*
Current inputs	0.30	0.33	0.39	0.45	0.56
	(0.12)	(0.15)	(0.16)	(0.15)	(0.12)
	[9.4]*	[6.4]*		[6.2]*	[18.7]*
Congestion	0.19	0.19	0.20	0.20	0.21
	(0.05)	(0.06)	(0.05)	(0.06)	(0.06)
	[3.4]*	[1.6]		[0.1]	[4.3]*
Facilities	0.10	0.18	0.24	0.33	0.36
	(0.14)	(0.18)	(0.22)	(0.25)	(0.27)
	[11.7]*	[5.1]*		[5.4]*	[7.5]*

* Significant at the 5 percent level.

Note: Standard deviations are in parentheses, and absolute values of *t*-statistics for differences from the third quintile are in square brackets. The results are based on local schools for all children ages 6–17 in the sample. The "facilities" variable is the proportion of buildings occupied by the school that are "permanent construction" (as opposed to "semi-permanent" or "temporary"). The "staff," "current inputs," and "congestion" indicators are each based on the sum of a number of components. For each component the range of responses for all schools has been normalized to between 0 and 1 for each of three schooling levels (primary, lower secondary, upper secondary) and then averaged across the three levels. The components for "staff" are whether the head of school is qualified to teach at the level of the school, the years of experience as head of the school, the proportion of the school's teachers who are qualified by the Ministry of Education and Training to teach at that level, the proportion of teachers ranked "excellent" or "good" by the head of the school, the proportion of teachers who are full-time, and the average teaching experience of teachers. The components for "current inputs" are the proportion of classrooms wired for electricity, the proportion of classrooms with legible blackboards, the proportion of classrooms with ceiling fans, whether the school has safe water, whether the school has hygienic latrines, whether the school has a library, whether the school has a science laboratory, the number of computers per student, and the number of textbooks per student for rental or loan to poor students. The components for "congestion" are the proportion of classes taught in the third shift, the number of students per class, and the number of students per teacher.

Source: Calculated from data from the 1996 Vietnam Social Sector Financing Survey.

ciated with grades passed per year of school and exam score in last completed grade and is associated less precisely with last completed grade (significant at the 15 percent level), but with much smaller elasticities (between 0.067 and 0.096). Moreover, the association with age when started school is significant and opposite in sign to that expected a priori. Congestion is significant and negatively associated with exam score in last completed grade, with an elasticity of –0.050, but is not significantly nonzero for the other indicators. Facilities are significantly nonzero only for last completed grade, with the opposite sign to that expected a priori.

What are the associations between these indicators of school quality and parental income? When we compared the estimates of the elasticities of the four school success indicators with respect to household income with and without controls for school and commune fixed effects, we found that, on net, school quality indicators are positively associated with income (that is, the basic estimates in table 3 are larger without than with controls for school and commune

Table 8. *Regressions of the Logarithm of Individual School Progress Indicators on School Quality Indicators*

	Elasticity with respect to							
Indicator	*Staff*	*Current inputs*	*Congestion*	*Facilities*	*Constant*	*R-squared*[a]	*Chi² test*[b]	*Number of observations*
Age when started school[c]	–0.206	0.026	–0.011	0.0004	1.770	0.048	157.9	2,308
	(11.7)*	(4.3)*	(1.1)	(0.1)	(109.9)*	[0.115]	[0.0000]	
Grades passed per year of school	0.420	0.090	0.022	0.012	0.288	0.048	128.3	2,403
	(7.4)*	(4.5)*	(0.7)	(1.1)	(5.2)*	[0.355]	[0.0000]	
Last completed grade[c]	1.094	0.096	0.109	–0.114	2.958	0.029	51.4	2,306
	(6.0)*	(1.6)	(1.1)	(3.4)*	(17.3)*	[0.732]	[0.0000]	
Exam score in last completed grade[d]	0.176	0.067	–0.050	–0.009	1.836	0.025	6.4	2,094
	(3.3)*	(3.6)*	(2.0)*	(1.1)	(41.4)*	[0.251]	[0.0000]	

* Significant at the 5 percent level.

Note: Absolute values for *t*-statistics are given in parentheses beneath point estimates. Because the variable log facilities is in the data for only 1,957 observations, a dummy variable is included to control for observations for which this variable is missing.

a. Standard error given in square brackets.

b. Probability given in square brackets.

c. Censored normal regression (with 1 right-censored and 2,307 uncensored observations for age when started school and with 2,173 right-censored and 442 uncensored observations for last completed grade). The R^2 is a pseudo R^2.

d. Regressions are with robust standard errors, and the standard deviations are corrected for clustering at the household level. The root mean standard error is given beneath R^2, and the *F*-test is given in the penultimate column.

Source: Calculated from data from the 1996 Vietnam Social Sector Financing Survey.

fixed effects). All of the estimated elasticities of the four quality indicators with respect to household income are positive, although that for congestion is significantly nonzero only at the 10 percent level (table 9). This result means that three of the four quality measures do not compensate poorer households; rather, they reinforce the advantages of higher-income households. The one exception is congestion, which weakly favors poorer households but is significantly associated with only one of our indicators. Because policies result in children from higher-income households on net having higher-quality facilities, part of the positive association between parental household income and household expenditures paid to schools may result from households' paying for higher-quality schooling and not from a progressive school fee structure for a given school quality.

Combined Implications

We now illustrate how the associations between household income and school-related expenditures paid directly to schools, school quality, and school enrollment can be combined to obtain an estimate of the association between the price of school and household income holding quality constant. Let household school-related expenditures paid directly to schools be the product of three components: the price for a given school quality, school quality, and quantity. That is, we posit the effective quantity of school to be the quantity adjusted for the quality by a multiplicative factor. Thus the expenditure elasticity with respect to income ($E_{exp,\ inc}$) is the sum of the elasticity of the constant-quality price with respect to income ($E_{price,\ inc}$), the elasticity of the quantity with respect to income ($E_{quan,\ inc}$), and the elasticity of the quality with respect to income ($E_{qual,\ inc}$):

$$(1) \qquad E_{exp,\ inc} = E_{price,\ inc} + E_{quan,\ inc} + E_{qual,\ inc}$$

Table 9. *Regressions of the Logarithm of School Quality Indicators on the Logarithm of Predicted Income per Household Member*

Indicator	*Elasticity with respect to income*	*Constant*	*R-squared*[a]	*F-test*[b]	*Number of observations*
Staff	0.086	-0.977	0.091	12.8	2,388
	(3.6)*	(5.3)*	[0.140]	[0.0005]	
Current inputs	0.435	-4.180	0.201	36.1	2,388
	(6.0)*	(7.6)*	[0.445]	[0.0000]	
Congestion	0.072	-2.184	0.021	2.9	2,388
	(1.7)**	(7.2)*	[0.253]	[0.0937]	
Facilities	0.686	-6.652	0.125	15.2	1,942
	(3.9)*	(5.2)*	[0.935]	[0.0002]	

* Significant at the 5 percent level.

** Significant at the 10 percent level.

Note: Regressions are with robust standard errors. Absolute values for *t*-statistics are given in parentheses beneath point estimates. The standard deviations are corrected for clusters at the commune level.

a. Root mean squared error given in square brackets.

b. Probability given in square brackets.

Source: Calculated from data from the 1996 Vietnam Social Sector Financing Survey.

If the first component—the elasticity of price with respect to income—is positive, the marginal cost curve is lower for poorer households, so pricing favors the poor. A positive value of the second component, the elasticity of quantity with respect to income, implies that higher-income households purchase more inputs than poorer households, which, ceteris paribus, translates into greater benefits for higher-income households. A positive value of the third component, the elasticity of quality with respect to income, implies that higher-income households purchase higher-quality inputs than poorer households, which, ceteris paribus, translates into greater benefits for higher-income households.

Let $E_{exp,\,inc}$ be the elasticity of household school-related expenditures paid directly to schools (1.063 in table 6), $E_{quan,\,inc}$ be the elasticity of enrollment with respect to household income (0.152 in table B-4), and $E_{qual,\,inc}$ be the average of the elasticities of the two components of school quality that are most related to child school success in table 8—the quality of staff and the quality of current inputs (0.261, the average of the first two rows of table 9). Equation 1 can then be solved for the constant-quality price elasticity with respect to income, $E_{price,\,inc}$ = 0.650. This value is considerably less than—in fact only about three-fifths as large as—the income elasticity of household school-related expenditures paid directly to schools. The structure of household fees paid directly to schools thus appears to be much less progressive once we correct for the fact that children from poor households are less likely to enroll in school and children from higher-income households have higher-quality schools available to them.

VI. Conclusions

Many societies are concerned about the association between parental income and child schooling because it is perceived that the stronger this association is, the less is intergenerational social mobility and the less equal is opportunity. Such concerns have been especially strong in Vietnam, although there is a perception that some important policies, such as school fee exemptions, have substantially weakened the association between household income and school success.

We investigated the magnitudes of these associations and found them to be considerable. For example, our estimate for the income elasticity of completed grades is five times the median estimate of 42 earlier studies. We found, moreover, that this association is strongest *not* for completed grades, on which most of the previous literature has focused, but for grades completed per year of school. That is, the most important relationship takes into account failed and repeated grades, not just the total number of grades finished. We also found significant, but smaller, associations between parental household income and age when started school and exam score in last completed grade. There is some evidence of gender differences in the income associations, the strongest being the smaller income association with grades completed per year of school for boys than for girls. This result implies that schooling of girls is treated as more of a luxury (less of a necessity) than is schooling of boys.

We also explored the sensitivity of our estimates to a number of different estimation choices made in previous studies. We found that the estimated associations with income are significantly larger when we use predicted income (to represent longer-run income), control for censoring, and represent additional channels through which there may be effects beyond just the number of completed grades. If our results generalize to other societies, much of the previous literature may understate the true associations between household income and school success and therefore overstate true intergenerational social mobility and equality of opportunity.

Finally, we explored some dimensions of how policies relate to parental household income. School fees are progressive in the sense that they favor children from lower-income households among those children enrolled in school, particularly because of the primary school fee exemption. But school fees are only about one-third of what households pay directly to schools and are a much smaller proportion of households' total school-related expenditures. Total expenditures paid directly to schools increase with household income much less proportionately than do school fees alone, so the overall structure of such payments is much less progressive than is the structure of school fees. Moreover, because school enrollment is positively correlated with household income, the structure of school fees is less progressive for the entire population of households with school-age children than it is for the selected subset of that population with children enrolled in school. Further, the two school quality measures that are most strongly and positively associated with our four indicators—the quality of the staff and the quality of current inputs—are much more available to higher-income households, meaning that higher-income households have greater school expenditures in part because they are obtaining higher-quality schooling, not because of progressive charges for the same quality of schooling across income classes. Therefore, although the school fee structure attempts to equalize schooling options, it has only a limited impact.

Appendix A. A Summary of Studies Exploring the Association between Income and Schooling

Table A-1. *Estimated Income Elasticities for Schooling*

Country and year	*Schooling indicator*	*Income elasticity*	*Source*	*Notes*
Bangladesh, 1980–81	School attendance	0.20	Hossain (1989)	Father's income; control for household and community characteristics
Bolivia, 1989	Grade repetition		Patrinos and Psacharopoulos (1993)	Income; control for household and community characteristics
Bolivia, 1990	Grade attained Days missed Grades failed	0.04* –0.06** –0.02	Behrman, Ii, and Murillo (1995)	Expenditure per household member; control for household characteristics and community fixed effects
Brazil, 1970	Completed years	0.09 to 0.16*	Birdsall (1985)	Father's income; control for household and teacher characteristics
Brazil, 1972–74	Enrollment	–0.17*	Singh (1992)	Rural only; income (excluding children's income); control for household characteristics
Brazil, 1982	Completed years	0.06 to 0.14*	Barros and Lam (1996)	For São Paulo and the Northeast; household head's income; control for parental schooling
Brazil, 1982	Completed years	0.12 to 0.22*	Thomas, Schoeni, and Strauss (1996)	Urban only; household income; control for household characteristics
Côte d'Ivoire, 1985–87	Completed years Current enrollment*	0.19*	Montgomery and Kouame (1993)	Consumption per adult; control for mother's characteristics and region
Côte d'Ivoire, 1985–87	School attainment	0.14 to 0.42*	Tansel (1997)	Expenditure per capita (instrumented); control for age, sex (significant for females, lower and insignificant or significant at 10 percent level for males), school distance, wages, rural area
Egypt, 1980	Ever attended; currently attending; and years attended		Cochrane, Mehra, and Osheba (1986)	Income per capita; control for household and school characteristics
Ghana, 1988–89	Grade attainment;* reading;* attending other than nearest school;* delayed enrollment;* mathematics; dropping out age		Glewwe and Jacoby (1994, 1995a)	Expenditure per capita (instrumented); control for household and (numerous) school characteristics and selectivity

(Table continues on the following page.)

Table A-1. *(continued)*

Country and year	*Schooling indicator*	*Income elasticity*	*Source*	*Notes*
Ghana, 1987	Ever-attended school; school attainment		Lavy (1996)	Income per capita; control for household and (numerous) school and community characteristics
Ghana, 1987–89	School attainment	0.18 to 0.56*	Tansel (1997)	Expenditure per capita (instrumented); control for age, sex (significant for males, lower and insignificant or significant at 10 percent level for females), school distance, wages, rural area
Guatemala, 1989	Repetition**		Patrinos and Psacharopoulos (1993)	Income; control for household and community characteristics
India, 1980–81	Enrollment		Duraisamy (1988)	Rural Tamil Nadu; non-labor market income; control for household and community characteristics
India, 1991	Achievement test	0.12*	Kingdon (1996)	Lucknow, Uttar Pradesh; index of consumer durables; control for household and school characteristics
Indonesia, 1987	Probability of attainment		Deolalikar (1993)	Nonlabor income; control for household and community characteristics
Indonesia, 1989	Transition probabilities for initial enrollment and to next school level*		King (1995)	Expenditure per capita; control for household and community characteristics
Jamaica, 1989	Enrollment*		Handa (1994)	Expenditure per capita; control for household and community characteristics
Jamaica, 1990	Mathematics	0.07*	Glewwe and others (1995)	Expenditure per capita; control for household and numerous school characteristics and selectivity
Kenya, 1994	Reading Enrollment;* household school expenditure;* student-teacher ratio*	0.04	Deolalikar (1997)	Expenditure per capita; interacts with household and school characteristics

Malaysia, 1975–76	Current enrollment* (Malays**)		De Tray (1984)	Income; control for household and school characteristics
Malaysia, 1975–76	Completed schooling (Malays,* Chinese)		King and Lillard (1987)	Income; control for household and school characteristics and right-censoring
Malaysia, 1988	Schooling progression probabilities		Lillard and Willis (1994)	Father's earnings; control for household characteristics, distance to primary schools, and region
Mali, 1981–82	Enrollment		Birdsall and Orivel (1996)	Income; control for school characteristics
Nepal, 1980–81	Grade attainment Enrollment*	0.38*	Moock and Leslie (1986)	Value of crop output; control for household characteristics, presence of local primary school, and region
Nicaragua, 1977–78	Grades completed	0.02 to 0.07	Behrman and Wolfe (1987)	Income or predicted mother's earnings plus other household income; control for household and community characteristics
Pakistan, 1991	Ever-enrolled;* expenditure on primary school*		Sather and Lloyd (1994)	Expenditure; control for household characteristics, distance to primary schools and region
Pakistan, 1989	Numeracy Literacy Attendance (males,* females)	0.05 0.23*	Alderman and others (1996a)	Household income (instrumented); control for household and community variables
Pakistan, 1986–92	Enrollment*		Alderman, Behrman, Khan, Ross, and Sabot (1997)	Rural only; expenditure (average over three years); control for household, school, and community characteristics
Paraguay, 1990	Current enrollment;* years; grade repetition; dropping out		Patrinos and Psacharopoulos (1995)	Income; control for household characteristics
Peru, 1985–86	Ever-enrolled;* early enrollment		Ilon and Moock (1991)	Rural only; household expenditure; control for household and community variables

(Table continues on the following page.)

Table A-1. *(continued)*

Country and year	*Schooling indicator*	*Income elasticity*	*Source*	*Notes*
Peru, 1985–86	Progression on time through school*		Jacoby (1994)	Income other than self-employed income; control for household and school variables
South Africa, 1993	Years	–0.01 to 0.10*	Case and Deaton (1996)	Expenditure per household member; ages 10–18 for years of school, 8–24 for enrollment; control for household characteristics including race and pupil–teacher ratio; years of schooling lower and insignificant for whites; reason not attending: expense, illness, completed, insignificant for pregnancy and cannot cope; expenditure share insignificant for secondary and, for whites, for primary
	Enrollment (blacks)	0.01*		
	Reason not attending (blacks)	–0.02 to 0.02*		
	Expenditure share—school	–0.02 to 0.12*		
	Test scores* (blacks; for whites literacy not significant and numeracy significant at 10 percent level)			
Taiwan (China), 1989	Years of schooling	0.12 to 0.33*	Parish and Willis (1993)	Father's income; birth cohorts 1940–49 through 1970–75, with declining elasticity for more recent cohorts; control for household characteristics
United States, 1957	Years of schooling	0.04*	Hauser and Daymont (1977)	Wisconsin high school graduates; parental income; control for household characteristics

United States, 1978	Years of schooling	–0.03 to 0.00	Datcher (1982)	Income; control for household and community characteristics
United States, 1981	Years of schooling	0.02*	Behrman and Taubman (1986)	Income; control for household characteristics
United States, 1982	Years of schooling	0.09*	Hill and Duncan (1987)	Income averaged over three; control for household and community characteristics
United States, 1981	Years of schooling	0.04*	Behrman and Taubman (1989)	Income; control for household characteristics
United States, 1979–86	Negative or insignificant for college grades and graduate probabilities		Datcher Loury and Garman (1995)	Income; control for individual test scores, race, and college mean test scores
Venezuela, 1987	Years Negative for currently in school,* repeating grade,* and illiterate*	0.01**	Psacharopoulos and Yang (1991)	Family income; control for age, sex, and father's schooling

* The underlying point estimates are significantly nonzero at the 5 percent level.

** The underlying point estimates are significantly nonzero at the 10 percent level.

Note: Income elasticities calculated at point of sample means for what appear to be preferred estimates. If information is not provided with which to calculate elasticities, the dependent variables and significant levels are indicated.

Source: Behrman and Knowles (1997).

APPENDIX B. ESTIMATION RESULTS FOR PREDICTED INCOME AND ASSOCIATIONS BETWEEN INCOME AND SCHOOLING

Table B-1. *Estimates of Current Income per Household Member and Total Household Expenditures per Household Member as a Function of Longer-Run Household Characteristics*

Right-side variables	*Current income*	*Current expenditures*
Parent's schooling		
Mother's schooling	2.49	5.38
	(2.3)	(2.2)
Father's schooling	2.20	1.90
	(1.9)	(2.1)
Mother's schooling missing	8.16	9.23
	(0.7)	(1.0)
Father's schooling missing	10.60	15.50
	(0.9)	(1.8)
Number of household members		
Male	-8.43	-6.31
	(3.5)	(3.4)
Female	-15.69	-11.23
	(7.0)	(6.5)
Ethnic group	4.72	4.88
	(0.5)	(0.7)
Formal religion	66.89	35.31
	(4.9)	(3.4)
Catholic	-56.95	-28.81
	(3.8)	(2.5)
Buddhist	-27.90	-12.41
	(2.1)	(1.2)
Assets per dong10,000	0.359	0.186
	(6.1)	(4.2)
Income primarily from primary sector	16.81	7.59
	(2.0)	(1.2)
Employment		
State	-30.67	10.17
	(1.1)	(0.5)
Cooperative	-104.84	-58.45
	(4.0)	(2.9)
Private sector	-31.20	-4.70
	(1.2)	(0.2)
Infrastructure availability or use		
Electricity	45.27	28.49
	(6.3)	(5.2)
Good water	5.51	6.89
	(0.8)	(1.2)
Latrine	62.49	41.90
	(8.1)	(7.1)
Good transportation	-2.98	-3.49
	(0.5)	(0.7)
Regular news	37.88	31.28
	(5.5)	(6.0)
Constant	166.90	119.06
	(5.7)	(5.3)
Root mean squared error	124.00	94.78
Adjusted R^2	0.27	0.26

Note: Sample includes all 1,844 households with relevant data. Absolute *t*-values are in parentheses.
Source: Calculated from data from the 1996 Vietnam Social Sector Financing Survey.

Table B-2. *Regressions of the Logarithm of School Progress Indicators on the Logarithm of Predicted Income per Household Member*

	Elasticity with respect to income			*Constant*				*Statistical tests*[a]			
									F-test		
Indicator	*Base: female ages 12–17*	*Difference from base if Male*	*Difference from base if Ages 16–11*	*Base: female ages 12–17*	*Difference from base if Male*	*Difference from base if Ages 6–11*	R^2[b]	*Chi² test for overall significance*	*School/ commune fixed effects*	*Male*	*Ages 6–11*
Age when started school (N = 2,625)[c]											
Basic	–0.051			2.22			0.030	90.7			
	(9.6)*			(57.4)*			[0.133]	[0.0000]			
Annual income, no control for censoring[d]	–0.051			2.22			0.056	37.4			
	(6.1)*			(34.9)*			[0.131]	[0.0000]			
Predicted income, no control for censoring[d]	–0.050			2.22			0.034	30.5			
	(5.5)*			(32.1)*			[0.132]	[0.0000]			
Basic plus school/commune fixed effects	–0.040			2.16			0.433	1312.8	25.1		
	(7.0)*			(48.2)*			[0.105]	[0.0000]	[0.0000]		
Basic plus male and age parameter differences	–0.048	–0.021	0.020	2.22	0.17	–0.20	0.068	205.8		7.7	50.8
	(5.8)*	(2.0)*	(1.9)**	(36.3)*	(2.3)*	(2.6)*	[0.130]	[0.0000]		[0.0005]	[0.0000]
Basic plus school/commune fixed effects and male and age parameter differences	–0.046	–0.008	0.023	2.21	0.07	–0.22	0.499	1512.8	27.3	5.5	98.3
	(5.8)*	(1.0)	(2.7)*	(38.8)*	(1.2)	(3.6)*	[0.101]	[0.0000]	[0.0000]	[0.0043]	[0.0000]
Grades passed per year of school (N = 2,774)[c]											
Basic	0.193			–1.398			0.046	148.0			
	(12.1)*			(12.1)*			[0.372]	[0.0000]			
Annual income, no control for censoring[d]	0.070			–0.666			0.036	30.2			
	(5.7)*			(7.3)*			[0.227]	[0.0000]			

(Table continues on the following page.)

Table B-2. *(continued)*

Indicator	Elasticity with respect to income: Base: female ages 12–17	Elasticity with respect to income: Difference from base if Male	Elasticity with respect to income: Difference from base if Ages 16–11	Constant: Base: female ages 12–17	Constant: Difference from base if Male	Constant: Difference from base if Ages 6–11	R^2[b]	Statistical tests[a]: Chi^2 test for overall significance	Statistical tests[a]: F-test: School/commune fixed effects	Statistical tests[a]: F-test: Male	Statistical tests[a]: F-test: Ages 6–11
Predicted income, no control for censoring[d]	0.118			–1.015			0.063	81.4			
	(9.0)*			(10.3)*			[0.224]	[0.0000]			
Basic plus school/commune fixed effects	0.134			–1.100			0.175	556.3	6.2		
	(6.3)*			(7.1)*			[0.342]	[0.0000]	[0.0000]		
Basic plus male and age parameter differences	0.198	–0.011	0.007	–1.477	0.016	0.129	0.091	290.1		8.0	59.4
	(7.9)*	(0.3)	(0.2)	(8.1)*	(0.1)	(0.5)	[0.367]	[0.0000]		[0.0003]	[0.0000]
Basic plus school/commune fixed effects and male and age parameter differences	0.131	–0.023	0.024	–1.150	0.110	0.011	0.226	720.4	6.4	7.9	69.8
	(4.7)*	(0.8)	(0.8)	(5.7)*	(0.5)	(0.1)	[0.336]	[0.0000]	[0.0000]	[0.0004]	[0.0000]
Last completed grade (N = 2,615)[c]											
Basic	0.356			–0.068			0.025	54.4			
	(7.4)*			(0.2)			[0.819]	[0.0000]			
Annual income, no control for censoring[d]	0.240			–0.367			0.046	80.7			
	(9.0)*			(1.8)**			[0.680]	[0.0000]			
Predicted income, no control for censoring[d]	0.178			0.090			0.016	23.7			
	(6.3)*			(0.3)			[0.691]	[0.0000]			
Basic plus school/commune fixed effects	0.353			0.192			0.191	425.7	5.9		
	(6.1)*			(0.4)			[0.651]	[0.0000]	[0.0000]		
Basic plus male and age parameter differences	0.426	–0.100	–0.122	0.620	0.729	1.52	0.052	116.5		0.4	20.9
	(5.8)*	(0.9)	(0.7)	(1.2)	(0.9)	(1.1)	[0.881]	[0.0000]		[0.6502]	[0.0000]

Basic plus school/commune fixed effects and male and age parameter differences	0.400 (5.3)*	–0.056 (0.6)	–0.120 (0.7)	–0.135 (0.2)	0.407 (0.6)	1.43 (1.1)	0.219 [0.693]	486.2 [0.0000]	5.5 [0.0000]	0.2 [0.8266]	20.5 [0.0000]
Exam score in last completed graded (N = 2,374)											
Basic	0.092 (6.1)*			1.292 (10.0)*			0.029 [0.263]	37.1 [0.0000]			
Annual income, no control for censoring[d]	0.085 (6.2)*			1.177 (16.6)*			0.038 [0.263]	27.1 [0.0000]			
Predicted income, no control for censoring[d]	0.092 (6.1)*			1.292 (10.0)*			0.029 [0.263]	37.1 [0.0000]			
Basic plus school/commune fixed effects	0.087 (3.7)*			1.225 (7.5)*			0.116 [0.255]	7.0 [0.0000]	5.1 [0.0000]		
Basic plus male and age parameter differences	0.094 (5.4)*	–0.020 (1.0)	0.024 (1.2)	1.111 (8.8)*	0.085 (0.6)	–0.107 (0.7)	0.060 [0.259]	21.6 [0.0000]		16.2 [0.0000]	22.6 [0.0000]
Basic plus school/commune fixed effects and male and age parameter differences	0.083 (3.6)*	–0.018 (1.0)	0.037 (1.8)**	1.246 (7.7)*	0.077 (0.6)	–0.200 (1.3)	0.143 [0.251]	7.4 [0.0000]	5.5 [0.0000]	14.3 [0.0000]	26.3 [0.0000]

* Significant at the 5 percent level.

** Significant at the 10 percent level.

Note: Absolute values for t-statistics are given in parentheses beneath point estimates.

a. Probabilities in square brackets.

b. Standard errors in square brackets.

c. Censored normal regression (with 8 right-censored and 2,617 uncensored observations for age when started school and with 2,173 right-censored and 442 uncensored observations for last completed grade). The R^2 is a pseudo R^2.

d. Regressions are with robust standard errors and with standard deviations corrected for household clusters. The overall test is an F-test, not a Chi^2 test. Root mean standard error is given beneath R^2.

e. Upper-limit tobit with upper limit at 1.0 and with 1,332 uncensored observations and 1,442 censored observations. The R^2 is a pseudo R^2.

Source: Calculated from data from the 1996 Vietnam Social Sector Financing Survey.

Table B-3. *Tobit Estimates of the Logarithm of Household School-Related Expenditures on the Logarithm of Predicted Income per Household Member*

	Elasticity with respect to income					
		Change for				
Private school-related expenditures	*Base*	*Ages 6–11*	*Male*	*Constant*	*Pseudo R^2*[a]	*Chi² test*[b]
School fees (N = 1,463)	1.528			−14.96	0.0001	3.7
	(1.9)**			(2.5)*	[12.4]	[0.0543]
School/commune fixed effects[c]	1.185			−12.35	0.068	346.2
	(1.1)			(1.5)	[10.6]	[0.0000]
Age and sex[d]	2.312	−2.228	0.190	−11.67	0.123	627.7
	(2.4)*	(1.6)	(0.2)	(1.6)*	[9.0]	[0.0000]
School/commune fixed effects and age and sex[c, d]	2.123	−0.448	−0.729	−12.28	0.202	1032.3
	(2.1)*	(0.4)	(0.7)	(1.7)**	[7.2]	[0.0000]
Total expenditures paid to schools (N = 1,553)	1.055			2.593	0.016	103.0
	(10.3)*			(3.4)*	[1.95]	[0.0000]
School/commune fixed effects[e]	0.300			4.61	0.182	1202.7
	(2.7)*			(5.7)*	[1.38]	[0.0000]
Age and sex[f]	1.063	−0.338	0.319	2.99	0.024	157.4
	(6.2)*	(1.7)*	(1.6)	(2.4)*	[1.92]	[0.0000]
School/commune fixed effects and age and sex[e, f]	0.243	0.015	0.221	5.45	0.200	1317.1
	(1.7)*	(0.1)	(1.5)	(5.2)*	[1.33]	[0.0000]
School-related expenditures not paid to schools (N = 1,553)	1.285			2.308	0.028	179.1
	(13.8)*			(3.4)*	[1.78]	[0.0000]
School/commune fixed effects[g]	0.527			6.84	0.209	1338.0
	(5.3)*			(9.5)*	[1.24]	[0.0000]
Age and sex[h]	1.223	0.133	0.039	3.12	0.034	216.0
	(7.8)*	(0.7)	(0.2)	(2.7)*	[1.76]	[0.0000]
School/commune fixed effects and age and sex[g, h]	0.336	0.508	0.038	8.58	0.227	1455.3
	(2.6)*	(3.9)*	(0.3)	(9.2)*	[1.19]	[0.0000]

* Significant at the 5 percent level.

** Significant at the 10 percent level.

Note: Conditional on current enrollment at time of survey. Absolute values for *t*-statistics are in parentheses beneath point estimates. Lower-limit tobit estimates are used because of left-censoring at zero (for school fees, 954 left-censored observations and 509 uncensored observations; for total school expenditures, 42 left-censored observations and 1,511 uncensored observations; for school-related expenditures not paid to schools, 26 left-censored observations and 1,527 uncensored observations).

a. Standard errors in square brackets.

b. Probabilities in square brackets.

c. *F*-tests indicate that school/commune fixed effects are significant ($F = 3.1$ for school/commune fixed effects and $F = 3.6$ for school/commune fixed effects and age and sex, probability in both cases 0.0000).

d. *F*-tests indicate that parameter dependence on age is significant even though individual additive and multiplicative parameter estimates are imprecise ($F = 223.5$, probability 0.0000 for age and sex; $F = 267.0$, probability 0.0000 for school/commune fixed effects and age and sex). *F*-tests indicate that parameter dependence on sex is insignificant ($F = 1.1$, probability 0.3458 for age and sex; $F = 0.9$, probability 0.4163 for school/commune fixed effects and age and sex).

e. *F*-tests indicate that school/commune fixed effects are significant ($F = 10.9$ for school/commune fixed effects and $F = 1.3$ for school/commune fixed effects and age and sex, probability in both cases 0.0000).

f. *F*-tests indicate that parameter dependence on age and sex is significant even though individual additive and multiplicative parameter estimates are imprecise ($F = 23.8$, probability 0.0000 for age and $F = 4.5$, probability 0.0111 for sex in age and sex; $F = 55.9$, probability 0.0000 for age and $F = 4.3$, probability 0.0130 for sex in school/commune fixed effects and age and sex).

g. *F*-tests indicate that school/commune fixed effects are significant ($F = 19.8$ for school/commune fixed effects and $F = 21.6$ for school/commune fixed effects and age and sex, probability in both cases 0.0000).

h. *F*-tests indicate that parameter dependence on age (though not on sex) is significant even though individual additive and multiplicative parameter estimates are imprecise ($F = 17.1$, probability 0.0000 for age and $F = 2.0$, probability 0.1368 for sex in age and sex; $F = 59.7$, probability 0.0000 for age and $F = 2.1$, probability 0.1255 for sex in school/commune fixed effects and age and sex).

Source: Calculated from data from the 1996 Vietnam Social Sector Financing Survey.

Table B-4. *Probit for School Enrollment on the Logarithm of Predicted Income per Household Member*

Alternative specifications	Coefficient of income: Base: female ages 12–17	Coefficient of income: Difference from base if Male	Coefficient of income: Difference from base if Ages 16–11	Constant: Base: female ages 12–17	Constant: Difference from base if Male	Constant: Difference from base if Ages 6–11	Pseudo R^2	Statistical tests[a]: Chi^2 test for overall significance	Statistical tests[a]: F-test: School/commune fixed effects	Statistical tests[a]: F-test: Male	Statistical tests[a]: F-test: Ages 6–11
Basic	0.000256 (6.3)* [0.152]			0.402 (5.6)*			0.017	39.3 [0.0000]			
Plus school/commune fixed effects	0.000332 (5.3)* [0.184]			1.167 (4.73)*			0.115	372.4 [0.0000]	314.2 [0.0000]		
Plus male and age parameter differences	0.000377 (5.6)* [0.194]	–0.000239 (2.9)* [0.071]	0.000141 (1.4) [0.267]	–0.277 (2.4)*	0.502 (3.7)*	0.959 (5.5)*	0.155	265.3 [0.0000]		14.9 [0.0006]	220.6 [0.0000]
Plus school/commune fixed effects and male and age parameter differences	0.000441 (4.5)* [0.196]	–0.000223 (2.5)* [0.092]	0.000112 (0.9) [0.246]	0.511 (1.9)	0.535 (3.4)*	1.214 (6.1)*	0.268	715.9 [0.0000]	358.0 [0.0000]	15.1 [0.0005]	288.8 [0.0000]

* Significant at the 5 percent level.

Note: $N = 2{,}789$. Standard errors are corrected for clustering at household level. Absolute values for z statistics are in parentheses beneath point estimates. Elasticity at means (enrollment = 0.791) are in brackets beneath standard errors (for males and ages 6–11 elasticities are for these categories, not for difference from base).

a. Probabilities are in brackets.

Source: Calculated from data from the 1996 Vietnam Social Sector Financing Survey.

Appendix C. Estimation of the Impact of Changes in Household Income on Future Income

We illustrate here the impact of changes in household income on future income for a child from a household that is one standard deviation above the mean and a child from a household that is one standard deviation below the mean of the distribution of household income per member (the example given in the fourth section of the paper). For these calculations we assume that the mean completed grades of schooling, S, is 7.5 grades, based on the completed schooling and enrollment rates of 17-year-olds in the sample; the real interest rate, r, is 5 percent; the retirement age, R, is 60 years; and the private cost of attending school for children is the children's time and the school-related expenditures of the household. Further, we assume that the students generate no income while in school and that, after school, annual income (or other returns from schooling), Y_S, is dependent on completed grades of schooling and is constant until retirement.

Delay in Starting School

The basic cost of starting school when a child is older is the delay in obtaining post-school returns. Consider the cost of a delay in finishing S grades of school in terms of the present discounted value of future income at an interest rate r evaluated at age six due to starting school when older. This cost is the difference in the present discounted value of future income with the delay and without the delay:

$$\text{(C-1)} \qquad \int_{S+D}^{R-6} e^{-rt} Y_S dt - \int_{S}^{R-6} e^{-rt} Y_S dt = \frac{Y_S}{r}\left[e^{-r(S+D)} - e^{-rS}\right]$$

where D is the delay in completing school beyond age $S + 6$. For the illustrative example considered in the fourth section of the article, the delay in starting school is 0.25 years, so that this expression equals $0.171 Y_S$.

Reduced Rate of Completing Grades

Reducing the rate of completing grades has two major effects on income. The first is a delay in obtaining the post-schooling returns from school, which has the same effect as a delay in starting school, so expression C-1 can be used to calculate this effect. For the illustrative example considered in the fourth section, D is 1.40 years for passing the average of 7.5 grades at a rate of 0.80 instead of 0.94, so this expression equals $0.929 Y_S$. The second effect is the monetary cost incurred by the household in terms of school-related expenditures. At the sample means, households' total school-related expenditures are 0.076 of annual parental household income per household adult, Y_H, which must be multiplied by 1.40 and discounted back to age six, yielding $0.071 Y_H$. Under the added assumption that average income does not change over time, so that $Y_S = Y_H$, the total present discounted value of the cost of this reduced rate of passing courses is $0.992 Y_S$.

Reduced Number of Completed Grades

A reduction in completed grades from S to S', also has two effects. First, the present discounted value of post-schooling income falls:

$$\text{(C-2)} \quad \int_{S+D}^{R-6} e^{-rt} Y_S dt - \int_{S'}^{R-6} e^{-rt} Y_{S'} dt = \frac{Y_S}{r}\left[e^{-rS} - e^{-r(R-6)}\right] - \frac{Y_{S'}}{r}\left[e^{-rS'} - e^{-r(R-6)}\right].$$

For the illustrative example considered in the fourth section, $S - S' = 2.2$. An estimated semilog income relation for the parents of the children in the sample yields $Y_S = 1{,}584$ and $Y_S' = 1{,}353$ for $S - S' = 2.2$, centered around the mean parental schooling level of 6.7 grades. [ln income = 0.072 schooling ($t = 14.7$) + 6.807 ($t = 156.4$), $R^2 = 0.227$, root mean squared error = 0.452, $F = 215.3$.] If these values are used to evaluate expression C-2 (again under the assumption that $Y_S = Y_H$), they imply a loss of income equal to $0.536Y_S$ at the sample means. Second, the household school-related expenditures also fall, under assumptions parallel to those above, by $0.120Y_S$ at the sample means. The net effect is a loss of $0.416Y_S$ at the sample means.

Reduced Exam Score in Last Completed Grade

For the illustrative example considered in the fourth section, exam performance falls about 7 percent, or about one-quarter of the sample variance. Because the data set does not have exam scores and income for the same individuals, we are not able to estimate the relationship between income and exam performance from the sample. We assume here that the effect on the present discounted value of post-schooling income is about one-quarter of the effect of completing an additional year of schooling, which implies a loss of $0.045Y_S$ at the sample means.

Impact as a Proportion of Present Discounted Value of Total Lifetime Wealth

Under the above assumptions the sum of these four effects is $1.624Y_S$ at the sample means. This compares with the present discounted value of lifetime income at the sample means:

$$\text{(C-3)} \quad \int_{S}^{R-6} e^{-rt} Y_S dt = \frac{Y_S}{r}\left[e^{-rS} - e^{-r(R-6)}\right]$$

which equals $12.402Y_S$ at the sample means. Therefore the sum of these four effects is 13.1 percent of lifetime wealth at the sample means.

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