



Stanford

Center for International
Development

Working Paper No. 123

**Economic Growth and the Demand for
Education:
Is there a Wealth Effect?**

by

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January 2002



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Abstract

Human capital investment in developing countries is thought to be significantly constrained by household resources. In this paper, we study the relationship between household resources and the demand for education using recently collected household survey data from Vietnam. Our data cover a period, 1993-98, of exceptional income growth in Vietnam, during which secondary school enrollment rose substantially. Using consumption expenditures as a proxy for household wealth, we find a positive and significant relationship between changes in wealth and changes in the demand for education. This wealth effect persists even after controlling for locality-specific factors such as changes in education returns and the supply and quality of schools, and for the opportunity costs of schooling.

I. Introduction

Education is often viewed as the principal route out of poverty in developing countries. Yet, poverty, it is also suggested, constrains schooling investment. If education were strictly an investment and households could borrow against future human capital, a family's financial resources should play no role in schooling decisions; only the returns to education should matter. In contrast, there are two scenarios under which households' financial resources would affect schooling decisions: 1. Education is strictly an investment good but households face credit constraints; and 2. No credit constraints exist but education is not only an investment good but also a consumption good.

This paper investigates whether there is an education "wealth effect" – an effect of a household's financial resources on its demand for schooling. The answer to this question is important because it tells us whether, and to what extent, households are borrowing constrained and/or value education as a consumption good. From a policy perspective, a wealth effect on the demand for education implies that schooling subsidies or loans, or even direct income subsidies, could enhance long-run social mobility. If, however, poverty does not constrain schooling, but instead the demand for schooling is low in developing countries simply because the returns to education are low, then such policies may be misguided, at least as efforts to raise educational attainment.¹ Policy-makers should, in this case, focus rather on fostering an environment of high education returns, while building more schools or improving existing ones, which households will take advantage of irrespective of their wealth.

Uncovering persuasive empirical evidence of a wealth effect is hampered by considerable obstacles. One major problem is endogeneity; namely, children from wealthier households may

¹ Foster and Rosenzweig (1996) find some support for this view using data from rural India.

have greater educational attainment merely because these children or their parents happen to be better endowed on some unobserved dimension (e.g., aptitude, motivation) than those from poor households. A second major problem is in choosing an appropriate measure of a household's "financial resources". Depending on the household's ability to borrow, current schooling decisions may depend on current income, past asset accumulation, and expected future income flows. Therefore, no single income or asset variable necessarily captures the wealth effect of interest.²

In this paper, we study the relationship between household resources and the demand for education using a recently collected nationwide household panel survey from Vietnam. Our data cover a period, 1993-98, of exceptional income growth in Vietnam, during which secondary school enrollment rose substantially. With panel data we can look at *changes* over time so as to disentangle the impact of household wealth from that of family background factors (endowments) that are presumed to be relatively stable over time. With a nationwide, but geographically clustered, survey we can also control for changes in the returns to education, and in the quality and supply of schools, to the extent that such changes are common across households residing in the same geographic location. In addition to child schooling, our data also provide detailed information on household consumption expenditures, which we use as a measure of current wealth or "permanent income". A number of other education demand studies based on cross-sectional data have also used consumption expenditures in this way (e.g., Glewwe and Jacoby, 1994; Behrman and Knowles, 1999), although without explicit theoretical justification. The main methodological contribution of this paper is, therefore, to spell out a set

² Foster and Rosenzweig (1996), for example, estimate a reduced form schooling decision rule that includes various agricultural production variables, some of which may capture current income, as well as household wealth, which captures past asset accumulation and, possibly, expected future income flows (to the extent that asset prices adjust).

of assumptions under which a wealth effect can be identified using data on consumption expenditures and schooling decisions.

The paper also illustrates the different ways to use panel data in analyzing schooling decisions. One approach is to examine changes in household average school enrollment rates across successive child cohorts (see, e.g., Foster and Rosenzweig, 1996). But this approach does not allow statements about how an increase in household resources will influence overall grade attainment. An approach that does allow such statements is to estimate a model of transition probabilities or duration of schooling. However, as we will see, dealing with the endogeneity of consumption expenditures is more complicated in the context of a duration model.

In the next section of the paper, we discuss the background of economic growth and educational progress in Vietnam. Section III then lays out a theoretical model that underlies our estimation and allows us to interpret the relationship between school attainment and consumption expenditures. Section IV details the empirical methodology, the results of which are reported in section V. Section VI concludes the paper.

II. Background: Economic Growth and School Enrollment in Vietnam

Starting in the late 1980's, Vietnam undertook a wide range of economic reforms (*Doi Moi*) that transformed it from a planned economy into a market economy. These policy changes led to rapid economic growth; in the decade preceding 1998, real GDP per capita rose at an average rate of 6% per year. Agricultural output alone increased by 4.5% per year, although employment in that sector expanded by only 0.4% per year in the 1993-98 period. This widespread economic progress rapidly increased household living standards. According to the

Vietnam Living Standards Surveys (VNLSS), implemented in 1992-93 and 1997-98,³ average real household consumption expenditures per capita rose by 7% per year from 1993 to 1998.

The two VNLSS surveys also show school enrollment increasing at all levels from 1993 to 1998. The percentage of children who eventually enter lower secondary school (grades 6-9) increased from 66% to 72%, and the percentage who entered upper secondary school (grades 10-12) increased from 23% to 31%.⁴ The vast majority of children in Vietnam attend primary school, but even here the enrollment rate increased by 5 percentage points across survey rounds.

These recent increases in enrollment stand in sharp contrast to the changes that took place from the late 1980s to the early 1990s. Although income growth was also robust during this earlier period, school enrollment at the secondary level *declined*. Table 1 shows this fact using school enrollment rates calculated from two different data sources, the official rates reported in the UNESCO statistical yearbook and rates calculated using retrospective data from the 1997-98 VNLSS. The UNESCO figures show that from 1988-93 the secondary gross enrollment rate *fell* from 44% to 32%, but rose steadily thereafter. The VNLSS data show a similar trend, although the timing is somewhat different. Both sources of data show an increase in enrollment starting in the early 1990s, reaching a gross rate of about 60% in 1997-98. In contrast, both data sources show a modest increase in the gross primary school enrollment rate during the 1990s.

The Vietnam experience suggests that income growth is not automatically accompanied by increases in human capital investment; clearly, other factors are at work. Glewwe and Jacoby (1998) conjecture that the opportunity costs of education rose dramatically in the late 1980s and

³ The VNLSS surveys are described in more detail in Section V.

⁴ These numbers refer to children age 16-17 and 19-20, respectively. These ranges are most informative about eventual entry into the corresponding levels of schooling because some children start school at a relatively late age and others repeat years of schooling. For example, some children age 14 and 15 are still in primary school and thus it is not clear whether they will enter lower secondary school.

early 1990s, which would explain the decline in secondary school enrollment rates. The objective of this paper is to understand the rapid increase in enrollment in the 1990s, focusing on the role played by economic growth.

III. Theoretical Model

In any empirical model of schooling decisions, assumptions must be made, either explicitly or implicitly, about: (1) labor markets, particularly for school-age children; (2) credit and insurance markets; (3) preferences; and (4) returns to education. This is not only true of structural models, but also of reduced form decision rules, since inevitably the choice of variables to include in such decision rules, not to mention their interpretation, must be guided by a theory. Our objective in this section is twofold, to define a wealth effect in the context of a fairly general human capital investment model and to lay out a set of theoretical assumptions that allow us to estimate this wealth effect using the data at hand.

In each period t households invest in two types of capital, human H_t and physical K_t . Human capital is accumulated by sending a fraction of the household's school-age children, e_t , to school and by purchasing a cash input (e.g., textbooks), x_t , at price p_t . Together, these inputs produce human capital according to

$$H_{t+1} = H_t + \psi_t G(e_t, x_t) \tag{1}$$

where G is a neoclassical production function and ψ_t is a learning productivity parameter that reflects factors such as school quality and child ability and motivation.⁵

⁵ The assumption that the stock of human capital is not “self-productive” (i.e., does not enter G) is important. Not only does it imply that the current enrollment decision does not depend on H_t , but it also simplifies the role of the shadow price of human capital in the model, as will be seen below.

Households generate current income Y_t by combining their physical capital with adult labor L_t^a and child labor L_t^c in the production function $Y_t = \theta_t F(K_t, L_t^a, L_t^c)$, where θ_t is a productivity parameter reflecting the state of the technology. For now, we assume that neither adults nor children work in the labor market. We also assume that households do not participate in credit markets (but relax this later). Thus, households finance all their investment by forgoing current consumption, c_t . In particular, physical capital accumulates according to

$$K_{t+1} = K_t + \theta_t F(K_t, L_t^a, L_t^c) - p_t x_t - c_t. \quad (2)$$

We also have the constraint $K_{t+1} \geq 0$, which says that the household cannot hold negative amounts of physical capital; this is equivalent to a “borrowing constraint”.

Consider a household with a given number of school-age children at time period zero. Suppose there is a fixed number of periods, $0 \leq t < T$, during which the children are eligible for school. In period T and beyond, children work exclusively and only then are the returns to education realized. Prior to this date, schooling does not augment children’s productivity. This assumption corresponds to the idea that school-age children, if they work at all, typically help out in the more menial tasks of the family farm or business and are not yet engaged in skilled labor or the management of an enterprise. Thus, utility over the remaining lifetime of the household (which would be infinite in a model of a dynastic household) may be written as a function $\Phi_T = \Phi_T(K_T, H_T)$ of the capital stocks, both human and physical, accumulated up until period T . This “terminal value” function, which households may be uncertain about (hence the T subscript), can incorporate some of the consumption benefits of having educated children, as well as the pecuniary benefits.

Current period household utility, U , is defined as a concave function of consumption, the leisure of adults and children (respectively $l_t^a \equiv 1 - L_t^a$ and $l_t^c \equiv 1 - L_t^c - e_t$), as well as of the school enrollment of children, thus allowing parents to contemporaneously value schooling. The household's objective is then to maximize

$$E_0 \left[\sum_{t=0}^{T-1} \delta^t U(c_t, l_t^a, l_t^c, e_t) + \Phi_T(K_T, H_T) \right] \quad (3)$$

subject to constraints (1) and (2), along with the borrowing constraint, where δ is the subjective discount factor and E_0 is the expectations operator with respect to information available to the household at time zero. Households are potentially uncertain about future values of ψ_t , θ_t , and p_t , as well as of Φ_T .

The first order conditions for an interior solution to this problem imply

$$U_c(t) = \lambda_t \quad (4)$$

$$U_{l^a}(t) = \lambda_t \theta_t F_{L^a}(t) \quad (5)$$

$$U_{l^c}(t) = \lambda_t \theta_t F_{L^c}(t) \quad (6)$$

$$U_e(t) + \mu_t \psi_t G_e(t) = \lambda_t \theta_t F_{L^c}(t) \quad (7)$$

$$\mu_t \psi_t G_x(t) = \lambda_t p_t \quad (8)$$

where λ_t and μ_t are the shadow values of physical and human capital, respectively, scaled by the discount factor δ^t . This system of equations can be solved to yield an enrollment demand function of the form

$$e_t^* = e^*(\lambda_t, \mu_t, \psi_t, \theta_t F_{L^c}(t), \theta_t F_{L^a}(t), p_t). \quad (9)$$

This enrollment demand function illustrates a major advantage of intertemporally additively separable preferences;⁶ namely, current period decisions depend on past and expected future prices and decisions only through the shadow values λ_t and μ_t .⁷ By the same token, the borrowing constraint, to the extent that it binds, affects intertemporal but not intratemporal decisions (conditional on λ_t); that is, the Lagrange multiplier on this constraint does not appear in equations (4)-(8), but only in the intertemporal Euler equation (see below).⁸

We can now trace through the potential impact of economic growth on school enrollment by considering an increase in current productivity θ_t ; this has four effects, according to equation (9). The first effect, and the one that is the focus of this paper, is through a fall the shadow price of physical assets λ_t , which occurs because resources available to the household have increased.⁹ As is usually the case, without further restrictions, this wealth effect on school enrollment cannot be signed *a priori*.

The second effect of an increase in θ_t is to change the shadow price of human capital. We may think of μ_t as the expected “return to schooling” for the following reason. The terminal condition on this shadow price requires that $\mu_T = \partial\Phi_T / \partial H_T$, which is just the marginal value of

⁶ Equation (9) is analogous to the life-cycle labor supply functions discussed by Heckman (1974,1976), MaCurdy (1981,1983), Browning, Deaton, and Irish (1985), and Blundell and Walker (1986), among others.

⁷ We can go further and write $e_t^{**} = e^{**}(\lambda_t, \mu_t, \psi_t, \theta_t, K_t, p_t)$, but, as will become apparent, this form of the enrollment demand function is inconvenient for estimation. Essentially, by conditioning on the physical capital stock, we lose our main source of identification.

⁸ Meghir and Weber (1996) use this fact that borrowing restrictions do not affect intratemporal allocations conditional on λ_t as the basis of a test for the presence of borrowing restrictions.

⁹ This wealth effect should be distinguished from changes in school enrollment that are part of the household’s initial lifetime plan. For example, to the extent that children become more productive workers as they age, households will plan on reducing child enrollment over time. Indeed, much of the enrollment changes we observe

human capital after the schooling period. Moreover, it is straightforward to show that

$\mu_t = E_t \mu_T$.¹⁰ Therefore, the effect of a change in θ_t on μ_t , and ultimately on e_t , depends on the specific form of Φ_T . For example, if H_T and K_T are complements in the production of future income, then to the extent that a higher θ_t implies greater physical capital accumulation, $\partial\Phi_T/\partial H_T$ will also be higher and the demand for schooling will rise.

The third and fourth effects of an increase in productivity operate through the current shadow prices of adult and child time. An increase in θ_t raises $\theta_t F_{L^c}(t)$, which lowers school enrollment. At the same time, it also raises $\theta_t F_{L^a}(t)$, which exerts a cross-price effect of unknown sign in equation (9).

To make progress in isolating the wealth effect, we must impose three additional restrictions:

(i) Normality of consumption: $-\partial c_t^*/\partial \lambda_t > 0$, where $c_t^* = c^*(\lambda_t, \mu_t, \psi_t, \theta_t F_{L^c}(t), \theta_t F_{L^a}(t), p_t)$, as in equation (9). Although certainly plausible, normality of total consumption must be assumed, as it not guaranteed by the concavity of the utility function.

(ii) Adult and child labor are perfect substitutes (though not necessarily equally productive at the margin): That is, $Y_t = \theta_t F(K_t, L_t)$, where $L_t = L_t^a + \gamma L_t^c$ and $0 < \gamma < 1$ reflects the lower productivity of child labor. Thus, $F_L = F_{L^a} = F_{L^c}/\gamma$, which means that the own and cross price (with respect to adult labor) effects of the shadow wage on child enrollment are combined into

as a given child ages may be for this reason, rather than due to shifts in λ_t during the child's schooling career. We return to this issue in the next section.

¹⁰ This Euler equation for the shadow price of human capital is more complicated if H_t interacts with e_t in the G function. In this case, μ_t loses its simple interpretation because human capital is also valuable during the schooling period.

one effect. Note that if adults participate in the labor market, the shadow wage would be replaced by the market wage rate.

(iii) Linearity of the terminal value function in human capital: $\Phi_T = \omega_T H_T + \varphi_T(K_T)$, which leads to $\mu_t = E_t \omega_T$. In particular, this assumption says that the expected returns to schooling does not vary across households according to the physical or human capital that has been accumulated up to period T . This is equivalent to Foster and Rosenzweig's (1996) assertion of an agricultural profit function linear in schooling (or, analogously, to an earnings function linear in schooling). As in that paper, we allow the return parameter ω_T to vary across localities, as might be the case if economic growth occurs at different rates in different regions.

Using assumptions (ii) and (iii), we obtain

$$e_t^* = e^*(\lambda_t, E_t \omega_T, \psi_t, \theta_t F_L(t), p_t). \quad (10)$$

Differentiating equation (10), as well as the analogous equation for consumption, then yields

$$\frac{\partial e_t^*}{\partial \log(c_t^*)} = - \frac{\partial e_t^*}{\partial \log(\lambda_t)} \bigg/ - \frac{\partial \log c_t^*}{\partial \log(\lambda_t)}. \quad (11)$$

In words, the response of school enrollment to a one percent increase in total consumption is proportional to the response of enrollment to a one percent *decline* in the shadow price of assets, where the constant of proportionality is the inverse of the shadow price elasticity of consumption (evaluated at the optimum). The latter term is positive by assumption (i). Thus, although the relationship between enrollment and consumption does not, strictly speaking, identify the wealth effect (since marginal utility is ordinal), it does deliver the *sign* of the wealth effect. Certainly, in the absence of a wealth effect, one should find that $\partial e_t^* / \partial \log(c_t^*) = 0$.

Note that, under our assumptions, household consumption reflects all information about past, present, and expected future household resources that is relevant for the current enrollment

decision. This result is entirely independent of the structure of credit or insurance markets; it applies equally to households that can borrow as it does to households that cannot borrow. The ability to borrow (and insure) does determine how household consumption moves over time, which is important for the identification strategy, as discussed in the next section. In particular, the intertemporal Euler equation for our problem is

$$\lambda_t = \delta E_t(\lambda_{t+1} + v_t)(1 + \theta_t F_K(t)). \quad (12)$$

where v_t is the multiplier on the borrowing constraint, $K_{t+1} \geq 0$. The implications of equation (12) are most easily seen when the borrowing constraint is not binding ($v_t = 0$),¹¹ preferences between consumption and other goods are additively separable, and labor is not a factor of production. In this case, by equation (4), λ_t is only a function of c_t^* and $F_K(t)$ depends only on the physical capital stock. Equation (12) then implies that consumption growth is slower for households with a high initial capital stock. In the general case, the physical capital stock in period t determines how consumption evolves from period t to $t + 1$, albeit in a complicated way.

This link between the household's capital stock and ex-post consumption growth is weakened, though not necessarily eliminated (as we argue in the next section), when households can borrow and lend using a financial asset. In this case, $v_t = 0$ and $\theta_t F_K(t)$ is replaced by the market interest rate in (12). Consumption growth is now determined by this interest rate and by unanticipated shocks to productivity or prices, to the extent that these shocks are uninsurable ex-ante. Finally, if households can borrow as well as somehow insure their consumption against every type of idiosyncratic and economy-wide shock, λ_t would be constant across households

¹¹ In addition to the conventional borrowing constraint, equation (12) incorporates a second credit market imperfection. Namely, households do not have access to a financial asset with a fixed return; they can only invest in physical capital, in which there is diminishing marginal returns.

and over time; only in this implausible case would there be no wealth effect on school enrollment due to productivity or price changes.

IV. Estimation Framework

Empirical specification

Empirical implementation of equation (10) requires additional assumptions about functional form and the nature of the unobservables. Taking a semi-log approximation and adding household (i) subscripts gives the regression equation

$$e_{it} = b_1 \log(c_{it}) + b_2 \log(\theta_{it} F_{Lit}) + b_3 p_{it} + b_4 E_t \omega_{Tv} + \psi_{it}. \quad (13)$$

Note that, according to equation (11), the parameter b_1 is the enrollment wealth effect scaled by the inverse of the consumption wealth effect. Expected returns to schooling are assumed to vary, if at all, by locality v , as discussed above, and so are captured by a locality-year intercept. The error term in this regression consists of unobserved learning productivity ψ_{it} , but could also reflect unobserved preferences for schooling or consumption, which we suppressed in the previous discussion for the sake of exposition.

Suppose, for the moment, that the shadow price of child time, $\log(\theta_{it} F_{Lit})$, is observed. Ordinary least squares estimates of equation (13) would not necessarily yield consistent estimates of the wealth effect, b_1 , because $\log(c_{it})$ is endogenous, i.e., correlated with ψ_{it} . A strategy adopted by Altonji (1986) to estimate an analogous labor supply function (for adult males) is to use parental wealth variables as instruments for $\log(c_{it})$. While we take a similar approach below, strictly for illustrative purposes, it is not obviously defensible in the present context because past asset accumulation may also be correlated with ψ_{it} . For example,

households with a high preference for current consumption may invest less in both physical and human capital. Suppose, however, that we have two rounds of panel data, one round collected in year t and the other in year $t-k$. Further, assume that the error term can be decomposed as follows $\psi_{it} = \eta_i + \varepsilon_{it}$ into a permanent household-specific effect and a transitory shock.

Differencing equation (13) across rounds gives

$$\Delta e_{it} = b_1 \Delta \log(c_{it}) + b_2 \Delta \log(\theta_{it} F_{Lit}) + b_3 \Delta p_{it} + d_v + \Delta \varepsilon_{it} \quad (14)$$

where Δ is the first-difference operator, and we have replaced $\Delta E_t \omega_{Tv}$ by the locality-specific intercept d_v .

While first-differencing eliminates permanent differences in the household learning environment and/or preferences for schooling that might be correlated with wealth and consumption (i.e., η_i), there is still the potential problem that consumption is correlated with transitory enrollment shocks (not to mention the problem of measurement error in consumption). The strategy, in this case, is to use year $t-k$ asset stocks, both physical and human, as instruments. Such variables should be unrelated to subsequent shocks and potentially correlated with consumption growth. As already pointed out in the discussion of equation (12), consumption growth generally depends on initial capital in our model. Even if we depart from our basic assumptions and allow households to have access to a financial asset, which they can also borrow, consumption growth would still arguably depend on their initial capital stocks. In particular, households may not fully anticipate the effects of economic growth, as they might the growth of income over their life-cycle. If so, this economic growth would represent, at least in part, an uninsurable income shock. Furthermore, the extent to which households are “exposed” to this shock may vary according to prior investments in physical and human capital (e.g., farmers with a lot of land suitable for rice may benefit relatively more from trade liberalization).

The discussion thus far assumes that $\log(\theta_{it} F_{Lit})$ is known, whereas in fact it must be estimated. To do so, assume that F is Cobb-Douglas. To be general, we can allow all the parameters to vary across households and time so that $Y_{it} = \theta_{it} K_{it}^{\alpha_{it}} L_{it}^{\beta_{it}}$, which gives $\log(\theta_{it} F_{Lit}) = \log(\beta_{it}) + \log(Y_{it} / L_{it})$.¹² Substituting this into equation (14), we have

$$\Delta e_{it} = b_1 \Delta \log(c_{it}) + b_2 \Delta \log(Y_{it} / L_{it}) + b_3 \Delta p_{it} + d_v + \Delta \tilde{\varepsilon}_{it} \quad (15)$$

where $\Delta \tilde{\varepsilon}_{it}$ now incorporates shocks to the output elasticity of labor ($\Delta \log(\beta_{it})$) as well as measurement error in the shadow wage. Clearly, $\Delta \log(Y_{it} / L_{it})$ is endogenous, but we can again use year $t-k$ asset stocks as instruments. These variables should be correlated with the shadow wage through the production function, but uncorrelated with $\Delta \tilde{\varepsilon}_{it}$ as long as innovations in $\log(\beta_{it})$ are not anticipated by households prior to year $t-k$.

Using panel data

There are two approaches to using panel data on child school enrollment in the estimation of equation (15). The first approach follows Foster and Rosenzweig (1996) by specifying a household-level school enrollment equation for successive cohorts of children. This approach has the advantage of simplicity, but it is not informative about how wealth influences the time-path of schooling. Thus, our second approach uses the child-level data to estimate a duration model, which allows us to estimate the impact of wealth on ultimate grade attainment. Each

¹²In the empirical work, we aggregate adult and child labor using a fixed value of γ (see assumption (ii) above). In principle, it is possible to estimate γ , but this would require nonlinear methods. For an application of this shadow wage methodology using less restrictive specifications of the technology see Jacoby (1993).

approach “differences” out η_i , the fixed component of ψ_{it} , in equation (13), and thus deals with the endogeneity problem, but in a different way.

In the household-level enrollment approach, we select households that have at least one child in a given age range, say 10-18, in both rounds of the panel and then we use average current school enrollment of those children for each household in each round. In this way, average enrollment can be differenced over time for each household, as can the explanatory variables. Foster and Rosenzweig choose the age range 5-14, young enough so that none of the children appearing in the first round of their survey reappear in the second round 11 years later. In other words, they choose non-overlapping cohorts. To see why this might be a good idea, consider what happens when the same children appear in both panel rounds. In the extreme case where there is only a single child in a household, one would end up differencing enrollment over time for the same child. But, as mentioned earlier, such enrollment changes, rather than being a consequence of growth-induced wealth effects, mainly capture a given child’s progression through school.

The problem of overlapping cohorts can be avoided in two ways. One can, of course, simply choose non-overlapping cohorts, but this can dramatically limit the sample of households, as we show below. Alternatively, one can include (average) grade attainment as a regressor in equation (15). Controlling for grades already completed removes the effect of school progression on enrollment for those children who appear in both rounds. However, since grades completed in the most recent round may be correlated with past enrollment shocks (introduced into equation (15) by first-differencing), there is an additional endogeneity problem. This problem can be dealt with by adding grades completed in the initial round to the instrument set.

Our child-level approach uses the Cox proportional hazard, or partial likelihood, model for schooling durations. This model takes into account the dependence of each child's current enrollment on past grade attainment. The baseline hazard function, which captures this dependence, is not specified parametrically. Moreover, the fixed effect η_i is removed by essentially differencing across siblings within the same family, as in Chamberlain's fixed effect logit model.¹³ This *stratified* partial likelihood estimator is described in a recent paper by Ridder and Tunali (1999). As with our first approach, this estimator imposes sample selection rules. In particular, only households with at least two children in the age cohort under consideration contribute to the likelihood. Importantly, at least one of these children must have left school during the period of observation (i.e. at least one observation must not be censored), a condition that, as we will see, severely limits the number of households contributing to the likelihood.

An additional complication is that consumption expenditures is a (calendar) time-varying regressor in the duration model. However, we only observe expenditures at two points in time, corresponding to the 12 months immediately preceding the date of interview in each survey.¹⁴ To interpolate expenditures for the intervening years, we assume that consumption grows at a constant rate between the two surveys, and that consumption was stable prior to the first survey. The impact of consumption expenditures is identified off of the fact that, within a household, it varies across children of a given age since children are born in different calendar years. In other words, identification comes from the interaction of consumption growth and differences in sibling ages.

¹³ However, there is an important difference: unlike the fixed effect logit, the proportional hazard model uses information on the rank order of schooling durations within each household.

¹⁴ This issue does not arise in our first approach because there we are only concerned with enrollment at two points in time rather than with the entire time path of enrollment.

Even though it removes the household fixed effect, the stratified partial likelihood estimator does not deal with endogenous regressors such as consumption expenditures. We use full information maximum likelihood (FIML), in which a first-stage equation for consumption growth between surveys is estimated jointly with the hazard model. The cross-equation correlation is modeled nonparametrically using a single factor discrete approximation (Heckman and Singer, 1984; Mroz, 1999). Essentially, we introduce a household-level error term into the hazard function that interacts with calendar time. This error term also appears in the consumption growth equation, but with a different factor loading. The likelihood function is derived in the Appendix.

V. Empirical Results

Data

The data used in our empirical analysis are from the 1992-93 and 1997-98 Vietnam Living Standards Surveys (VNLSS). Both surveys are nationally representative and provide detailed information on a wide range of topics, including education, labor market activities, farm and non-farm production, and consumption expenditures. The 1992-93 survey sampled 4800 households and the 1997-98 survey covered 6000 households. Most importantly for our analysis, 4305 households were interviewed in both years, providing a large panel data set with a household attrition rate of only 8.5%.¹⁵ For more general information on these surveys see World Bank (1995, 2000).

The surveys are geographically clustered at the level of the commune. We do not exclude urban communes, but in any case Vietnam's population is around 80% rural. Our

¹⁵ Of the 4800 households in the 1992-93 survey, 96 were dropped due to changes in the sample design, thus the attrition rate refers to the 4704 households that were targeted for reinterviews in 1997-98.

sample comprises 147 communes located in 58 of the country's 61 provinces (the survey does not cover 3 small provinces). Each estimation approach uses different subsamples of the 4305 panel households depending on the characteristics of children present in each round, as described below.

The key variables in the empirical model are household consumption, which captures the wealth effect, and the log shadow wage, $\log(Y_{it} / L_{it})$, which captures the substitution effects. Consumption includes food, non-food, and imputed service flow from durables and from owner-occupied housing. It excludes direct education expenditures, which, in the model, are part of x_{it} (see equation (1)) rather than part of c_{it} . The theory is silent on the precise way in which household demographics affect the utility obtained from (total) household consumption. In most of the empirical analysis, we use *per-capita* expenditures as our measure of household consumption, but we also use *total* expenditures to check the robustness of our results. The shadow wage variable is constructed using data on the total value of output from, and family labor supplied to, farm and non-farm enterprises. Most households report some self-employment output, if only from home gardens in urban areas, but we drop 99 households from our main sample (see below) that have no such output of any kind in one or both years. Among children age 10-18, 44% report in the 1998 survey that they worked in the past 12 months, and all but 8% of these children worked on their family farm or business. Adult and child labor is aggregated into efficiency units using $\gamma = 0.5$ (see assumption (ii) in section III), but we also experiment with alternative values of γ and find only negligible changes in our results.

Finally, we looked for variables in the survey representing the price of school supplies p_{it} or, more generally, the quality of schooling. An important criterion is that the variables be comparable across the two rounds of the survey. Distance of the commune to the nearest

secondary school was one candidate, but there were too many missing values to be useable. In the end, we had to abandon the exploration of school characteristics.¹⁶ In the empirical work, therefore, we must assume that to the extent that these characteristics vary in the sample they do so mainly across localities, rather than within localities.

Household-level school enrollment

Table 2 reports the household-level enrollment results for the 10-18 year-old age cohort; children begin dropping out of school in significant numbers only after age 9. This cohort is overlapping, in the sense of Section IV, because most of the 10-13 year-olds from the first panel round in 1993 also appear in the second round in 1998 (as 15-18 year olds). As discussed above, we address this problem by conditioning on the household average of grades completed in the cohort. Only children of the household head are included in the sample and only households with at least one child in this age cohort in both panel rounds contribute for a total sample of 1485 households.¹⁷ On average, for the panel sample there are 2.2 children age 10-18 per household in 1993 and 2.1 in 1998. We also constructed a panel based on a non-overlapping cohort of children age 14-18. The youngest children in this cohort in 1993 are too old to be included in the second round cohort in 1998. However, the sample for this panel falls to 864 households, or by more than 40 percent, with serious efficiency costs. In particular, although our results are similar to those in Table 2, the estimates are considerably less precise, so we do not report them in this paper.

¹⁶ Glewwe and Jacoby (1998) examine the effect of school quality on enrollment in Vietnam using the 1993 data.

¹⁷ Since we have detailed data on schooling and household consumption expenditure only for children who are living with their parents, we must exclude those living away from their parents' home. However, only 3% of children in the 10-18 age cohort are not living with their parents (8% of 17-year olds and 14% of 18-year olds), so this exclusion should not create a serious sample selection bias.

Before turning to our main results, it is instructive to estimate the counterpart to equation (15) in *levels* (rather than in differenced form). Something like this specification has appeared in past studies of education demand, most of which are based on cross-sectional data. We use the 1993 survey round, including non-panel households, for a sample size of 1598. Per-capita expenditures, the shadow wage, and mean grades completed are all treated as endogenous in specifications (1) and (2) of Table 2, with a set of farm and non-farm enterprise asset variables from the 1993 survey used as instruments (see notes Table 2). The results are sensible: the wealth effect is positive and significant, while the shadow wage effect is negative and significant. Recall that given the efficiency units specification (assumption (ii) in section III), the substitution effects of child and adult shadow wages are combined into one effect of theoretically ambiguous sign. However, one might expect the negative own shadow wage substitution effect to dominate the cross-substitution effect, and the results in specification (1) appear to support this.

Specification (2) includes province dummies in the levels specification to control for differences in the rate of return to education and school supply and quality. Although the province dummies have considerable explanatory power, the wealth effect remains significantly positive and the substitution effect significantly negative. We also tested whether adding commune dummies in place of province dummies improves the explanatory power of the regression. The answer is no ($p\text{-value}=0.45$), which suggests that most of the variation in education returns or school quality and availability is at the province level. Of course, as discussed above, these levels specifications make the strong assumption that household asset accumulation (in the form of the excluded instrument set) is uncorrelated with the household

fixed effect η_i . To see what happens to our estimates of the wealth effect when we relax this assumption, we turn next to the results based on differences over time.

Specifications (3)-(5) of Table 2 are 2SLS regressions of *changes* in household average enrollment on changes in the log of per capita expenditures and other variables. The excluded instrument set is essentially the same as in specifications (1) and (2), with two additions: education of the household head in the initial round (included in the second stages of specifications (1) and (2)) and mean grades completed in the initial round. Recall from our discussion in Section III that initial (i.e., 1993) round asset variables should be correlated with consumption growth between 1993 and 1998 to the extent that households are unable to borrow against future income or are uninsured against income shocks. A test of instrument relevance (i.e., significance of the 14 excluded instruments) yields an F-value of 9.62 for the consumption growth equation and an F-value of 104.6 for the shadow wage growth equation. However, these F-statistics are lower when province dummies are included, 4.05 and 69.1, respectively.

Specification (3), without province dummies, again shows a positive and significant wealth effect, one that is not much different from its levels counterpart in specification (1).¹⁸ This latter finding suggests that bias due to the correlation of household wealth with household level unobservables that affect education demand are not important in this sample. Note also that the shadow wage coefficient is now positive, though not significantly different from zero. One problem with specification (3) is that the overidentifying restrictions are rejected (p-value = 0.02), which casts some doubt on the instruments. Interestingly, when we exclude mean grades completed in specification (4), the overidentifying restrictions are resoundingly rejected (p-value

¹⁸ It is worth noting, however, that the OLS estimate of this same differenced regression produces a coefficient on log expenditures of only 0.093 (3.91). This downward bias in the OLS estimate is consistent with measurement error in consumption expenditures.

= 0.003) and the coefficient on log per capita expenditure rises considerably. Part of the reason for this rejection is that the education of the head, which is among the identifying instruments, is significantly and positively correlated with the second stage residual, which now includes the effect of cohort average grades completed.

Next, we add province dummies, in specification (5), to take out the variation in both (changes in) the returns to and the supply of education. Notice that the coefficient on log per capita expenditures remains significant and it increases by nearly 50 percent in magnitude, a similar increase as in the levels specifications (1) and (2). This occurs despite the fact that the province dummies are not even jointly significant at the 0.05 level (also, adding commune dummies provides no additional explanatory power; p-value = 0.37). Importantly, with the inclusion of the province dummies, the overidentifying restrictions cannot be rejected. Finally, the coefficient on the shadow wage variable remains positive, but does not even approach significance.

We also reestimated specification (5) using total household expenditures, rather than per-capita expenditures, with similar results; specifically, the estimated wealth effect is 0.217 (1.63). Thus, our results seem to be robust to the form in which household demographics affect the utility derived from household expenditures.

Child-level schooling duration

Table 3 presents the results of the partial likelihood analysis of schooling durations on a sample of children ages 10-18.¹⁹ Since schooling durations are censored for the many children who are still enrolled in school, the effective number of households contributing to the likelihood

¹⁹ The estimates are conditional on a child having been enrolled in school at 9 years of age, but this is true of all but 3% of children in the 1997-98 survey.

is only 423, much smaller than even the non-overlapping cohort sample. On the other hand, the latter estimator only uses information on household averages, whereas the duration model uses data on individual children. We do not include province dummies (interacted with the sibling age difference) in the hazard function, as these had little explanatory power in the earlier regressions. We also omit the shadow wage variable, which was insignificant in Table 2 as well, since its inclusion would require an additional first-stage equation to be jointly estimated.

The first specification in Table 3 reports the maximum likelihood estimates of the parameters of the hazard function in which expenditures is treated as exogenous. The negative sign on log per capita expenditures is consistent with our earlier findings; higher wealth lowers the hazard rate of leaving school. However, the wealth affect is statistically much weaker than before, undoubtedly due to the large decline in sample size. When expenditure changes are treated as endogenous, the estimate of the wealth effect increases somewhat, but remains statistically weak.²⁰ In sum, our child-level duration approach is not very informative, given the limited sample information it exploits (less than a third of the sample of *households* used in Table 2). However, it is still worth discussing how these estimates can be used to simulate the impact of economic growth on grade attainment.

Using an auxiliary regression procedure outlined by Ridder and Tunali (1999), we first recover a nonparametric estimate of the baseline hazard, which indicates how rates of school leaving vary with time in school. We then calculate the predicted survivor function, which represents the probability of remaining in school at each age given enrollment up to that time. Our model predicts that 50 percent of 18 year-olds are still in school, which is somewhat higher than the actual figure of 38 percent. In any case, we can then ask what would be the impact of a

²⁰ Only two points of support are needed to approximate the underlying heterogeneity distribution, with one of the points having the very low probability of 0.01.

50 percent increase in wealth; that is, in per capita consumption expenditures. About 58 percent of 18 year-olds would now remain in school, which translates into about a quarter of a year increase in expected time in school, from 7.53 to 7.81 years.²¹ Of course, some children remain in school after age 18, so this figure probably understates the total wealth effect on eventual grades completed.

VI. Conclusions

Using panel data from Vietnam, we find that child school enrollment increased faster over the 1993-98 period in households that experienced greater increases in wealth. This finding persists even after controlling for locality-specific factors such as changes in education returns, and in the supply and quality of schools, and for changes in the opportunity costs of schooling. To get a better sense of the magnitude of the wealth effect, we also estimate a duration model. These estimates show that, as a result of the economic growth during 1993-98, grade attainment rose by about a quarter of a year.

According to our results, rising returns to education did not play much of a role in increasing education demand in Vietnam during the mid-1990s, contrary to what Foster and Rosenzweig (1996) find for rural India during the Green Revolution era. In our analysis, as in theirs, changes in the returns to education are assumed to vary only by locality, but in our case there is no significant difference in enrollment growth across provinces. One interpretation of this finding is that changes in the returns to education actually vary more across households within the same village than across villages; in this case, the return effect is confounded with the

²¹ Note that we equate time (years) in school with grades completed, which is justified for the 10-18 age cohort because the vast majority of grade repetition occurs early in primary school.

wealth effect. Alternatively, changes in the returns to education may not vary much at all in our sample and end up in the constant term of our regressions. In this regard, it is worth noting that, based on the wealth effect alone (using specification (5) of Table 2), we predict an increase in school enrollment of about 10.3 percentage points for children age 10-18 over the 1993-98 period, compared to the actual increase of about 8.1 percentage points. Thus, there appears to be little scope for higher returns to education to explain enrollment growth in Vietnam. Perhaps the returns to education really did not change much during this period of economic liberalization, or perhaps households had not yet responded to the higher returns.

Although our finding of a significant wealth effect does not allow us to say with certainty whether households in Vietnam are borrowing constrained or merely value school enrollment for its own sake, it does have important implications. First, it suggests that economic growth and human capital accumulation reinforce one another; that is, while education leads to growth, growth, in turn, raises the demand for education. Second, it indicates that any policy that makes a household wealthier will also lead it to educate its children more, thereby increasing wealth in the succeeding generation.

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Appendix

Likelihood Function for FIML Estimation of Duration Model

Let the hazard function for child i in family j be

$$h_{ij}(t) = \omega_{ij}(t)h_j(t) \quad (\text{A.1})$$

where $\omega_{ij}(t) = \exp(\beta'x_{ij}(t))$ and $h_j(t)$ is the family specific baseline hazard rate, which we assume takes the following additive form $h_j(t) = \alpha_j + h(t)$. The parameter α_j is the household fixed effect. The vector of covariates, $x_{ij}(t)$, includes the change in log expenditures, Δc_j , interacted with calendar time. Ignoring endogenous regressors, the contribution of family j to the partial likelihood (see Ridder and Tunali, 1999) is

$$L_j = \prod_{i=1}^{I_j} \prod_t \left[\frac{\exp(\omega_{ij}(t))}{\sum_{k=1}^{I_j} Y_{kj}(t) \exp(\omega_{kj}(t))} \right]^{d_{ij}(t)} \quad (\text{A.2})$$

where I_j is the number of children in family j , $d_{ij}(t)$ is an indicator for whether the child has exited school in period t and $Y_{ij}(t)$ is an indicator for whether the child is still under observation in period t (e.g., once a child leaves school he is no longer under observation). The sum over families of $\log L_j$ forms the partial likelihood that we maximize in specification (1) of Table 4.

To deal with the endogeneity of consumption expenditures, we introduce an error term that interacts with calendar time. Without loss of generality, we may write $\omega_{ij}(t, \rho_1, v_m) = \exp(\beta'x_{ij}(t) + \rho_1 v_m t)$, where ρ_1 is a factor loading parameter and v_m is a discrete random variable taking on M values, $m=1, \dots, M$, each with probability π_m . We also have a first-stage regression for the log consumption change, $\Delta c_j = \gamma'z_j + \rho_2 v_m + \varepsilon_j$, where ρ_2 is another factor loading parameter and ε_j is an $N(0, \sigma^2)$ error term. Taking the probability weighted sum across the possible values of v_m , the likelihood contribution of family j becomes

$$L_j = \sum_{m=1}^M \pi_m \left\{ \prod_{i=1}^{I_j} \prod_t \left[\frac{\exp(\omega_{ij}(t, \rho_1, v_m))}{\sum_{k=1}^{I_j} Y_{kj}(t) \exp(\omega_{kj}(t, \rho_1, v_m))} \right]^{d_{ij}(t)} \right\} \frac{1}{\sigma} \varphi \left(\frac{\Delta c_j - \gamma' z_j - \rho_2 v_m}{\sigma} \right) \quad (\text{A.2})$$

where φ is the univariate normal pdf. Maximizing the sum of the $\log L_j$ over families yields the FIML estimates in Table 4.

Table 1
School Enrollment Rates in Vietnam

Year	UNESCO Data (Gross Rates)			1997-98 VNLSS			
	Primary	Secondary	Tertiary	Primary Net	Primary Gross	Secondary Net	Secondary Gross
80/81	109	42	2	-	-	-	-
81/82	-	-	-	-	-	-	-
82/83	-	-	-	-	-	-	-
83/84	-	-	-	-	-	-	-
84/85	104	40	-	-	-	-	-
85/86	103	43	2	62	75	39	44
86/87	104	-	-	66	82	37	41
87/88	106	44	-	68	85	35	39
88/89	104	40	2	68	85	32	35
89/90	102	35	2	72	89	32	35
90/91	103	32	2	74	91	32	35
91/92	104	31	2	76	93	35	38
92/93	109	32	2	79	96	37	41
93/94	111	35	2	81	102	41	44
94/95	113	41	3	85	107	44	48
95/96	114	47	4	87	110	48	53
96/97	115	52	7	89	113	53	58
97/98	113	57	-	92	113	57	63

Table 2
Wealth Effects on Average Household School Enrollment: Ages 10-18

	2SLS/1993 levels		2SLS/1993-98 differences		
	(1)	(2)	(3)	(4)	(5)
<i>Household variables</i>					
Log(per-capita expenditures) ^a	0.255 (3.35)	0.255 (3.35)	0.202 (2.48)	0.269 (3.01)	0.297 (2.12)
Log (Y/L) ^a	-0.079 (2.41)	-0.079 (2.41)	0.007 (1.37)	0.006 (1.07)	0.002 (0.31)
<i>Cohort variables</i>					
Mean grades completed ^a	0.065 (2.96)	0.065 (2.96)	0.077 (3.73)	---	0.073 (3.15)
Mean age	-0.153 (11.8)	-0.153 (11.8)	-0.148 (12.6)	-0.107 (22.7)	-0.145 (11.4)
Proportion males	0.080 (4.19)	0.080 (4.19)	0.125 (4.91)	0.128 (4.78)	0.124 (4.72)
Grades completed of household head	0.002 (0.35)	0.002 (0.35)	---	---	---
<i>Specification tests</i>					
Province dummies joint test: p-value	---	0.06	---	---	0.09
Overidentification test: p-value	---	---	0.02	0.003	0.40

Notes: Absolute t-values in parentheses. Dependent variable is household average school enrollment for all children age 10-18. All regressions include a constant. Sample size is 1598 households for columns (1) – (2) and 1485 households for columns (3) - (5).

^aEndogenous variable. Instruments include annual cropland holding and interactions with proportion of cultivated land that is irrigated and of good and medium quality, other landholding, value of farm equipment, value of farm animals, value of nonagricultural business assets, highest grade completed of household head, and, for specifications (3) and (5), mean grades completed in the cohort in the 1992-93 survey.

Table 3
Wealth Effects on Schooling Duration: Ages 10-18

	ML	FIML
Log(pcxp) ^a	-0.491 (1.16)	-0.613 (1.36)
Age of child in 1998	0.299 (9.09)	1.11 ^b (0.61)
Child is male	-0.564 (4.08)	-0.563 (4.07)
Log-likelihood	-364.4	-558.3

Notes: Coefficients are for hazard function (see Appendix). Absolute t-values in parentheses. A total of 423 households contribute 3838 child/time observations to the likelihood.

^aEndogenous variable in FIML. Instruments include annual cropland holding and interactions with proportion of cultivated land that is irrigated and of good and medium quality, other landholding, value of farm equipment, value of farm animals, value of nonagricultural business assets, value of owned house, and highest grade completed of household head.

^bCoefficient has random component due to time-varying heterogeneity and therefore the overall age coefficient takes a value of 0.298 with probability 0.99 and a value of 1.91 with probability 0.01.