Determinants of Schooling Outcomes among Children from Rural Ethiopia*

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Abstract: Investment in schooling is associated with higher economic and non-economic gains in the future for the individual, the household, and at the aggregate level for the economy. Yet, a large percentage of primary school age children from rural Ethiopia have not yet completed primary schooling and little is known about the determinants of schooling outcomes. This paper outlines the socioeconomic determinants of schooling enrollment and relative grade attainments among primary school age children from rural Ethiopia. We find using data from the three waves (1994, 1999 and 2004) of the Ethiopian Rural Household survey (ERHS), that it is household income, among other factors that has an important role in improving schooling enrollments and grade progression. We also know that schooling outcomes realized today are not independent of schooling decisions made in the last period. Hence, we also estimate a dynamic conditional schooling demand function, where schooling outcome today is a function of lagged outcome and other socioeconomic characteristics. To estimate the relationship between current and lagged schooling outcomes, we construct a panel data on children between the age of 7 and 14 years in 1994 and follow them through the 1999 and 2004 waves of the ERHS. We find that there exists a strong positive association between past schooling outcomes and current schooling outcomes. We find that a child who was enrolled in the last period is 32 percentage points more likely to be enrolled today compared to child who were not enrolled in the last period. There also exists a positive association between relative grades today and relative grades accumulated in the last period, suggesting that individual's are able to compensate for at least some of the loss in grades occurred during the initial years. The dependence on past schooling suggests that, even a one time policy initiative targeted towards improving household incomes in rural Ethiopia will not only improve children's schooling outcomes today but also translate into improvements in the child's complete trajectory of future schooling outcomes.

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1 Introduction

Investments in schooling are positively associated with higher economic and non-economic gains in the future for the individual, the household and at the aggregate level for the economy [see Psacharopoulos and Patrinos (2004) for a recent review]. In the past, economists have identified labor market returns, parental education, household income, and school characteristics as some key determinants of schooling outcomes.[1] For instance: Behrman and Knowles (1999) find that a child from Vietnam who belonged to a household whose average income was one standard deviation above the mean, on an average completed 2.2 additional years of schooling and scored 7% higher on examinations. Glewwe and Jacoby (1994) find that in Ghana, adding a library increased math test scores by 1.2 standard deviations.

The existing literature mostly uses cross-sectional data to characterize the socioeconomic determinants of schooling outcomes which suggest that policy initiatives aimed at directly affecting the demand (household income) and or supply side factors (availability of schools) of schooling are likely to improve enrollments and completed grades of schooling. However, schooling outcomes today are not independent of schooling related decision made in the last period. Schooling decision in the current period is a choice between continuing in school and dropping out for those already enrolled; this decision changes to a choice between enrolling or not for children who have never been to school. Hence, schooling decisions today are interconnected with past period's schooling decisions, all of which affect an individual's final schooling attainment as observed in the current period. For instance, Behrman, Sengupta, and Todd (2005) account for past schooling decisions in explaining current period attainments using experimental data from Mexico. They assess the impact of PROGRESA (a school subsidy program in Mexico) on schooling outcomes, treating the

[1]See Foster and Rosenzweig (1996) and Deolalikar (1993) for the role played by differences in labor market returns in determining schooling outcomes. Lillard and Willis (1992), Parish and Willis (1993), bring out the impact of parental education in determining schooling outcomes. Behrman and Knowles (1999) provide a review on the role played by household income in determining educational outcomes. Glewwe and Jacoby (1995) and Glewwe (2002) have focused on supply side determinants of schooling outcomes. Handa and Peterman (2007) and Alderman et. al (2006) examine the impact of childhood malnutrition on future schooling outcomes.

initial distribution of schooling states (not enrolled, enrolled in grade 1, enrolled in grade 2) at each age as given, to estimate a probability transition matrix which specifies the vector of schooling states for the next age. This methodology allows them to capture the association between an individual's enrollment status in the past period and it's effect on current period enrollments. A transition matrix is estimated for children in both the treatment group and the control group and the difference in these matrices at each age determines the program impacts on schooling enrollments and progression.

Behrman, Sengupta and Todd (2005) account for past schooling outcomes in determining current attainments, however, do not account for the impact of socioeconomic factors and other unobservables characteristics that have a role to play in determining the child's complete trajectory of current and future schooling outcomes. For instance, the child's innate genetic ability to perform well in school affects both the child's current state in the schooling transition matrix and his/her state in the last period. Socioeconomic factors like household income and demographic characteristics are also likely to determine the child's current schooling outcome. It is difficult to add these characteristics additionally in a probability transition matrix framework as it poses additional estimation problems[see Behrman, Sengupta and Todd (2005) for details].

The main objective of this paper is to use an empirical framework that allows us to capture the impact of past schooling outcomes in explaining current outcomes together controlling for household socioeconomic factors and demographic characteristics. We specify a dynamic conditional schooling demand function, where the coefficient on the lagged schooling outcome variable captures the extent to which an individual's past schooling inputs can affect his/her final schooling attainments.[2] In this paper, We find evidence which suggests the existence of a strong positive association between current and lagged schooling outcomes. This implies that in the absence of such a dynamic specification, the impact of past schooling resources (including short-run policy

[2]Such a dynamic specification capturing past schooling inputs have been estimated previously and are commonly referred as value-added specifications. The value-added specifications have mostly restricted their analysis to schooling outcomes capturing cognitive achievements as measured by test scores (see Todd and Wolpin, 2007).

intervention) that affect lagged outcomes may be underestimated.

In this paper, we begin with characterizing the socioeconomic determinants of current schooling outcomes using cross-sectional data from rural Ethiopia. We find household income and parental schooling to be the key factors affecting schooling outcomes. Identifying such factors can be helpful for guiding reforms and policy initiatives for the future.

To address the above objectives, we draw empirical evidence from rural Ethiopia. Why Ethiopia? First, developing countries from Asia and Africa pose a huge challenge to policy makers who aim to achieve universal primary school education by 2015. Ethiopia is no exception, where even today less than 60% of children in each age cohort enter grade 1 and only 60% of these enter grade 4 (Schaffner, 2004). Hence, factors that explain for schooling outcomes can be used to guide public investments in the future. Second, despite the very low levels of grade completion, average primary school enrollments almost doubled during 1994-2004. Much of this increase in schooling outcomes during 1994-1999 was accompanied by substantial increments in average household per capita consumption expenditures. However, the period of 1999-2004 saw similar improvements in schooling outcomes with little improvement in household incomes. A dynamic specification as previously described can be useful in this context as it brings out the role played by past schooling resources including household income in explaining future schooling outcomes. Third, our focus is primarily on rural areas as achieving improvements in educational outcomes in rural areas is the more difficult objective to achieve.[3]

To address the objectives of this paper, we estimate both a static[4] and dynamic conditional schooling demand function. The static function is estimated separately for primary school age children using data from each of the 1994, 1999 and 2004 waves of the Ethiopian Rural Household Survey (ERHS). The dynamic function is estimated using observations on children between 7 and

[3]See Orazem and King (2008) and Schaffner (2004) for details on rural-urban differences in schooling attainments among school age children from Ethiopia.

[4]Static framework uses accounts for households' decision making in period as independent of other period's decisions and in a dynamic framework the household decision in any period is related with all past and future periods' decisions.

14 in 1994 that are followed through the 1999 and 2004 waves of the ERHS. This paper uses enrollment status and relative grade attainment as main outcome variables of interest.

The paper contributes to the existing literature in two ways - First, the paper uses a dynamic conditional schooling demand function to establish the link between past schooling resources and current schooling attainments controlling for other observable socioeconomic factors and unobservables such as individuals' genetic endowments. Second, the paper identifies the socioeconomic factors that have contributed towards the improvements in schooling outcomes in Ethiopia over the last decade.

The static specification also explicitly addresses some of the concerns that have not been addressed by existing cross-section studies which use the following approaches that are somewhat limited in scope. For example, most papers in the literature use data on individuals who have already completed their schooling spell and use socioeconomic characteristics from the individual's current period to explain his/her completed schooling attainments (Parish and Willis, 1993). This kind of an empirical specification is potentially misspecified as the right hand side demand and supply side characteristics may not map to the year in which the decision regarding schooling outcomes is actually made.[5] We address this concern by using socioeconomic characteristics that appropriately map to the year in which the child's schooling related decisions are made.

Also, many papers from the literature use data on children with only completed schooling spell's restricting their sample to older children (usually to at least 15 years and above on an average). This is likely to create out migration related selection concerns. For example: boys older than 15 years from poor rural areas are likely to migrate in search of job opportunities and similarly girls older than 15 are likely to migrate due to early marriage. Hence the analysis sample is likely to be a pool of non-randomly selected individuals who continue to live in the same household.[6] In

[5]Authors often use the same household level and community level characteristics to explain for the 5 grades of schooling that a 12 year old and an 18 year old have attained without taking into account that the 18 year old may have completed 5 grades of schooling 6 years ago and it is the demand and supply side factors from that year which affects his/her completed grades today.

[6]A few papers in the literature have tried to address this issue by re-defining the outcome variable to a relative

order to address out-migration related selection concerns, we restrict our sample to include only primary school age (7-14 years) children. However, household level attrition could potentially create a non-random sample even among primary school age children. We later show that the low rates of household level attrition in the ERHS make this data set especially attractive.

Finally, schooling outcomes today is not just a function of current resources and it is the history of demand and supply side factors that determines an individual's complete trajectory of schooling outcomes. Hence, we must use a dynamic specification to capture such an association.

The static regressions identify household income and parental schooling as the key determinants of enrollments and relative grades accumulated.[7] Our preferred estimates of household income suggest that in 1994 income had almost no role in explaining schooling enrollments and only very little role in explaining relative grades. However, during 1999-2004 income effects gained sizable importance. In 2004 a 100% increase in real per capita consumption expenditure increased the probability of a 7-14 year old being enrolled by 0.17. A similar impact is seen on relative grades, where a 100% increase in real per capita consumption expenditure increases relative grades accumulation by 0.08. The increasingly strong association between household income and schooling in Ethiopia suggests that more income in the hands of the rural households will improve schooling among the next generation.

The preferred (IV) estimate on household income reported here is three times larger than the OLS parameter estimate, suggesting that not accounting for the endogeneity in the income variable would result in a large downward bias in the OLS estimate of household income. The IV coefficient estimates of income reported in this paper are also robust to the problem of weak instruments.

The dynamic estimation results indicate that current schooling is strongly associated with past schooling resources, where the impact of past schooling resources is captured by the coefficient

measure Most papers assume this problem away and only a very few even mention it [see Tansel (1998), Holmes (1999) for discussion on these issues].

[7]Relative grade is calculated as actual grades divided by potential grades. Where potential grade is calculated as the total number of grades accumulated if the individual completed one grade of schooling by age 7 and accumulated one additional grade in each subsequent year.

on the lagged dependent variable. We find that a children who were enrolled in the last period is 32 percentage points more likely to be enrolled today as compared to children who were not enrolled in the last period. There also exists a positive association between relative grades attained today and relative grades accumulated in the last period, the magnitude of this coefficient is 0.25 suggesting that individual's are able to compensate for at least some of the loss in grades occurred during the initial years.

To summarize, this paper contributes to the existing literature in multiple ways - (1) we use socioeconomic characteristics that appropriately map to the year in which the child's schooling related decisions was made. (2) In order to address migration related selection concern, we restrict our sample to include only primary school age (7-14 years) children. However, household level attrition could potentially create a non-random sample even among primary school age children. We later show that the low rates of household level attrition in the ERHS make this data set especially attractive. (3) We treat our measure of income (measured by household per capita consumption expenditure) as endogenous and compare the extent and sources of bias in PCE comparing the OLS and IV estimates. This has previously been addressed in the literature by several papers, but not all due to the lack of available data on possible instruments for PCE [see Behrman and Knowles (1999) for review]. (4) The coefficient estimates on the household characteristics reported in this paper are robust to the inclusion of actual village level supply side factors or village fixed-effects. (5) We establish the relationship between current and lagged schooling outcomes. (6) We use variants of the GMM estimation strategy to deal with the endogeneity problem in the lagged schooling outcome variable. In particular, the estimation strategy adopted here addresses all sources of time-invariant heterogeneity - at the individual, household, and village level in determining the impact of lagged schooling outcomes on current schooling outcomes.

The rest of the chapter is organized as follows. Section 2 outlines the theoretical model. Section 3 outlines the empirical specification used for estimation purposes. Section 4 describes the data, provides descriptives, and details on variables constructed. Section 5 discusses the results obtained from the static and dynamic regressions. Concluding remarks follow in Section 6.

2 Model

Parental investment in schooling is guided by either altruistic preferences or economic returns. In the former case, schooling is treated as a pure consumption good from which the parent derives utility. Whereas, in the latter case, schooling does not separately enter the utility function; it is only the expected future returns from schooling (wage earnings) that affect parents utility [Mincer (1958), Schultz (1960), Bommier and Lambert (2000), Brown and Park (2002).[8]] This framework does not account for any non-pecuniary benefits associated with schooling.[9]

Following the health production function specified in Sahn and Fedorov (2005); Strauss and Thomas (1995, 1998, 2008); Foster (1995), we specify our schooling production function (1), where schooling in period t, S_t is a function of schooling inputs, community resources, individual demographics, child characteristics, household characteristics, and genetic endowments.

$$S_t = s(M_t, M_{t-1}, ..., M_0, I_t, I_{t-1}, ..., I_0, D_\sigma, \theta_{c\sigma}, \theta_c, \mu_{h\sigma}, \mu_h, G) \qquad \sigma = 0, 1, ...t \tag{1}$$

S_t is measured as enrollment status, completed grades or relative grade attainment. Schooling inputs, M_t include books, school uniform, and home inputs which affect the accumulation of schooling outcome. It is assumed that the household does not derive any direct utility from the consumption of M_t except via its impact on determining S_t. Environmental characteristics are

[8]Brown and Park (2002), define parents as being altruistic as long as parents care as much about their children as themselves.

[9]There is another household framework, where parent's investments in schooling outcomes are higher among the relatively less endowed children. Parents here aim to equalize the expected future returns from schooling among all children in a household [Behrman, Pollak and Taubman (1982)]. It is important to have a good measure of ability to identify the need for differential schooling investments among children. The ERHS does not collect data that measures the child's ability and hence we do not dwell any further in these models.

important in determining schooling outcomes as they affect the age at which the child first starts schooling and also continue to affect enrollment in every subsequent period. It characterizes the environment where the child lives capturing school resource availability in the community. D_σ include time-varying household demographic characteristics such as mother's age and age of the head of the household capturing household experience. G captures genetic endowments that pass from the parent to the child affecting the child's overall cognitive development and learning. $\theta_{c\sigma}$ and θ_c includes child specific time-varying and time-invariant observables such as child's sex and age which captures age and gender specific differences in the accumulation of schooling. $\theta_{c\sigma}$ and θ_c also include time-varying and time-invariant unobservables such as the child's own innate ability to perform well in school. μ_h and $\mu_{h\sigma}$ capture household specific time-invariant and time-varying rearing and caring practices as captured by parental schooling variables.

Households decide to allocate schooling inputs based on the following inter-temporal utility maximization problem defined over T time periods. Expected lifetime utility - U (2) is maximized, subject to a lifetime budget constraint (3), and a dynamic schooling production function (4)[10]

$$Max : U = E_t \sum_{t=0}^{T} \beta^t u_t[C_t, S_t, L_t; \theta_{pt}] \tag{2}$$

Subject to:

$$A_T = (\prod_{t=0}^{T}(1+r_t))A_0 + \sum_{t=0}^{T}(\prod_{\tau=t}^{T}(1+r_\tau))(w_t(T_t - L_t) + \pi_t - P_t^c C_t - P_t^m M_t) \tag{3}$$

$$S_t = f(S_{t-1}, M_t, I_t, D_t, \theta_{ct}, \theta_c, \mu_{ht}, \mu_h, G) \tag{4}$$

[10]We substitute for all past period's schooling inputs and environmental factors by the one-period lagged schooling outcome in equation (1). Redefining equation (1) the dynamic child schooling production function (4) can be obtained.

The utility function specified here assumes the existence of a unitary household, that is, each member of the household shares the total available resources in equal measure. We are aware that there exists little empirical validation for the existence of a unitary household model (Haddad et. al, 1995). However, limited data availability does not allow us to use a collective approach to model household behavior. In addition, given the data restrictions, a 'unitary' vs. 'collective' approach to model household behavior will not alter the empirical specification.

The sub-utility function (u_t) in each period depends upon consumption goods that include food and non-food commodities, C_t, leisure, L_t, and the child's educational status, S_t. E_t is the expectations operator conditional upon information at time t. β is the subjective discount factor which captures household preferences for higher utility today *vis-a-vis* the future. P_t^c is a vector of prices of food and non-food consumption goods. P_t^m is a vector of price of school inputs. w_t is the wage rate (price of leisure). T_t is parents total time endowment and A_0 is assets the households owns at the beginning of period 0. Profit income from farm and non-farm activities and all other sources of non-labor income is captured by π_t.

The solution to this optimization problem relies on the following assumptions - (a) the household's utility function is additively separable over time. (b) The sub-utility functions are concave and twice differentiable. (c) The one-period lagged schooling outcome is sufficient to capture the impact of all past schooling inputs, environmental factors and other characteristics starting from birth onwards up until the last observed period in the sample. (d) The household can potentially borrow and or lend against its future in each period [Deaton and Meullbauer (1980), Strauss and Thomas (2008)]. Under these assumptions, we can solve for the optimal conditional schooling input demand function, as M_t^*.[11]

$$M_t^* = m(S_{t-1}, P_t^c, P_t^m, w_t, I_t, \lambda, D_t, \theta_{ct}, \theta_c, \mu_{ht}, \mu_h, G, \theta_{pt}, E_t(Z_{t+j})) \tag{5}$$

[11] See the derivation of first-order conditions obtained for a similar dynamic model from chapter 3

for $j = 1, 2, ..., T - t$ and $Z = P_t^c, P_t^m, w_t, I_t, D_t, \theta_{ct}, \theta_c, \mu_{ht}, \mu_h, \theta_{pt}, G$

The dynamic conditional child schooling demand function (6) can be derived by replacing M_t^* for M_t in equation (4):

$$S_t^* = f(S_{t-1}, P_t^c, P_t^m, w_t, I_t, \lambda, D_t, \theta_{ct}, \theta_c, \mu_{ht}, \mu_h, G, \theta_{pt}, E_t(Z_{t+j})) \tag{6}$$

for $j = 1, 2, ..., T - t$ and $Z = P_t^c, P_t^m, w_t, I_t, D_t, \theta_{ct}, \theta_c, \mu_{ht}, \mu_h, \theta_{pt}, G$

The optimal dynamic conditional child schooling demand function derived in (6) is expressed as a function of the one period lagged schooling outcome, price of consumption (food and non-food) goods, price of schooling inputs, wage rate, community infrastructure, child characteristics, household characteristics and marginal utility of wealth in period 0 (λ). Expectations at time t about future periods - prices of consumption goods and schooling inputs, wage rates, community resources, household characteristics and child characteristics are all captured by the term Z. Theoretically the term Z enters the dynamic schooling demand function in an unrestricted manner. However, we assume that the term Z (in equation 6) enters the dynamic empirical specification only additively.

In a static model, the optimal school input (M_t^*) is determined by maximizing utility in current period subject to a period specific budget constraint and a static schooling production function.[12] The optimal school inputs are then substituted into the static schooling production function to obtain the optimal schooling demand function, as specified below:

$$S_t^* = f(P_t^c, P_t^m, w_t, I_t, D_t, \pi_t, \theta_{ct}, \theta_c, \mu_{ht}, \mu_h, G, \theta_{pt}) \tag{7}$$

[12] See Mani (2007) for a derivation of static conditional child health demand function. An identical framework is followed to write out the static conditional child schooling demand function specified here.

In a static framework, current schooling is a function of current period prices of consumption goods, price of leisure, price of school inputs, environment characteristics and household income.

The theoretical model specified in this section guides the choice of the right hand side variables used in the empirical specification.

3 Empirical specification

The empirical counterpart of the static [equation 7] and dynamic [equation 6] conditional schooling demand functions can be written as follows:

$$S_{it} = \beta_0 + \sum_{j=1}^{R} \beta_j^X X_{jit} + \sum_{j=1}^{S} \beta_j^Z Z_{ji} + \epsilon_v + \upsilon_{it}; \quad \upsilon_{it} = \epsilon_i + \epsilon_h + \epsilon_{it} \tag{8}$$

$$S_{it} = \beta_0 + \beta_1 S_{it-1} + \sum_{j=1}^{R} \beta_j^X X_{jit} + \sum_{j=1}^{S} \beta_j^Z Z_{ji} + \epsilon_i + \epsilon_h + \epsilon_v + \epsilon_{it} \tag{9}$$

S_{it} and S_{it-1} is the child's enrollment status or relative grades accumulated at time t and t-1 respectively, where subscript i refer to the child. Enrollment status is characterized as a dummy variable defined =1 if the child is enrolled in school at the time of the survey and 0 otherwise.[13] Relative grades is defined as actual grades divided by potential grades. Where potential grades is calculated as the total no. of grades accumulated had the individual completed one grade of schooling by age 7 and continued to accumulate one more additional grade of schooling in each subsequent year.

[13] In addition to enrollment in regular school, some of the children are also enrolled in religious schools. We are interested in the child's actual human capital accumulation which comes only through learning subjects like mathematics, science or social science and is directly related to the child's future earnings potential, none of which is taught in religious schools. Hence, for our purpose in this paper, we treat children enrolled in religious schools as not enrolled.

The time-invariant regressors, Zs include a male dummy, measure of parental schooling, and mother's age. The male dummy is coded as 1 for males and 0 for females and captures gender differences in schooling outcomes. We include measures for both mother's schooling and father's schooling. Our measure for mother's schooling is constructed as a dummy=1 if the mother has completed non-zero years of schooling and 0 otherwise. Our measure for father's schooling is constructed using the same rule. Parental schooling variables reflect parental rearing and caring practices that affect schooling outcomes. Our measures of parental schooling is characterized using dummy variables because the average grades accumulated among parents is about 1 grade in 1994 and increases to only 2 grades by 2004. There is only a marginal variation in completed grades of schooling among parents. Hence, we use a categorical variable to describe parental schooling attainments. Mother's age captures mother's experience in efficiently allocating schooling inputs among school age children. Identical time-invariant regressors are specified in the static and dynamic specifications (equation 8 and equation 9).

Xs include time-varying regressors at the individual level, household level, and village level. A point to note is that some of the time-varying regressors specified in the static specification are different from the one's specified in the dynamic specification. In the static specification, we control for time-varying child level regressors such as age of the child, where age is specified using dummy variables with a separate dummy variable for each year between 7-14 years. The age dummies capture the age differential impact of schooling. In the dynamic specification, we follow panel respondents who were between 7-14 in 1994. Hence we use a spline in lag age in years with the cut-off at 14.99 years.[14] The age-gender specific determinants of schooling are captured using interaction terms between the age variables and the gender dummies.

The time-varying household level regressors in the static specification include household composition variables such as number of adult (>18 years) males, number of adult (>18 years) females capturing household demographic composition. Age of the head of the household is an additional

[14]This cut-off was determined using a lowess plot on age in months and schooling outcome

regressor used to capture household experience and life-cycle position. In the dynamic specification we use one-period lagged measure of the same household composition variables, as current period household composition variables would be endogenously determined in the dynamic model. In order to reduce the number of endogenous regressors in the right hand side, we use lagged measure of household composition variables.

Time-varying village specific characteristics can only be included in the static model since the information on village characteristics are only available for 2004. To control for schooling infrastructure and environment in the community, we include distance to primary school in km, a dummy for availability of electricity, and a dummy for availability of piped water.

The static specification in equation 8 must also include a measure for household wage and non-wage income. Households tend to smooth consumption and hence, consumption is likely to be a better measure of income than current information on wage and non-wage income. Hence, we use logarithm of real per capita consumption expenditure (PCE) as a measure of household income [Behrman and Knowles (1999)]. Total household consumption expenditure is constructed as the sum of value of food items (questionnaire included details on 33 specific food items) consumed including purchased and non-purchased consumption goods (consumption out of own stock), and value of non-investment type non-food items purchased. Non-food items include consumables such as matches, batteries, kerosene but exclude expenditure on durables such as housing [Dercon, Hoddinott and Woldehanna (2005)]. Consumption levels are valued using prices obtained from market survey fielded at the same time as the household surveys. The total household consumption expenditure is further divided by household size to capture the per person resource availability in the household. The nominal per capita consumption values are then converted to real per capita consumption expenditure using the food price index. Logarithm of the real per capita household consumption expenditure is further taken to account for non-linearity in the per capita consumption variable. In the static specification, we treat our measure of household income as captured by log(real pce) as being endogenously determined. OLS estimate on PCE is likely to be biased and inconsistent due to - (1) the potential correlation between household specific time-invariant unobservables (parent's preferences and time discount rate) and PCE, resulting in omitted variables bias. (2) The presence of random measurement error in data is likely to additionally bias the estimated coefficient on PCE towards zero. We use two-stage least squares to address the endogeneity problem in per capita consumption variable.

The dynamic model includes λ, marginal utility of wealth in period 0, which is a function of both retrospective information (period 0 to period t-1) and prospective information (period t+1 to period T) on prices, incomes, child characteristics, and household characteristics, that enter the demand function through the lifetime budget constraint. Empirically, treating marginal utility of wealth as a constant would be a strong assumption since it relies on the existence of complete markets, an assumption that is not likely to hold in a developing country set up where most households may be credit constrained. In addition the household cannot perfectly control for his future wealth and changes in the environment where he lives making λ stochastic. In order to allow for λ to reflect some of these time-varying changes and dynamics, we use household's access to resources in the long-run as measured by lag of log of household's real per capita consumption expenditure [lag log (PCE)] as an additional control variable in the right hand side. The sequence of expected future household characteristics, prices, incomes, and other factors affecting current schooling through $E_t(Z_{t+j})$ empirically enters either through the time-invariant household specific unobservables (ϵ_h) or the time-varying i.i.d term (ϵ_{it}) given in equation (9). We do need to assume that $E_t(Z_{t+j})$ enters the dynamic reduced form conditional schooling demand function only additively.

In addition to the observable characteristics included in the regressions, there are four sources of unobservables in this model - ϵ_i, ϵ_h, ϵ_v, and ϵ_{it}. ϵ_i captures the time-invariant individual specific unobservables such as the child's innate ability. ϵ_h captures time-invariant household specific unobservables which reflect parental preferences toward schooling and parents time discount rate. ϵ_v captures all time-invariant village specific unobservables like political connections. ϵ_{it} includes time-varying unobservables which include time-varying price shocks, income shocks and expectations at date t about future period's income, prices, and other characteristics which are all unknown to the econometricians at date t. We assume ϵ_{it} to be random.

The condition of zero correlation between the error term and lagged dependent variable may never be satisfied due to the presence time-invariant and time-varying unobservables. The one-period lagged schooling outcome, S_{it-1}, is likely to be correlated with the time-invariant individual-specific unobservables like the child's ability to perform well in school, which creates an upward bias in the estimated coefficient on the one-period lagged schooling status - β_1. The time-invariant household-specific unobservables like parental preferences towards child schooling and time discount rate, is also likely to create an upward bias in the estimated coefficient on - β_1. However, we know that parents could also invest more in children who had lower genetic ability and thus biasing the coefficient on β_1 biased downwards. The time-invariant community-specific unobservables like political connections of a community are also likely to be positively correlated with the lagged dependent variable creating an upward bias in the estimated coefficient on β_1. At the same time, pro-poor policies at the community level can bias the estimated coefficient of β_1 downwards. In addition, β_1 is likely to be biased downwards, towards zero due to the presence of classical measurement error in schooling outcomes.

Given the different sources of the potential biases in S_{it-1}, it is difficult to assign the net direction of bias on β_1. However, one can broadly classify the main sources of the endogeneity in the estimated coefficient on the lagged dependent variable as omitted variables and or measurement error in data. The results section discusses variants of the GMM estimation strategies that can be used to address the potential sources of endogeneity in the lagged dependent variable.

4 Data

4.1 Ethiopian Rural Household Survey

Ethiopia is divided into 11 regions and each region is sub-divided into zones and zones into woredas.[15] Each woreda is further sub-divided into peasant associations. The smallest administrative unit in Ethiopia is called a 'peasant association' which is sometimes equivalent to one village or a cluster of villages. The data used in this paper comes from the 1994, 1999, and 2004 waves of the Ethiopian Rural Household Survey (ERHS). The ERHS is a large-scale socioeconomic survey which has collected individual level, household level, and occasionally community level data from selected rural peasant associations in Ethiopia during 1989-2004.[16]

The first wave of the ERHS was fielded in 1989 during which households from 7 farming villages in central and southern Ethiopia were surveyed. In 1989, only a narrow set of questions were included in the survey as at the time there was no intention of creating a longitudinal data set. In 1994, 6 of the 7 original villages from 1989 (one of the villages could not be re-visited due to civil unrest) and 9 new villages that most suffered from the 1984-85 and 1987-89 droughts were additionally selected for survey purposes. A total of 15 rural villages were surveyed in 1994 with the aim of constructing a longitudinal data set. The 15 rural villages included in the 1994 survey are representative of the diverse farming systems practiced across the thousands of rural villages in Ethiopia. In 1994, two waves of the ERHS were administrated, the first wave during January-March and the second during August-October. The ERHS subsequently followed households residing in these 15 rural villages during 1995, 1997, 1999 and 2004 [see Dercon and Hoddinott (2004) and Dercon et. al (2006) for more details on survey design].

The ERHS provides extensive information on household composition, income, consumption expenditure, farm and non-farm assets, ownership and value of land and livestock units, anthropo-

[15] A woreda in Ethiopia is roughly equivalent to a county in the U.S.

[16] Peasant associations were first set up in 1974, as an aftermath of the revolution. We use the term "villages" and "peasant association" interchangeably throughout this paper

metrics, harvest use and schooling outcomes. In 1997 and 2004, the ERHS also collected detailed community level information on infrastructure availability, prices of consumption goods, and community level shock variables.

4.2 Sample composition

This paper uses cross-sectional and panel data methods to outline the socioeconomic determinants of schooling outcomes in a static and dynamic context. The repeated cross-sectional regressions use observations on children aged 7-14 years from each of the 1994, 1999, and 2004 waves of the ERHS. The panel data regressions use observations on children who were initially between 7-14 years in 1994 and could be followed through the 1999 and 2004 waves of the ERHS.

The choice of drawing empirical evidence from only the 1994, 1999 and 2004 waves deserves explanation. It is only from 1994 that we have a larger pool of villages that were first surveyed and details on enrollments and completed grades were first collected only in 1994. In addition, to keep the interpretations on our cross-sectional coefficient estimates straightforward, we maintain the years between two consecutive survey rounds unchanged restricting our final sample to exclude observations from the 1995 and 1997 waves of the ERHS.

Why restrict the sample to include only primary school age children? First, only a third of all school age children from rural Ethiopia have at least one completed grade of schooling and less than 10% of these children have completed primary schooling. Hence it is the socioeconomic environment that primary school age children face that determines their complete trajectory of current and future schooling attainments. Second, restricting the sample to primary school age children addresses out migration related selection concerns. There occurs huge out migration among high school age children due to early marriage. For instance: Ezra and Kiros (2001) find that 79% of the female Ethiopian migrants identified marriage as the primary reason for out migration with average age at marriage around 16 years. Also, Fafchamps and Quisumbing (2005) document average age of a bride during first marriage in rural Ethiopia to be 17 years.

4.3 Attrition

The long time line of the ERHS raises concerns about attrition. Sample attrition occurs at two levels - household and individual. If attrition were random and not related to the outcome variable of interest then there is no source of inconsistency in the parameters estimated. However, if sample attrition were related to the outcome variable of interest either through observables or unobservables then the coefficients estimated are likely to suffer from attrition bias [Fitzgerald et. al (1998)]. In this section we address, attrition related concerns at both the household and individual level.

The focus on primary school age children alone allows us to naturally deal with individual level selection issues such as migration that can potentially contaminate our cross-sectional parameter estimates. However, households could migrate in search of better schooling opportunities for their children. This can result in a non-random sample of school age children creating attrition bias even among the 7-14 year olds. The low levels of household attrition rates in the ERHS (as described below) address this selection concern too.

In 1994, the ERHS surveyed a total of 1477 households from 15 rural villages in Ethiopia. In 1999, 1371 of the original 1477 households were re-contacted for interview purposes. Even after 5 years, in 1999, 92.82% of the original households were re-contacted. In 2004, 1304 of the original 1477 households were re-interviewed. Between 1999 and 2004, re-contact rate is 95.1%. After almost a decade, the re-contact rates were as high as 88.2% with total household level attrition rate at 11.8% between 1994 and 2004. Household level attrition is minimal in these rural areas and is supported by other studies using the ERHS.[17] The Ethiopian economy is primarily agrarian

[17] Dercon and Hoddinott (2004) report that sample attrition rates are as low as 7% using data from the 1989 and 1994 waves of the ERHS. Dercon et. al (2006) report household level attrition between 1994 and 2004 to be around 12.4% which is very close to the numbers reported in this paper on household level attrition.

and 85% of the total working age population depends on agricultural income for survival. Land is owned by the government and households cannot obtain land if they decide to move to another location. This severely constraints the household's mobility and keeps household level attrition rates low.

Our dynamic regressions use observations on children who are initially between 7 and 14 in 1994 and can be followed through the 1999 and 2004 waves of the ERHS. By 2004, we lose more than 50% of the initial sample due to attrition at the individual level. A simple mean test on the difference in average completed grades of schooling between panel respondents and attriters is 0.18 (standard error = 0.06), which is statistically significant at 5%. Attriters have higher completed grades of schooling compared to children who can be followed over time. This indicates that attrition is related to schooling outcomes from the intial period. Further, to determine the extent to which initial period schooling outcomes affect attrition, we estimate a linear probability model on attrition, where the dependent variable attrition takes the value equal to 1 if the individual can be followed through all three waves of the ERHS and 0 otherwise. We control for age, mother's schooling, father's schooling, log of real household per capita consumption expenditure, no. of adult males, no. of adult females, village dummies, and allow for interactions between the age variables and the sex dummy. The results from column 1 table 16 (appendix) indicates that higher the accumulated grades of schooling in 1994, the lower the probability that the individual be followed through time. Column 2 captures a similar relationship, where attrition is negatively associated with grade progression. The third column captures the relationship between attrition and enrollment status in 1994. Enrollment status in the initial period is unrelated to sample attrition. All three columns in the table bring out the strong negative relationship between age and attrition. The older the individual, the less likely he/she is to be observed over time. The interaction terms between the age variable and the sex dummy suggest that male children are more likely to remain in the sample compared to females, this observation is consistent with marriage related migration among females. Sample attrition here is also related to household income, children who lived in households with higher per capita incomes were less likely to be followed over time. The strong association between attrition and relative grades indicates that attrition is not likely to be random. However, the more relevant concern here is to determine the extent to which the non-random nature of sample attrition is likely to contaminate our preferred coefficient estimates in the dynamic specification. Our preferred specification uses a first-difference IV estimation strategy, which removes the time-invariant unobservables from the empirical specification addressing some of the concerns relating to attrition bias. The time-varying unobservables in empirical specification can also potentially cause attrition bias. However, due to the lack of valid instruments available to us, we are not able to use any selection correction methods to address this source of attrition bias.

4.4 Descriptive statistics

During 1994-2004, rural Ethiopia witnessed huge improvements in its primary school enrollment rates. In 1994 only 12.7% of primary school children were enrolled in school. A decade later, in 2004 the percentage of primary school aged children enrolled increased by three times to 45.49%. Figures 1 and 2 depict schooling enrollment rates (in %) among boys and girls for all primary school children during this period.

Increment in enrollment rates for children between 7-9 years occurs during 1994-1999, there is practically no increase in enrollment among the younger age group during 1999-2004. The low levels of enrollment in 1994 combined with the initial increase in enrollment during 1994-1999 reflects mostly new enrollments, whereas improvements between 1999 and 2004 can be attributed to both new and continued enrollments. There is strong association between age and enrollment. In all three years, the enrollment rate is smallest at age 7 and peaks only after age 11. There is some reflection of declining enrollment rate among girls after age 12. For boys the enrollments and age have an almost linear relationship. During 1994-1999 the improvements in enrollment is higher among male children, however, between 1999 and 2004 the initial gender differences decline.

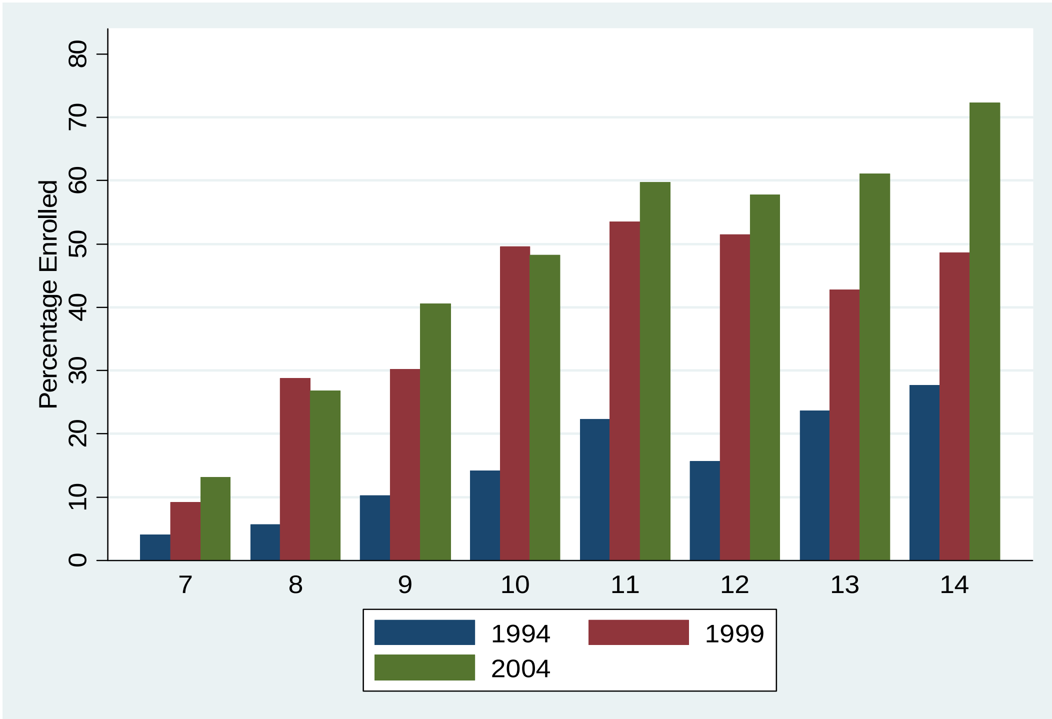

Figure 1: Male enrollment rate (%) by age in years

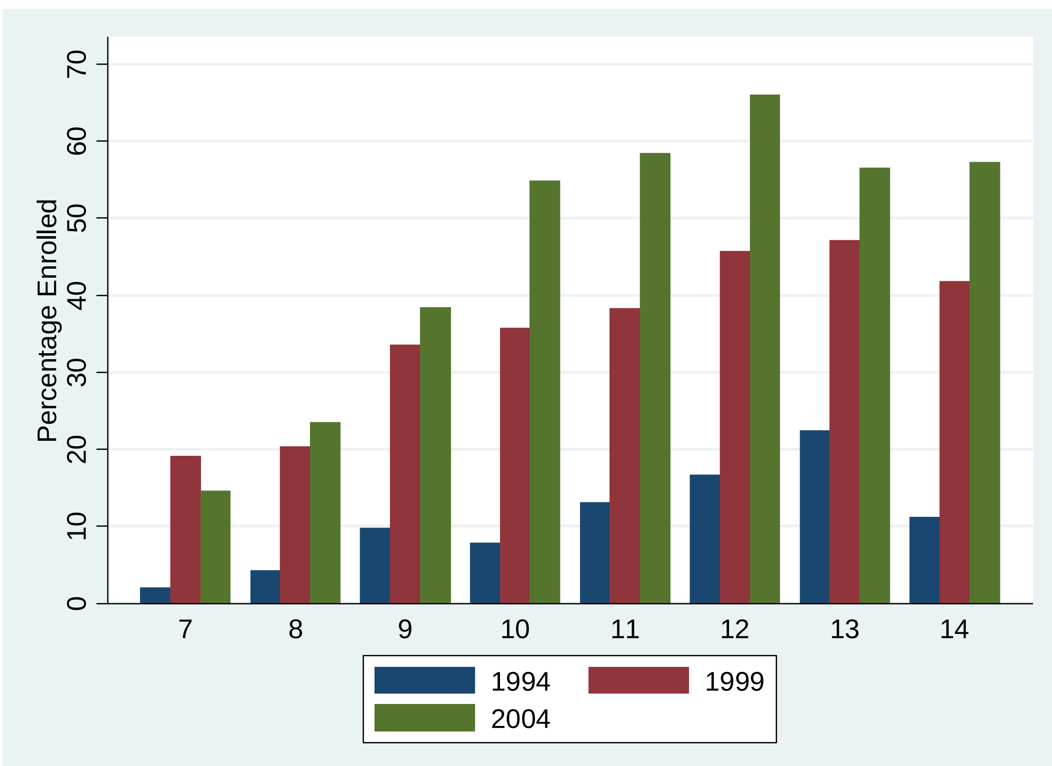

Figure 2: Female enrollment rate (%) by age in years

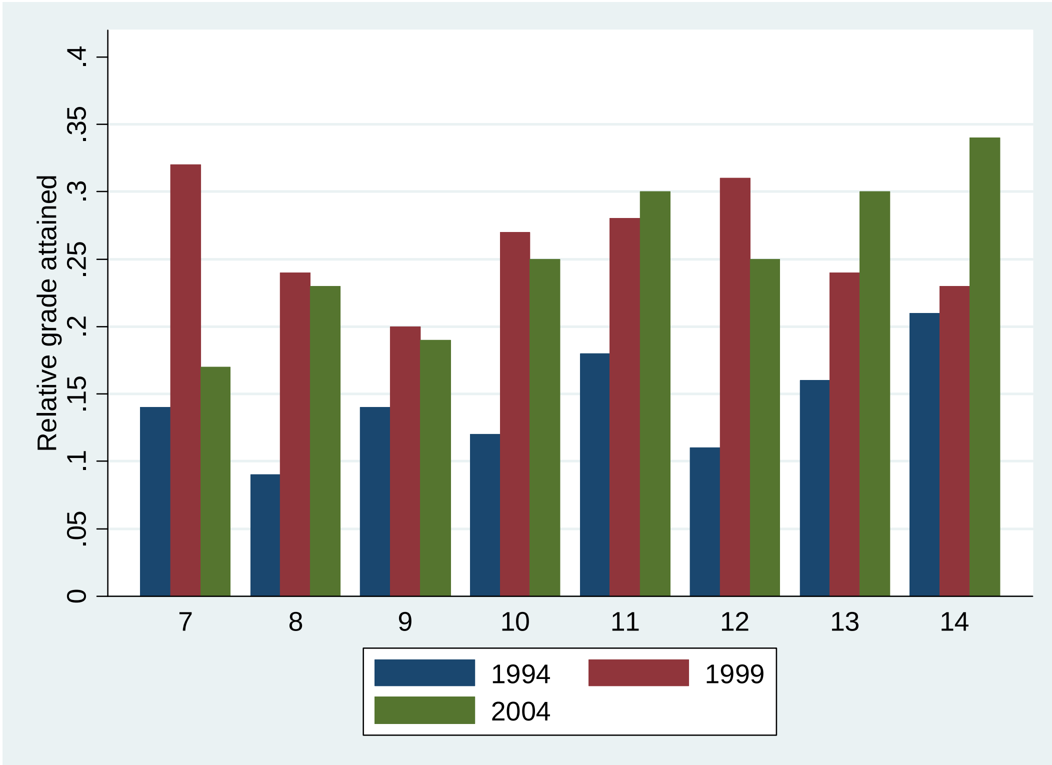

Figure 3: Male relative grade attainment

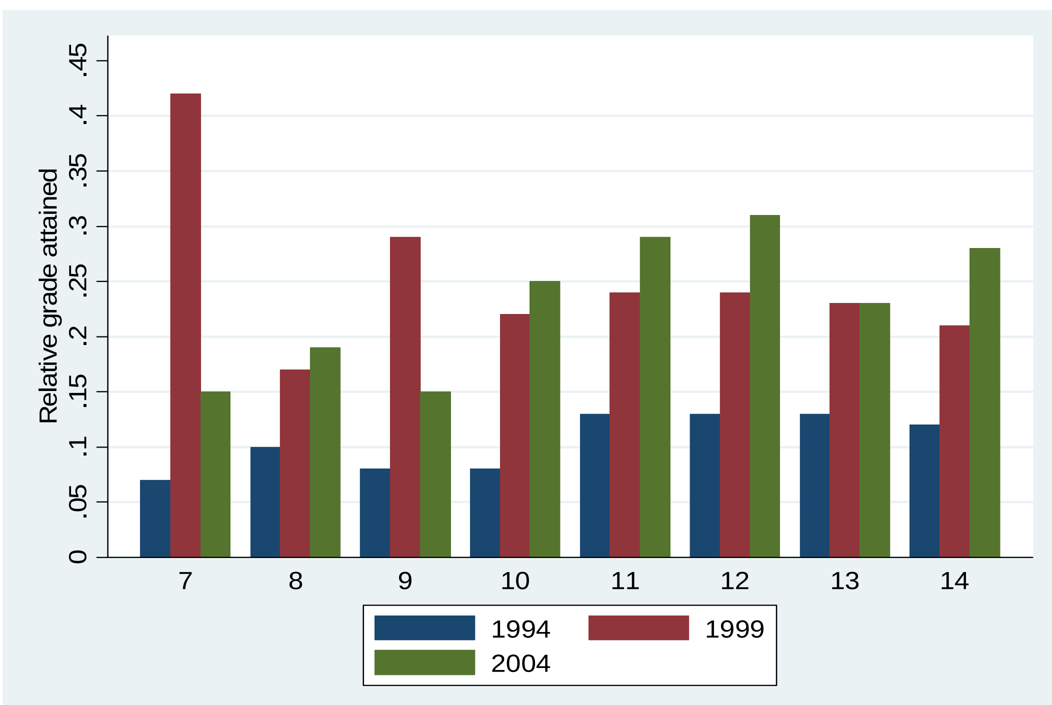

Figure 4: Female relative grade attainment

We find a steep increase in the percentage of children enrolled between 1994 and 1999 with a relatively smaller increase during 1999-2004. Figures 1 and 4 depict trends in relative grade attainment among all primary school age children. Early enrollments in 1999 are reflected in the steep increase in relative grades accumulated by age 7. The pattern of improvement in relative grades is similar to the patterns in enrollment rates. The male-female differences in relative grades also continue to decline.

Tables 1-3 provide sample averages and standard deviation on the dependent variables and the regressors used in the cross-sectional regression specifications.

Table 1: Sample averages using data for primary school children from 1994

Variable	Mean	Std. dev
Enroll, Enroll=1 if currently enrolled in school and 0 otherwise	0.13	0.34
Completed grades of schooling	0.60	1.43
Relative grade attained*	0.13	0.38
Household size	8.29	3.17
Log real per capita household consumption expenditure (PCE)	3.79	0.73
Mother's schooling	0.17	0.37
Father's schooling	0.39	0.49
Male dummy	0.50	0.50
Age in years	10.80	2.29
Land in hectares per adult member	0.57	0.56
Livestock units	3.16	4.12
No. of adult males	1.65	1.18
No. of adult females	1.71	1.05
Mother's age	38.64	9.60
Age of the head of the household	48.7	13.49
observations	2047	

- *Relative grade attained = actual grade completed/potential grade.

Table 2: Sample averages using data for primary school children from 1999

Variable	Mean	Std. dev
Enroll, Enrolled=1 if currently enrolled in school and 0 otherwise	0.38	0.48
Completed grades of schooling	1.13	1.70
Relative grade attained*	0.27	0.47
Household size	7.30	2.90
Log real per capita household consumption expenditure (PCE)	4.05	0.76
Mother's schooling	0.22	0.42

Father's schooling	0.44	0.49
Male dummy	0.50	0.50
Age in years	10.81	2.21
Land in hectares per adult member	0.49	0.50
Livestock units	3.32	3.10
No. of adult males	1.58	1.15
No. of adult females	1.70	1.03
Mother's age	39.76	9.91
Age of the head of the household	49.27	12.64
observations	1877	

- *Relative grade attained = actual grade completed/potential grade.

Table 3: Sample averages using data for primary school children from 2004

Variable	Mean	Std. dev
Enroll, Enrolled=1 if currently enrolled in school and 0 otherwise	0.45	0.49
Completed grades of schooling	1.17	1.67
Relative grade attained*	0.24	0.41
Household size	7.15	2.33
Log real per capita household consumption expenditure (PCE)	4.05	0.75
Mother's schooling	0.31	0.46
Father's schooling	0.51	0.50
Male dummy	0.51	0.50
Age in years	10.70	2.41
Land in hectares per adult member	0.64	0.56
Livestock units	3.42	3.61
No. of adult males	1.49	0.95
No. of adult females	1.5	0.83
Mother's age	40.05	8.65
Age of the head of the household	49.32	12.37
observations	1629	

- *Relative grade attained = actual grade completed/potential grade.

Table 4 captures the change in the outcome variable and all other regressors between 1994 and 2004.

Table 4: Mean changes in schooling outcomes and other variables between 1994 and 2004

Variable	Mean difference (2004-1994)	Std. error
Enroll, Enrolled=1 if currently enrolled in school and 0 otherwise	0.33	0.01
Completed grades of schooling	0.58	0.05
Relative grade attained*	0.11	0.01
Log real per capita household consumption expenditure (PCE)	0.25	0.02

Mother's schooling	0.15	0.01
Father's schooling	0.13	0.01
Land in hectares per adult member	0.06	0.02
Livestock units	0.21	0.12
No. of adult males	-0.17	0.03
No. of adult females	-0.19	0.03
Mother's age	1.41	0.38
Age of the head of the household	0.53	0.52

- *Relative grade attained = actual grade completed/potential grade.

5 Results

5.1 Static results

Before we outline the main regressions results, we clarify the choice of the econometric strategies used in this paper.

Enrollment status is defined as a limited dependent variable and hence, most papers in the literature use a probit specification to characterize the determinants of enrollment [Dostie and Jayaraman (2006), Pal (2004), Tansel (1998)]. Alternately one could specify a linear probability model (LPM), which can be estimated using an ordinary least square (OLS) estimation strategy. The OLS estimation strategy provides consistent and unbiased parameter estimates (Maddala, 1981). However, the presence of heteroskedastic errors results in incorrect inference, which can be corrected by applying robust standard errors [see pg 454, Wooldridge (2002)] to the OLS estimates. The only limitation associated with the application of OLS technique to the enrollment regression is that, sometimes the predicted probability of enrollment may not necessarily be restricted to the 0 to 1 interval and hence cannot be interpreted as probabilities.

One of the most commonly used outcome variable in this literature is completed grades of schooling. There are a number of econometric difficulties associated with using completed grades of schooling - First, many children in the sample have not yet been enrolled in school and hence a large number of observations are censored at zero. Second, observations on completed grades of schooling will also be right-censored for children currently enrolled in school. Both sources of censoring result in inconsistent parameter estimates. In addition, OLS estimation techniques cannot be applied to completed grades of schooling, since the outcome variable is not a continuous variable.

There are several approaches used in the literature to address the above issues. One way is to restrict the sample to include only observations on children with completed schooling spells. Such a sample can be estimated using ordered probit estimation techniques to obtain unbiased and consistent parameter estimates. However, this is likely to create out migration related selection concern. Also the right hand side variables used to characterize the determinants of schooling outcome may not be representative of the actual socioeconomic environment that affected the schooling decision.

An alternative is to create a relative measure of completed grades of schooling. For example Birdsall (1982) defines schooling as actual grades divided by the mean grades for the relevant age-sex category. Behrman (1984) used actual grades divided by potential grades as their relative measure of schooling. Some authors have also used grades completed per year as a relative measure for schooling attainments. The advantage of using relative measures of schooling is that a continuous outcome variable is created making OLS estimates consistent. The relative measure also accounts for delays in enrollments and grade attainments. Relative grades control for the difference in the time taken to complete 'x' grades of schooling. Individuals with the same completed grades of schooling are treated differently depending upon their age, except if the actual completed grade is zero. Hence, sample censoring at zero continues to remain a concern.

The third approach is to estimate a censored ordered probit specification [King and Lillard (1983, 1987)]. In this specification, a maximum likelihood framework is used where children who have completed their entire schooling spells (uncensored observations) and children who have not completed their entire schooling spell (censored observations) both enter the likelihood function separately. This estimation strategy addresses both sources of censoring bias producing consistent parameter estimates. However, this specification may not be attractive in this context as it relies on the strong assumption that children who belong to the uncensored category do not re-enter schools.

Another important econometric issue addressed here is clustering. Individuals residing in the same village share common unobserved village level characteristics and hence the error terms are correlated across individuals residing in the same village. Any such correlation in the error term violates the standard OLS assumptions producing incorrect inference.[18] The most common way to address this is to cluster the standard errors at the village level. In all the specifications reported in this paper, particularly our preferred estimates are obtained using an IV estimation strategy with village fixed-effects. The application of village fixed-effects in our cross-sectional data removes all possible sources of unobserved correlation between individuals residing in the same village [Wooldridge (2002, 2003)]. Hence we need not adjust the standard errors to clustering at the village level. We do correct the standard errors in our specifications to adjust for the presence of any arbitrary form of heteroskedasticity using the White (1980) formulation [see Wooldridge (2002)]. Hence the standard errors reported here are reliable and can be readily used to draw inferences.

The static conditional schooling demand function is estimated separately for the 1994, 1999, and 2004 waves of the ERHS. Each year's regression uses right hand side characteristics from that year alone. In table 5, a dummy for enrollment is regressed upon a set of child level and household level characteristics using observations on primary school children from the 1994 wave of the ERHS. Similar regression estimates are reported in tables 6 and 7, using data from the 1999 and 2004 waves of the ERHS respectively.

Table 5: Determinants of schooling enrollment among primary school age children from 1994

Covariates	(1) OLS Enroll	(2) OLS Enroll	(3) IV Enroll	(4) IV Enroll
Mother's schooling	0.0852**	0.0931**	0.0985**	0.0985**
	(0.03)	(0.03)	(0.04)	(0.04)

[18]The drawback is identical to the problems caused by heteroskedastic errors. We assume away any potential correlation between individuals residing in two different villages.

Father's schooling	0.1079***	0.1088***	0.1072***	0.1072***
	(0.02)	(0.02)	(0.02)	(0.02)
Log (real pce)	0.0319***		-0.0172	-0.0172
	(0.01)		(0.05)	(0.05)
Land		-0.0218		
		(0.01)		
Livestock units		0.0018		
		(0.002)		
Male dummy	0.0175	0.0136	0.0146	0.0146
	(0.02)	(0.02)	(0.02)	(0.02)
dummy=1 if the child	0.0299	0.0298	0.0300	0.0300
is 8 years	(0.02)	(0.02)	(0.02)	0.02
dummy=1 if the child	0.0833***	0.0792***	0.0799***	0.0799***
is 9 years	(0.02)	(0.02)	(0.02)	(0.02)
dummy=1 if the child	0.0589**	0.0587**	0.0591**	0.0591**
is 10 years	(0.02)	(0.02)	(0.02)	(0.02)
dummy=1 if the child	0.0936***	0.0900***	0.0909***	0.0909***
is 11 years	(0.03)	(0.03)	(0.03)	(0.03)
dummy=1 if the child	0.1374***	0.1380***	0.1398***	0.1398***
is 12 years	(0.03)	(0.03)	(0.03)	(0.03)
dummy=1 if the child	0.2042***	0.1995***	0.2014***	0.2014***
is 13 years	(0.03)	(0.03)	(0.03)	(0.03)
dummy=1 if the child	0.0922***	0.0920***	0.0910***	0.0910***
is 14 years	(0.03)	(0.03)	(0.03)	(0.03)
Male dummy*dummy=1 if	-0.0156	-0.0121	-0.0122	-0.0122
the child is 8 years	(0.03)	(0.03)	(0.03)	(0.03)
Male dummy*dummy=1 if	-0.0189	-0.0159	-0.0194	-0.0194
the child is 9 years	(0.04)	(0.04)	(0.04)	(0.04)
Male dummy*dummy=1 if	0.0411	0.0417	0.0408	0.0408
the child is 10 years	(0.04)	(0.04)	(0.04)	(0.04)
Male dummy*dummy=1 if	0.0822	0.0875***	0.0855*	0.0855*
the child is 11 years	(0.05)	(0.05)	(0.05)	(0.05)
Male dummy*dummy=1 if	-0.0002	0.0019	0.0007	0.0007
the child is 12 years	(0.04)	(0.04)	(0.04)	(0.04)
Male dummy*dummy=1 if	-0.0162	-0.0076	-0.0094	-0.0094
the child is 13 years	(0.05)	(0.05)	(0.05)	(0.05)
Male dummy*dummy=1 if	0.1545***	0.1597***	0.1604***	0.1604***
the child is 14 years	(0.05)	(0.05)	(0.05)	(0.05)
Number of adult males	0.0141	0.0130	0.0159*	0.0159*
	(0.008)	(0.009)	(0.008)	(0.008)
Number of adult females	-0.0175**	-0.0207**	-0.0201**	-0.0201**
	(0.008)	(0.008)	(0.008)	(0.008)
Mother's age	0.0010	0.0010	0.0011	0.0011
	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Age of the head of	-0.0001	-0.0001	-0.0002	-0.0002
the household	(0.0005)	(0.0006)	(0.0006)	(0.0006)
Observations	2047	2047	2047	2047
Village fixed-effects	Yes	Yes	Yes	Yes
F statistic on the			14.84	11.17
excluded instruments from			(0.00)	(0.00)

the first-stage regression		
Hansen J statistic	4.195 (0.12)	4.20 (0.24)

- Robust standard errors in parentheses
- *** significant at 1%; ** significant at 5%; * significant at 10%
- p-values are reported for the F statistic on the excluded instruments and
- the Hansen J statistic
- In column 3, PCE is instrumented with land and livestock units
- In column 4, PCE is instrumented with land, livestock units, and rainfall*land
- omitted age dummy - 7 years, omitted sex dummy - female
- Also included in the regressions are dummy variables capturing missing observations for each particular variable parental schooling, land, household composition variables where the missing observations were imputed by the sample mean

Table 6: Determinants of schooling enrollment among primary school age children from 1999

Covariates	(1) OLS Enroll	(2) OLS Enroll	(3) IV Enroll	(4) IV Enroll
Mother's schooling	0.0620	0.0613	0.0441	0.0413
	(0.03)	(0.03)	(0.04)	(0.04)
Father's schooling	0.0930***	0.0967***	0.0746**	0.0717**
	(0.02)	(0.02)	(0.03)	(0.03)
Log (real pce)	0.0417**		0.2400*	0.2712**
	(0.01)		(0.13)	(0.13)
Land		-0.0383		
		(0.02)		
Livestock units		0.0124***		
		(0.004)		
Male dummy	-0.0689	-0.0666	-0.0740	-0.0748
	(0.04)	(0.04)	(0.05)	(0.05)
dummy=1 if the child	0.0568	0.0594	0.0601	0.06061
is 8 years	(0.05)	(0.05)	(0.05)	(0.05)
dummy=1 if the child	0.1824***	0.1892***	0.1698***	0.1678***
is 9 years	(0.05)	(0.05)	(0.05)	(0.05)
dummy=1 if the child	0.2021***	0.2103***	0.1786***	0.1749***
is 10 years	(0.05)	(0.05)	(0.05)	(0.05)
dummy=1 if the child	0.2583***	0.2684***	0.2241***	0.2187***
is 11 years	(0.05)	(0.05)	(0.06)	(0.06)
dummy=1 if the child	0.3124***	0.3126***	0.3119***	0.3118***
is 12 years	(0.05)	(0.05)	(0.05)	(0.05)
dummy=1 if the child	0.3380***	0.3434***	0.3165***	0.3131***
is 13 years	(0.05)	(0.05)	(0.05)	(0.05)
dummy=1 if the child	0.3008***	0.3074***	0.2833***	0.2805***
is 14 years	(0.05)	(0.05)	(0.06)	(0.06)
Male dummy*dummy=1 if	0.1746**	0.1735**	0.1548**	0.1516*
the child is 8 years	(0.07)	(0.07)	(0.07)	(0.07)
Male dummy*dummy=1 if	0.0515	0.0479	0.0353	0.0327
the child is 9 years	(0.07)	(0.07)	(0.07)	(0.07)
Male dummy*dummy=1 if	0.2670***	0.2666***	0.2805***	0.2826***

the child is 10 years	(0.07)	(0.07)	(0.07)	(0.07)
Male dummy*dummy=1 if	0.2142***	0.1964**	0.2573***	0.2640***
the child is 11 years	(0.07)	(0.07)	(0.08)	(0.08)
Male dummy*dummy=1 if	0.1454*	0.1488**	0.1286	0.1260
the child is 12 years	(0.07)	(0.07)	(0.07)	(0.07)
Male dummy*dummy=1 if	0.0388	0.0353	0.0391	0.0391
the child is 13 years	(0.08)	(0.08)	(0.08)	(0.08)
Male dummy*dummy=1 if	0.1651**	0.1579*	0.1808**	0.1833**
the child is 14 years	(0.08)	(0.08)	(0.08)	(0.08)
Number of adult males	-0.0032	-0.0173	0.0115	0.0138
	(0.01)	(0.01)	(0.01)	(0.01)
Number of adult females	0.0087	-0.0019	0.0232	0.0255*
	(0.01)	(0.01)	(0.01)	(0.01)
Mother's age	-0.0035***	-0.0035***	-0.0039***	-0.0040***
	(0.001)	(0.001)	(0.001)	(0.001)
Age of the head of	0.0001	0.0002	-0.0003	-0.0003
the household	(0.001)	(0.001)	(0.001)	(0.001)
Observations	1877	1877	1877	1877
Village fixed-effects	Yes	Yes	Yes	Yes
F statistic on the			14.26	9.95
excluded instruments from			(0.00)	(0.00)
the first-stage regressions				
Hansen J statistic			8.45 (0.003)	8.97 (0.011)

- Robust standard errors in parentheses
- *** significant at 1%; ** significant at 5%; * significant at 10%
- p-values are reported for the F statistic on the excluded instruments and the Hansen J statistic
- In column 3, PCE is instrumented with land and livestock units
- In column 4, PCE is instrumented with land, livestock units, and rainfall*land
- omitted age dummy - 7 years, omitted sex dummy - female
- Also included in the regressions are dummy variables capturing missing observations
for each particular variable parental schooling, land, household composition variables where the missing observations were imputed by the sample mean

Table 7: Determinants of schooling enrollment among primary school age children from 2004

Covariates	(1) OLS Enroll	(2) OLS Enroll	(3) IV Enroll	(4) IV Enroll	(5) IV Enroll	(6) IV Enroll
Mother's schooling	0.0775**	0.0839**	0.0661*	0.0646*	0.1012***	0.1014***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Father's schooling	0.0775**	0.0800***	0.0663**	0.0649**	0.0843***	0.0843***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Log (real pce)	0.0608***		0.1645*	0.1780**	0.1650***	0.1641***
	(0.01)		(0.08)	(0.08)	(0.05)	(0.05)
Land		0.0477*				
		(0.02)				
Livestock units		0.0055				
		(0.004)				
Male dummy	0.0099	0.0030	0.0222	0.0238	0.0197	0.0196
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
dummy=1 if the child	0.1207**	0.1225**	0.1183**	0.1180**	0.1171**	0.1172**
is 8 years	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
dummy=1 if the child	0.2474***	0.2471***	0.2548***	0.2558***	0.2612***	0.2611***
is 9 years	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
dummy=1 if the child	0.4152***	0.4107***	0.4196***	0.4201***	0.4223***	0.4223***
is 10 years	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
dummy=1 if the child	0.4318***	0.4274***	0.4428***	0.4443***	0.4488***	0.4488***
is 11 years	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
dummy=1 if the child	0.5288***	0.5213***	0.5424***	0.5442***	0.5541***	0.5540***
is 12 years	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
dummy=1 if the child	0.4588***	0.4510***	0.4643***	0.4650***	0.4570***	0.4569***
is 13 years	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
dummy=1 if the child	0.4403***	0.4389***	0.4461***	0.4469***	0.4553***	0.4553***
is 14 years	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Male dummy*dummy=1 if	0.0351	0.0299	0.0520	0.0542	0.0520	0.0519
the child is 8 years	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Male dummy*dummy=1 if	0.0322	0.0310	0.0174	0.0155	0.0097	0.0099
the child is 9 years	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Male dummy*dummy=1 if	-0.0648	-0.0574	-0.0757	-0.0771	-0.0766	-0.0765
the child is 10 years	(0.07)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Male dummy*dummy=1 if	0.0172	0.0221	0.0011	-0.0009	0.0046	0.0047

the child is 11 years	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Male dummy*dummy=1 if	-0.0659	-0.0562	-0.0803	-0.0821	-0.0868	-0.0866
the child is 12 years	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Male dummy*dummy=1 if	0.0494	0.0638	0.0296	0.0270	0.0330	0.0332
the child is 13 years	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Male dummy *dummy=1 if	0.1527*	0.1580**	0.1462*	0.1453*	0.1338*	0.1339**
the child is 14 years	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Number of adult males	0.0010	0.0003	0.0005	0.0005	0.0004	0.0004
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Number of adult females	0.0298*	0.0289*	0.0406**	0.0420**	0.0600***	0.0600***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's age	-0.0022	-0.0019	-0.0022	-0.0022	-0.0018	-0.0018
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age of the head of	-0.0003	-0.0002	-0.0009	-0.0010	-0.0006	-0.0006
the household	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Distance to primary					-0.0205	-0.0204
school in km					(0.01)	(0.01)
Dummy=1 if the village					0.0403	0.0409
has electricity					(0.05)	(0.05)
Dummy=1 if the village					0.0026	0.0027
as piped water					(0.02)	(0.02)
Observations	1629	1629	1629	1629	1629	1629
Village fixed-effects	Yes	Yes	Yes	Yes	No	No
F statistic on the			44.08	32.03	108.52	72.28
excluded instruments from the			(0.00)	(0.00)	(0.00)	(0.00)
first-stage regressions						
Hansen J statistic			2.88 (0.08)	3.64 (0.16)	0.31 (0.57)	0.80 (0.66)

- Robust standard errors in parentheses
- *** significant at 1%; ** significant at 5%; * significant at 10%
- p-values are reported for the F statistic on the excluded instruments and the Hansen J statistic
- In columns (3) and (5) PCE is instrumented with land and livestock units
- In columns (4) and (6) PCE is instrumented with land, livestock units, and rainfall*land
- omitted age dummy - 7 years, omitted sex dummy - female
- Also included in the regressions are dummy variables capturing missing observations
for each particular variable (parental schooling, land, household composition variables) where the missing observation was imputed by the sample mean

In table 8, relative grade attainment is regressed upon a set of child level and household level characteristics using the 1994 data. In tables 9 and 10, similar regression estimates are reported using data from 1999 and 2004 waves of the ERHS respectively.

Table 8: Determinants of relative grades attained (RGA) among primary school age children from 1994

Covariates	(1) OLS RGA	(2) OLS RGA	(3) IV RGA	(4) IV RGA
Mother's schooling	0.0738	0.0820*	0.0517	0.05204
	(0.04)	(0.04)	(0.04)	(0.04)
Father's schooling	0.1135***	0.1162***	0.1147***	0.1147***
	(0.02)	(0.02)	(0.02)	(0.02)
Log (real pce)	0.0242**		0.1058**	0.1046**
	(0.01)		(0.04)	(0.04)
Land		0.0032		
		(0.01)		
Livestock units		0.0050**		
		(0.002)		
Male dummy	0.0565	0.0552	0.0614	0.0613
	(0.07)	(0.07)	(0.07)	(0.07)
dummy=1 if the child is 8 years	0.0356 (0.04)	0.0368 (0.04)	0.0354 (0.04)	0.0354 (0.04)
dummy=1 if the child is 9 years	0.0091 (0.04)	0.0090 (0.04)	0.0146 (0.04)	0.0145 (0.04)
dummy=1 if the child is 10 years	0.0046 (0.03)	0.0068 (0.03)	0.0042 (0.03)	0.0042 (0.03)
dummy=1 if the child is 11 years	0.0198 (0.04)	0.0211 (0.04)	0.0243 (0.04)	0.0242 (0.04)
dummy=1 if the child is 12 years	0.0426 (0.04)	0.0421 (0.04)	0.0386 (0.04)	0.0387 (0.04)
dummy=1 if the child is 13 years	0.0566 (0.04)	0.0524 (0.04)	0.0613 (0.04)	0.0612 (0.04)
dummy=1 if the child is 14 years	0.0404 (0.04)	0.0426 (0.04)	0.0423 (0.04)	0.0423 (0.04)
Male dummy*dummy=1 if the child is 8 years	-0.0786 (0.08)	-0.0769 (0.08)	-0.0844 (0.08)	-0.0843 (0.08)
Male dummy*dummy=1 if the child is 9 years	0.0075 (0.08)	0.0055 (0.08)	0.0082 (0.08)	0.0082 (0.08)
Male dummy*dummy=1 if the child is 10 years	-0.0197 (0.08)	-0.0229 (0.08)	-0.0191 (0.08)	-0.0191 (0.08)
Male dummy*dummy=1 if the child is 11 years	0.0229 (0.08)	0.0214 (0.08)	0.0175 (0.08)	0.0176 (0.08)
Male dummy*dummy=1 if the child is 12 years	-0.0431 (0.08)	-0.0425 (0.08)	-0.0445 (0.08)	-0.0445 (0.08)
Male dummy*dummy=1 if the child is 13 years	-0.0395 (0.08)	-0.0341 (0.08)	-0.0507 (0.08)	-0.0506 (0.08)
Male dummy*dummy=1 if	0.0451	0.0406	0.0353	0.0355

the child is 14 years	(0.08)	(0.08)	(0.08)	(0.08)
Number of adult males	0.0022	0.00007	-0.0007	-0.0007
	(0.007)	(0.008)	(0.007)	(0.007)
Number of adult females	-0.0173*	-0.0191**	-0.0131	-0.0132
	(0.009)	(0.009)	(0.009)	(0.009)
Mother's age	0.0010	0.0011	0.0007	0.0007
	(0.0009)	(0.0009)	(0.0009)	(0.0009)
Age of the head of the household	0.0001	0.0002	0.0003	0.0003
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Observations	2047	2047	2047	2047
Village fixed-effects	Yes	Yes	Yes	Yes
F statistic on the excluded instruments from the first-stage regressions			19.99	15.11
			(0.00)	(0.00)
Hansen J statistic			4.31 (0.11)	5.34 (0.14)

- Robust standard errors in parentheses
- *** significant at 1%; ** significant at 5%; * significant at 10%
- p-values are reported for the F statistic on the excluded instruments and the Hansen J statistic
- In column 3, PCE is instrumented with land and livestock units
- In column 4, PCE is instrumented with land, livestock units, and rainfall*land
- omitted age dummy - 7 years, omitted sex dummy - female
- Also included in the regressions are dummy variables capturing missing observations for each particular variable parental schooling, land, household composition variables where the missing observations were imputed by the sample mean, this holds for table 9 too

Table 9: Determinants of relative grade attained (RGA) among primary school age children from 1999

Covariates	(1) OLS RGA	(2) OLS RGA	(3) IV RGA	(4) IV RGA
Mother's schooling	0.0061	0.0062	-0.0112	-0.0108
	(0.03)	(0.03)	(0.04)	(0.04)
Father's schooling	0.1066***	0.1106***	0.0888***	0.0892***
	(0.03)	(0.03)	(0.03)	(0.03)
Log (real pce)	0.0452***		0.2364*	0.2322*
	(0.01)		(0.12)	(0.12)
Land		-0.0301*		
		(0.01)		
Livestock units		0.0115***		
		0.004		
Male dummy	-0.0916	-0.0888	-0.0964	-0.0963
	(0.12)	(0.12)	(0.12)	(0.12)
dummy=1 if the child is 8 years	-0.2256***	-0.2231***	-0.2225***	-0.2225***
	(0.08)	(0.08)	(0.08)	(0.08)
dummy=1 if the child is 9 years	-0.1166	-0.1100	-0.1287	-0.1284
	(0.08)	(0.08)	(0.08)	(0.08)
dummy=1 if the child is 10 years	-0.1999**	-0.1916**	-0.2225***	-0.2220***
	(0.08)	(0.08)	(0.08)	(0.08)
dummy=1 if the child	-0.1603**	-0.1498*	-0.1933**	-0.1925**

is 11 years	(0.08)	(0.08)	(0.08)	(0.08)
dummy=1 if the child	-0.1806**	-0.1802**	-0.1812**	-0.1812**
is 12 years	(0.08)	(0.08)	(0.08)	(0.08)
dummy=1 if the child	-0.1680**	-0.1623**	-0.1888**	-0.1883**
is 13 years	(0.07)	(0.07)	(0.07)	(0.07)
dummy=1 if the child	-0.1948**	-0.1881**	-0.2116***	-0.2113***
is 14 years	(0.08)	(0.08)	(0.08)	(0.08)
Male dummy*dummy=1 if	0.1751	0.1740	0.1560	0.1564
the child is 8 years	(0.13)	(0.13)	(0.13)	(0.13)
Male dummy*dummy=1 if	-0.0002	-0.0031	-0.0159	-0.0155
the child is 9 years	(0.13)	(0.13)	(0.13)	(0.13)
Male dummy*dummy=1 if	0.1907	0.1894	0.2037	0.2034
the child is 10 years	(0.12)	(0.12)	(0.12)	(0.12)
Male dummy*dummy=1 if	0.1332	0.1149	0.1747	0.1738
the child is 11 years	(0.13)	(0.13)	(0.13)	(0.13)
Male dummy*dummy=1 if	0.1876	0.1909	0.1715	0.1718
the child is 12 years	(0.12)	(0.12)	(0.12)	(0.12)
Male dummy*dummy=1 if	0.1048	0.1011	0.1051	0.1050
the child is 13 years	(0.12)	(0.12)	(0.12)	(0.12)
Male dummy*dummy=1 if	0.1317	0.1234	0.1468	0.1465
the child is 14 years	(0.12)	(0.12)	(0.12)	(0.12)
Number of adult males	0.0176	0.0045	0.0317**	0.0314**
	(0.01)	(0.01)	(0.01)	(0.01)
Number of adult females	-0.0251**	-0.0350***	-0.0111	-0.0114
	(0.01)	(0.01)	(0.01)	(0.01)
Mother's age	-0.0002	-0.0002	-0.0006	-0.0006
	(0.007)	(0.006)	(0.01)	(0.01)
Age of the head of	0.0019	0.0021	0.0016	0.0016
the household	(0.001)	(0.001)	(0.001)	(0.001)
Observations	1877	1877	1877	1877
Village fixed-effects	Yes	Yes	Yes	Yes
F statistic on the excluded instruments from the first-stage regressions			32.63 (0.00)	22.98 (0.00)
Hansen J statistic			10.97 (0.0009)	11.45 (0.003)

- Robust standard errors in parentheses
- *** significant at 1%; ** significant at 5%; * significant at 10%
- p-values are reported for the F statistic on the excluded instruments and the Hansen J statistic
- In column 3, PCE is instrumented with land and livestock units
- In column 4, PCE is instrumented with land, livestock units, and rainfall*land
- omitted age dummy - 7 years, omitted sex dummy - female

Table 10: Determinants of relative grade attained (RGA) among primary school age children from 2004

Covariates	(1) OLS RGA	(2) OLS RGA	(3) IV RGA	(4) IV RGA	(5) IV RGA	(6) IV RGA
Mother's schooling	0.0812**	0.0857**	0.0765*	0.0763*	0.0918**	0.0916**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Father's schooling	0.0467	0.0491	0.0421	0.0418	0.0456	0.0455
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Log (real pce)	0.0417**		0.0848	0.0869	0.1091**	0.1098**
	(0.01)		(0.07)	(0.07)	(0.04)	(0.04)
Land		0.0248				
		(0.02)				
Livestock units		0.0028				
		(0.003)				
Male dummy	0.0281	0.0315	0.0267	0.0366	0.0369	0.0453
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
dummy=1 if the child	0.0547	0.0559	0.0538	0.0537	0.0645	0.0645
is 8 years	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
dummy=1 if the child	0.0012	0.0003	0.0043	0.0045	0.0106	0.0107
is 9 years	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
dummy=1 if the child	0.1057	0.1029	0.1075*	0.1076*	0.1138*	0.1138*
is 10 years	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
dummy=1 if the child	0.1141	0.1107	0.1187	0.1189	0.1314*	0.1314*
is 11 years	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
dummy=1 if the child	0.1599**	0.1546**	0.1655**	0.1658**	0.1763***	0.1764***
is 12 years	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
dummy=1 if the child	0.0892	0.0846	0.0915	0.0916	0.0988	0.0989
is 13 years	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
dummy=1 if the child	0.1291**	0.1278**	0.1315**	0.1316**	0.1355**	0.1355**
is 14 years	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Male dummy*dummy=1 if	0.0139	0.0095	0.0209	0.0212	0.0096	0.0097
the child is 8 years	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)
Male dummy*dummy=1 if	0.0101	0.0110	0.0040	0.0037	-0.0049	-0.0051
the child is 9 years	(0.09)	(0.08)	(0.09)	(0.09)	(0.09)	(0.09)
Male dummy *dummy=1 if	-0.0244	-0.0194	-0.0289	-0.0291	-0.0330	-0.0331
the child is 10 years	(0.09)	(0.09)	(0.08)	(0.08)	(0.08)	(0.08)
Male dummy*dummy=1 if	-0.0163	-0.0122	-0.0230	-0.0233	-0.0292	-0.0293

the child is 11 years	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Male dummy*dummy=1 if	-0.0793	-0.0728	-0.0852	-0.0855	-0.0957	-0.0958
the child is 12 years	(0.08)	(0.09)	(0.08)	(0.08)	(0.08)	(0.08)
Male dummy*dummy=1 if	0.0515	0.0609	0.0432	0.0428	0.0311	0.0310
the child is 13 years	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Male dummy*dummy=1 if	0.0319	0.0353	0.0292	0.0291	0.0259	0.0258
the child is 14 years	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Number of adult males	-0.0061	-0.0064	-0.0063	-0.0064	-0.0087	-0.00876
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Number of adult females	0.0082	0.0067	0.0127	0.0129	0.0224*	0.0225*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's age	-0.0013	-0.0011	-0.0013	-0.0013	-0.0010	-0.0010
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Age of the head of	0.0011	0.0007	0.0007	0.0008	0.0008	
the household	(0.001)	(0.001)	(0.001)	(0.0009)	(0.0009)	(0.0009)
Distance to primary					-0.0212*	-0.0214*
school in km					(0.01)	(0.01)
Dummy=1 if the village					0.1279**	0.1273**
has electricity					(0.06)	(0.06)
Dummy=1 if the village					-0.0092	-0.0093
has piped water					(0.02)	(0.02)
Observations	1629	1629	1629	1629	1629	1629
Village fixed-effects	Yes	Yes	Yes	Yes	No	No
F statistic on the			61.97	45.16	116.78	78.03
excluded instruments from			(0.00)	(0.00)	(0.00)	(0.00)
the first-stage regressions						
Hansen J statistic			0.76 (0.38)	0.82 (0.66)	0.00 (0.99)	0.25 (0.88)

- Robust standard errors in parentheses
- *** significant at 1%; ** significant at 5%; * significant at 10%
- p-values are reported for the F statistic on the excluded instruments and the Hansen J statistic
- In columns (3) and (5) PCE is instrumented with land and livestock units
- In columns (4) and (6) PCE is instrumented with land, livestock units, and rainfall*land
- omitted age dummy - 7 years, omitted sex dummy - female
- Also included in the regressions are dummy variables capturing missing observations for each particular variable (parental schooling, land, household composition variables) where the missing observation was imputed by the sample mean

Relative grade attained = actual grade completed/potential grade.

In tables 7 and 10, we also control for community level characteristics which are only available for the 2004 wave of the ERHS.[19]

5.1.1 Child characteristics and gender differences

Our preferred estimates on enrollment are reported in column 4 of tables 5, 6, and 7. Age dummies, male dummy, and age interacted male dummies are included in the regression specifications to account for age, gender, and age-gender specific differences in schooling outcomes. The coefficient estimates on the age dummies from all three years indicate a strong positive association between age and enrollment. The older the child, the more likely he/she is enrolled compared to a 7 year old. In 1994, the coefficient estimates on the age dummies reflect delayed enrollments. The parameter estimates from the 1999 regressions depict an increase in the enrollment probabilities among all age groups suggesting both timely enrollments and continued enrollments. By 2004, more children are likely to be enrolled by age 8 and the probability of being enrolled tends to peak at a much earlier age, 12 years.

Age-gender specific differences in enrollments are captured by the parameter estimates on the age interacted male dummies. The interaction terms in column 4 of tables 5, 6, and 7 suggest that there are little gender differences in enrollments in 1994 with enrollment probabilities only marginally higher among boys. By 1999 the gender differences get magnified, boys at all ages are more likely to be enrolled than their female counterparts. These gender differences in schooling enrollments narrow down by 2004.

[19] A pooling test combining the sample from 1994, 1999, and 2004 waves results in a F statistic of 4.33, statistically significant at 1%, rejecting the null of pooling the sample from all three waves together. Hence the determinants of schooling outcomes are separately estimated for the 1994, 1999 and 2004 waves of the ERHS. We also test if the socioeconomic characteristics controlled in the regression specifications vary by gender. A joint test on the interaction between the gender dummy and the socioeconomic characteristics from 1994, yields an F statistic of 1.17 (0.31 as p-value), which is statistically insignificant. A similar test on the pooled sample from 1999 yields an F statistic of 1.39 (0.20 as p-value) and from 2004 yields an F statistic of 0.39 (0.91 as p-value). We can now conclude that the socioeconomic characteristics included in the empirical specifications do not vary by gender and hence, we estimate our static model pooling the sample on boys and girls together. Our pooled specifications allow for age-gender specific differences in schooling attainments.

Our preferred estimates from the relative grade attainment regressions are reported in column 4 of tables 8, 9 and 10 respectively. In 1994, only 13% of primary school age children were enrolled and an even smaller percentage of these children had non-zero completed grades of schooling. There is little variation in the outcome variable based on age and hence no significant relationship between age and relative grades. It is only in 1999 that the coefficient estimates on the age dummies suggest a strong negative relationship between age and relative grades, suggesting delayed enrollments which contribute towards slower accumulation of schooling grades. By 2004, relative grades systematically improved among all ages and yet none of the coefficient estimates on the age dummies are statistically significant. There is no evidence for age specific gender differences in relative grades attained.

5.1.2 Parental characteristics

Parental schooling variables capture the efficiency with schooling inputs are transformed into actual schooling outcomes. The coefficient estimates on the parental schooling variables reported in column 4 of tables 5-7 indicate a strong positive relationship between measure of parental schooling and children's enrollment status.[20] Every child whose mother has non-zero grades of schooling is 9 percentage points more likely to be enrolled in 1994, 4 percentage points more likely to be enrolled in 1999, and 6 percentage points more likely to be enrolled in 2004 compared to a child whose mother has zero accumulated grades. For every child whose father has non-zero accumulated grades is 10 percentage points more likely to be enrolled in 1994, 7 percentage points more likely to be enrolled in 1999 and 6 percentage points more likely to be enrolled in 2004 as compared to a child whose father has zero accumulated grades of schooling. In this paper, we treat parental schooling variables as exogenous. Parental schooling variables can potentially be endogenous due to the correlation between unmeasured innate ability across generations that is likely to

[20]Behrman and Wolfe (1984), Birdsall (1985), Alderman et. al (1997) and Parish and Willis (1993) all find that parental education has an important role in determining of child schooling outcomes.

affect both the child and parent's schooling attainments. However, data restrictions do not allow us to explicitly address this potential source of endogeneity.[21]

In addition to the independent effects of parental schooling, we examine add interaction terms between the age dummies and the parental schooling variables to capture the age differential impact of parental schooling on child schooling. The interaction terms are all jointly insignificant for all the three enrollment and relative grade attainment regressions.[22]

5.1.3 Household income

Schooling is considered as a normal good and hence increase in income is likely to have a positive impact on schooling attainments. In this paper we use logarithm of real per capita household consumption expenditure (PCE) as our measure of full income. As discussed earlier the presence of potential correlation between unobservables and the per capita consumption variable is likely to bias the coefficient estimate on PCE. In order to address the endogeneity problem in the income variable, we use an instrument variable estimation strategy to obtain our preferred unbiased and consistent estimates on household income.

Now, we need instruments that are correlated with the per capita consumption variable. The

[21] Lillard and Willis (1994) explicitly control for the correlation between parent's unobservables and child specific unobservables. Behrman and Rosenzweig (2002, 2005) show that the impact of parental schooling declines once we control for the unobservables that affect both parent and child schooling outcomes.

[22] The F statistic on the interaction between the age dummies and mothers schooling for the enrollment regressions in 1994, 1999 and 2004 are - 0.69 (0.67), 1.32 (0.23) and 1.27 (0.26) respectively with p-values in parenthesis. The F statistic on the interaction between the age dummies and fathers schooling in the enrollment regressions from 1994, 1999 and 2004 are - 1.41 (0.19), 0.91 (0.50) and 0.69 (0.64) respectively. The F statistic on the interaction between the age dummies and pce in the enrollment regressions from 1994, 1999 and 2004 are - 0.47 (0.85), 1.77 (0.08) and 1.07 (0.38) respectively. The joint F statistic on the interaction between the age dummies and pce, age dummies and mothers schooling, age dummies and fathers schooling in the enrollment regressions from 1994, 1999 and 2004 are - 0.86 (0.63), 1.30 (0.18) and 1.14 (0.29) respectively. The F statistic on the interaction between the age dummies and mothers schooling for the relative grade attainment regressions in 1994, 1999 and 2004 are - 0.64 (0.72), 1.17 (0.31) and 0.98 (0.44) respectively. The F statistic on the interaction between the age dummies and fathers schooling in the relative grade attainment regressions from 1994, 1999 and 2004 are - 0.94 (0.47), 2.28 (0.02) and 0.84 (0.55) respectively. The F statistic on the interaction between the age dummies and pce in the relative grade attainment regressions from 1994, 1999 and 2004 are - 1.25 (0.27), 1.57 (0.14) and 1.68 (0.11) respectively. The joint F statistic on the interaction between the age dummies and pce, age dummies and mothers schooling, age dummies and fathers schooling in the relative grade attainment regressions from 1994, 1999 and 2004 are - 1.02 (0.43), 1.37 (0.12) and 1.15 (0.29) respectively.

ERHS provides details on land holdings and livestock units, two forms of assets that households own. In a static framework, assets can be treated as being exogenously determined. In addition, the land distribution policy in rural Ethiopia is such that land is only owned by the government. The allocation of land to household's is determined outside household's schooling investment decision. Hence, we use land and livestock units as excluded instruments for PCE in the first-stage regressions.

Ethiopia is primarily a agrarian economy with more than 80% of the working age population employed in the agricultural sector. Rural households are largely dependent on rainfall for agricultural output. Hence, household food consumption and rainfall are likely to be correlated. Also rainfall can be treated as exogenous and this can be used as an additional instrument for PCE. We construct our measure of rainfall as the average mm of rainfall over the main harvest months in the village. The measure of rainfall constructed varies at the village level. To create household level variation in the impact of rainfall; land is interacted with rainfall and the interaction term is used as an additional instrument for PCE in our preferred IV estimates reported in column 4 of tables 5-10. There still remains a question of whether rainfall can be appropriately excluded from the second-stage regressions. Rainfall does not affect schooling directly as described in the theoretical model. Hence, the interaction term between land and rainfall can be used as excluded instruments for per capita consumption variable.

The preferred IV estimates of log (PCE) are reported in column 4 of tables 5-10, a summary of which are reported below in tables 11 and 12. Also included are the IV estimates on log (PCE) as reported in column 6 of tables 5-10 where the village fixed-effects are now replaced with the actual village level supply side determinants of schooling.

Table 11: Coefficient estimates on log (PCE) as reported in Tables 5-7

Coefficient estimates on log (PCE)	1994	1999	2004
Without IV, column 1	0.03***	0.04**	0.06***
With IV, column 4	-0.01	0.27**	0.17**
With IV, column 6, including actual			0.16***

supply characteristics in the right hand side	
- *** significant at 1%, ** significant at 5%, * significant at 10%	

Table 12: Coefficient estimates on log (PCE) as reported in Tables 8-10

Coefficient estimates on log (PCE)	1994	1999	2004
Without IV, column 1	0.02**	0.04***	0.04**
With IV, column 4	0.10**	0.23*	0.08
With IV, column 6, including actual supply characteristics in the right hand side			0.10**

- *** significant at 1%, ** significant at 5%, * significant at 10%

The OLS estimates reported in table 11 indicates that a 10% increase in household income increases the enrollment probability by 0.3 percentage points in 1994 and goes up to a maximum of 0.6 percentage points in 2004. The IV estimates reported in 2004, suggest that a 10% increase in income increases enrollment probability by 1.7 percentage points. OLS estimates reported in table 12 indicate that a 10% increase in income increases relative grades by 0.2 percentage points in 1994 and by 0.4 percentage points in 2004. Whereas the IV estimates of PCE show that a 10% increase in income is associated with 1 percentage point increase in relative grades in 1994 and a 0.8 percentage point increase in 2004. The importance of household income in determining schooling outcomes is re-established by our preferred estimates on PCE.

Improvements in household income are positively associated with improvements in schooling outcomes. The parameter estimate on PCE went from almost 0 in 1994 to 0.17 in 2004 highlighting the differential impact of income in explaining schooling outcomes among the two cohorts of primary school age children. We find that income effects are likely to have a persistent role in explaining for the improvements in schooling outcomes among children from rural Ethiopia. In the long run, household income will remain as one of the key determinants of schooling outcomes in rural Ethiopia.

The preferred IV estimate of PCE is almost three times higher than OLS estimate. The significantly large differences between the OLS and IV estimates obtained capture the magnitude of biasedness in the OLS parameter estimate. The increase in the coefficient estimate on PCE from OLS to IV indicates that the OLS estimate of PCE is likely to be biased downward due to measurement error and not biased upwards due to omitted variables. Some papers in the literature have not accounted for the endogeneity in the PCE variable and hence their estimated coefficient on PCE is likely to be both biased and inconsistent.

Our preferred IV estimates reported here are also additionally robust to an important econometric concern i.e. instrument validity. An instrument is defined to be valid only if it satisfies the following two conditions - (1) the excluded instruments must be strongly correlated with the endogenous regressor. (2) The instrument must be uncorrelated with the error term in the second stage regression. In the presence of weak correlation between the instruments and the endogenous regressors, the IV estimates reported are likely to suffer from a higher bias and inconsistency compared to the bias obtained on the OLS parameter estimate. It is important to verify that the IV estimates reported here satisfy the two above mentioned conditions.

Stock et. al (2002) and Staiger and Stock (1997) have discussed a test statistic that can be used to test the relevance of the instruments used in IV estimation. Stock et. al. (2002) and Stock and Yogo (2005) define an instrument to be weak based on two criteria - First, based on the relative two-stage least squares (TSLS) bias where the instrument is deemed to be strong if the Cragg-Donald F statistic is large enough such that the TSLS bias with respect to the OLS bias is say at most x% (5, 10, 15 depending the extent of bias the author wants to allow). The second criterion is based on size, i.e. the instruments are defined to be strong only if the Cragg-Donald F statistic is large enough, such that a 5% hypothesis test is rejected no more than say x% of the time, otherwise the instruments are deemed to be weak.

The main limitation with the application of the Cragg-Donald test statistic is that the test relies on the assumption of i.i.d errors and hence, is not robust to the presence of heteroskedasticity in the error term. It appears that there exists no clear consensus in the literature on the existence of an alternative test statistic that can be used to test for weak instruments with non-i.i.d errors. In which case it is suggested that the robust F statistic from the first-stage regression be used as a test for the presence of weak instruments.

Staiger and Stock (1997) suggest a simple rule of thumb to test for instrument relevance. They suggest that in the presence of a single endogenous regressor, instruments are deemed to be weak if the first-stage F statistic on the excluded instruments is less than 10. The F statistic on the excluded instruments is again well over 10 in all the IV specifications, rejecting the null of a weak correlation between the instruments and the endogenous regressor.

The second condition for instrument validity is the test of lack of correlation between the errors in the second stage and the excluded instruments from the first-stage regressions. The Hansen J statistic is a joint test of the lack of correlation between the errors in the second stage regression and the excluded instruments and the correct exclusion of the instruments from the second stage regressions. Under the null the Hansen J statistic must satisfy both the aforementioned conditions. The Hansen J statistic is also appended in tables 5-10, where we cannot reject the null i.e. the instruments are uncorrelated with the error term and appropriately excluded from the second stage regression specification.

The preferred estimates of PCE reported in this paper are robust to an important econometric concern, i.e., weak instrument problem. The weak instrument problem has received very little attention in the schooling literature. Hence, our preferred estimates are much more robust as compared to the parameter estimates reported earlier in this literature.

In a earlier work, Behrman and Knowles (1999) review the role of household income using 42 studies covering 21 countries show that there exists significant association between schooling outcomes and income, as also established in this paper. They find that about three-fifths of the schooling indicators show significant associations between schooling and income. They compute the median income elasticity to be 0.07 with the highest being 0.20 for low income countries - Ghana, Cote d'Ivoire, China and Nepal. The coefficient estimates on income elasticity reported in Behrman and Knowles (1999) is likely to be a lower bound on the true estimate since most studies used to compute the elasticity does not treat measures of PCE as endogenous. Additionally rural areas in general have higher income effects as compared to urban areas. The strong association between income and schooling outcomes also often reflects the existence of other unobservable factors like poor credit markets, parental preferences, household's ability to pay fees and substitute for farm labor. Since the income measure reported here also potentially captures the impact of unobservable factors, hence policy makers must target more than one determinant of household income so that improvements in income can have a large impact on the improvements in schooling outcomes.

5.1.4 Household composition variables and supply side factors

We use number of adult males, and number of adult females as additional right hand side variables to control for the impact of household composition on schooling outcomes. The coefficient estimates on adult males, adult females, mother's age and age of the head of the household do not suggest any systematic pattern in their impact of schooling outcomes.

The usual supply side characteristics that determine schooling outcomes include - number of schools available, distance to school, availability of water and sanitation facility in the community, teacher-pupil ratio, number of blackboards, teaching materials and quality of road in the village. There exists a number of papers which establish the impact of distance to school, school characteristics (no. of blackboards, no. of desks, teacher-pupil ratio, leaking classrooms) and community resources to have a significant role in determining schooling outcomes [Glewwe and Jacoby (1994), Glewwe and Jacoby (1995), Dostie and Jayaraman (2006), Lavy (1996), Schaffner (2004)]. However, the magnitude and the role played by the supply side factors have not been systematically established in the literature.

The ERHS did not collect detailed supply side information for all three waves of the survey data used in this paper, except for 2004. In order to establish the importance of the supply side characteristics, we replace the village level fixed-effect estimates with the actual supply side characteristics in columns 5 and 6 of tables 7 and 10. The supply side factors included are - distance to primary school measured in km, dummy for access to electricity, and dummy for availability of piped water.

We find that the coefficient estimates on the supply side factors reported in column 6, table 7 are all statistically insignificant and have little impact in determining schooling enrollments. The coefficient estimates reported in column 6, table 10 indicate that distance to primary school in km and the availability of electricity in the village both have a statistically significant impact on determining relative grades. Distance to school has a negative impact on the child's relative grades. This is consistent with most other work in the literature [Lavy (1996), Schaffner, (2004)]. Children residing in villages that have access to electricity have higher levels of relative grades compared to children who live in villages without access to electricity.

There is evidence to suggest that the correlation between village specific unobservables and the supply side characteristics biases the coefficient estimates on the supply side variables [Rosenzweig and Wolpin (1986)]. Ghuman et. al (2005) shows that the potential correlation between household specific observables and the village specific unobservables can bias the parameter estimates obtained on the household characteristics as well. Therefore, it would be useful to compare the extent of bias in the household characteristics by providing estimates with village fixed-effects and replacing these village dummies with the actual supply side variables.

In the regressions using the 2004 data, we compare our coefficient estimates on the household characteristics with both village fixed-effects and replace these village level fixed-effects with the actual supply side characteristics, to determine the extent of bias (if any) in the household characteristics with the exclusion of the village fixed-effects. We find that the inclusion of the village supply side characteristics does not change the parameter estimates on the demand side variables (for instance parental schooling and household income) reported in column 4 of tables 7 and 10. This indicates that the inclusion of the actual supply side factors is not likely to bias the demand side coefficient estimates.

In addition, to the role played by household income, the impact of child and household characteristics reported in this paper are all consistent with other related work done by Chaudhury et. al (2006) and Schaffner (2004) using cross sectional data from Ethiopia. The aforementioned papers have additionally emphasized on the role played by the school supply side characteristics and village level characteristics in determining schooling improvements in rural Ethiopia. In our work, due to limited data availability on community characteristics we have no direct evidence on the role played by the village characteristics in improving schooling outcomes. The main limitations associated with the analysis of the supply side factors in the aforementioned papers is that they can neither account for endogenous program placement effects and nor do they acknowledge the potential correlation between village specific unobservables and household specific observables that can bias the coefficient estimates obtained on the household characteristics as well.

5.2 Dynamic results

The static results discussed so far only capture the impact of current socioeconomic factors in explaining current schooling outcomes. However, schooling outcome in any period t is not just a function of current period factors and resources. It is the demand and supply side factors from all periods (0 to t) that determine an individual's complete trajectory of schooling outcomes. In order to capture the impact of all factors that led to the choice of current schooling outcomes, we estimate a dynamic conditional schooling demand function, where the coefficient on the one period lagged schooling outcome captures the history of all demand and supply side determinants of schooling outcomes.

We observe that in rural Ethiopia, majority of the improvements in household income took place between 1994 and 1999 with little change in income between 1999 and 2004. Despite the little change in household income during 1999-2004, schooling outcomes continue to improve. This section establishes the relationship between the demand for schooling in the last period and its continued impact on current schooling outcomes. For estimating a dynamic conditional schooling demand function, we construct a panel data on primary school age children between 7-14 years in 1994 and follow them through the 1999 and 2004 waves of the ERHS.

We first estimate a static schooling demand function using observations on the panel respondents, pooling data from the 1994, 1999, and 2004 waves of the ERHS. In Table 13, preferred IV estimates are reported in column 1, where the enrollment dummy is regressed on a set of child level, household level and community level characteristics using observations on the panel respondents.

Table 13: Determinants of enrollment and relative grades attained among panel respondents

Covariates	(1) IV Enroll	(2) IV RGA
Male dummy	-0.0961	0.0003
	(0.62)	(0.32)
Spline if age<=14.99	-0.0071	-0.0117
	(0.03)	(0.01)
Spine if 14.99<age<17.99	-0.0328	-0.0098
	(0.02)	(0.01)
Spline if age>=17.99	-0.0309*	0.0078
	(0.01)	(0.009)
Male dummy*spline if age<=14.99	0.0108	0.0037
	(0.04)	(0.02)
Male dummy*spine if 14.99<age<17.99	0.0214	0.0083
	(0.03)	(0.01)
Male dummy*spline if age>=17.99	-0.0206	-0.0021
	(0.02)	(0.01)
Mother's schooling	0.1406***	0.0547**
	(0.05)	(0.02)
Father's schooling	0.0816**	0.0557**
	(0.03)	(0.02)
Log (real pce)	0.5097**	0.2494*
	(0.25)	(0.14)
Number of adult males	0.0160	0.0102
	(0.01)	(0.008)
Number of adult females	0.0801***	0.0243
	(0.02)	(0.01)
Mother's age	-0.0043***	-0.0006
	(0.001)	(0.0008)
Age of the head of the household	-0.0026 (0.002)	-0.0021* (0.001)
observations	1618	1618
village*time fixed-effects	Yes	Yes
F statistic on the	6.12	6.12

excluded instruments from the first-stage regressions	(0.00)	(0.00)
Hansen J statistic	0.85 (0.65)	2.97 (0.22)

- Robust standard errors in parentheses
- *** significant at 1%; ** significant at 5%; * significant at 10%
- p-values are reported for the F statistic on the excluded instruments and the Hansen J statistic
- In columns (1) and (2), PCE is instrumented with two-period lagged measure of - land and livestock units
- Also included in the regressions are dummy variables capturing missing observations for each particular variable (parental schooling, land, household composition variables) where the missing observation was imputed by the sample mean

In column 2, similar estimates are reported for relative grade attainment. The coefficient estimates on the parental schooling and household income indicate a positive relationship with enrollment and relative grade attainments, in line with much of the results outlined in the previous section. The actual parameter estimate is slightly higher now for the parental schooling variables and particularly higher for household income. The difference in the magnitude of the parameter estimates obtained from the cross-sectional and panel regressions for the static model indicate strong sample composition effects, that is, the sample itself is changing over time and hence the relative impact of parental schooling and household income is different using cross-sectional observations and the panel observations.

In the dynamic specification, the coefficient estimate on the lagged enrollment status/lagged relative grade attainment is of primary interest. OLS coefficient estimates of the lagged dependent variable are likely to be biased due to the presence of omitted variables and random measurement error in data. Omitted variable bias stems from the positive correlation between the lagged schooling outcome and other child and household specific time-invariant unobservables which creates an upward bias in the estimated coefficient on the lagged dependent variable. Random measurement error in data additionally biases the estimated coefficient on the lagged outcome variable towards zero. The lagged PCE variable controlled in the right hand side of the dynamic specification captures for household's access to resources in the long-run. This measure of household income is also treated as endogenously determined due to both omitted variables problem measurement error problem in household income.

We use variants of the IV/GMM estimation strategy to deal with the endogeneity in the lagged schooling variable and lagged PCE. The coefficient estimates from the dynamic regressions are reported in tables 14 and 15.

Table 14: Dynamic schooling enrollment demand function

Covariates	(1) OLS Enroll	(2) IV Enroll	(3) IV Enroll	(4) IV Enroll	(5) IV Enroll
Lagged enrollment	0.3422***	0.3220***	0.3961***	0.2466***	0.2970***
	(0.02)	(0.09)	(0.04)	(0.09)	(0.09)
Lagged log (real pce)	0.0046	-0.0311	-0.0169	-0.0330	-0.0487*
	(0.01)	(0.04)	(0.03)	(0.04)	(0.02)
Male dummy	0.0288		0.0912		
	(0.11)		(0.45)		
Spline at lag age <14.99 years	-0.0249***	-0.4292	-0.0181	-0.4263	-0.4125
	(0.008)	(0.55)	(0.02)	(0.53)	(0.54)
Spline at lag age >=14.99 years	-0.0339**	-0.4288	-0.0396**	-0.4323	-0.4140
	(0.01)	(0.54)	(0.01)	(0.52)	(0.54)
Male dummy*Spline at lag age <14.99 years	0.0029	0.0006	-0.0026	0.0008	0.0007
	(0.009)	(0.01)	(0.03)	(0.01)	(0.01)
Male dummy*Spline at lag age >=14.99 years	-0.0090	-0.0135	-0.0037	-0.0123	-0.0127
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Mother's schooling	0.0983**		0.07423		
	(0.04)		(0.05)		
Father's schooling	0.0627**		0.0224		
	(0.03)		(0.04)		
Lagged no. of adult males	0.0054	-0.0114	-0.0105	-0.0091	-0.0108
	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)
Lagged no. of adult females	0.0302**	0.0186	0.0353***	0.0195	0.0172
	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)
Mother's age	-0.0024*		-0.0030*		
	(0.001)		(0.001)		
Lagged age of the head of the household	-0.0013	-0.0044	0.0008	-0.0039	-0.0040
	(0.001)	(0.003)	(0.001)	(0.003)	(0.003)
Observations	1618	809	809	809	809
Number of village*time fixed-effects	Yes	Yes	Yes	Yes	Yes
F statistic on the excluded instruments from the first-stage regressions		111.34 (0.00)	728.82 (0.00)	61.03 (0.00)	116.78 (0.00)
Hansen J statistic		0.00	0.00	11.60 (0.003)	2.27 (0.13)
Difference on the coefficient estimate on first-differenced lagged log (PCE) obtained using a Hausman specification					0.01 (0.03)

- Robust standard errors in parentheses
- *** significant at 1%; ** significant at 5%; * significant at 10%
- p-values are reported for the F statistic on the excluded instruments and the Hansen J statistic
- In column (2), first-differences in lagged PCE and lagged enrollment are instrumented with two-period lagged PCE and two-period lagged enrollment
- In column (3), lagged enrollment and lagged PCE are instrumented with the first-differences in lagged PCE and first-differences in lagged enrollment
- In column (4), first-differences in lagged enrollment and lagged PCE are instrumented with two-period lagged enrollment, two-period lagged PCE, two-period lagged PCE*two-period lagged rainfall, two-period lagged rainfall*mother's schooling
- In column (5), first-differences in lagged enrollment is instrumented with two-period lagged enrollment, two-period lagged rainfall*mother's schooling, first-differenced lagged pce is now treated as exogenous
- Also included in the regressions are dummy variables capturing missing observations for each particular variable (parental schooling, land, household composition variables) where the missing observation was imputed by the sample mean

Table 15: Dynamic schooling relative grade attainment demand function

Covariates	(1) OLS RGA	(2) IV RGA	(3) IV RGA	(4) IV RGA	(5) IV RGA
Lagged relative grade attainment (RGA)	0.6200***	0.2568***	0.7303***	0.2579***	0.2566***
	(0.02)	(0.06)	(0.06)	(0.06)	(0.06)
Lagged log (real pce)	0.0213***	-0.0030	0.0081	-0.0029	0.0051
	(0.007)	(0.01)	(0.01)	(0.01)	(0.007)
Male dummy	0.0134		-0.0670		
	(0.05)		(0.16)		
Spline at lag age <14.99 years	-0.0096**	-0.0494	0.0133	-0.0495	-0.0567
	(0.004)	(0.14)	(0.009)	(0.14)	(0.14)
Spline at lag age >=14.99 years	0.0141*	-0.0298	0.0022	-0.0299	-0.0373
	(0.007)	(0.14)	(0.008)	(0.14)	(0.14)
Male dummy* Spline at lag age <14.99 years	0.0022	0.0019	0.0069	0.0019	0.0018
	(0.004)	(0.003)	(0.01)	(0.003)	(0.003)
Male dummy*Spline at lag age >=14.99 years	0.0005	0.0011	0.0018	0.0011	0.0009
	(0.009)	(0.007)	(0.01)	(0.007)	(0.007)
Mother's schooling	0.0329*		0.0468**		
	(0.01)		(0.02)		
Father's schooling	0.0130		0.0129		
	(0.01)		(0.01)		
Lagged no. of adult males	0.0111**	0.0112	0.0125**	0.0112	0.0115*
	(0.004)	(0.006)	(0.0058)	(0.006)	(0.006)
Lagged no. of adult females	0.0091*	0.0150*	0.0148**	0.0150*	0.0158*
	(0.005)	(0.008)	(0.006)	(0.008)	(0.008)
Lagged mother's age	-0.0003		-0.0002		
	(0.0006)		(0.0006)		
Lagged age of the head of the household	-0.0008	0.0009	-0.0001	0.0009	0.0008
	(0.0005)	(0.0009)	(0.0007)	(0.0009)	(0.0009)
Observations	1618	809	809	809	
Village*time fixed-effects	Yes	Yes	Yes	Yes	Yes
F statistic on the excluded instruments		145.86	30.43	95.18	141.77
from the first-stage regressions		(0.00)	(0.00)	(0.00)	(0.00)
Hansen J statistic		0.00	0.00	0.02 (0.88)	0.05 (0.81)
Difference on the coefficient estimate					-0.008
on first-differenced lagged log (PCE)					(0.01)
obtained using a Hausman specification					

- Robust standard errors in parentheses
- *** significant at 1%; ** significant at 5%; * significant at 10%
- p-values are reported for the F statistic on the excluded instruments and the Hansen J statistic
- In column (2), first-differences in lagged PCE and lagged RGA are instrumented with two-period lagged PCE and two-period lagged RGA
- In column (3), lagged RGA and lagged PCE are instrumented with the first-differences in lagged PCE and first-differences in lagged RGA
- In column (4), first-differences in lagged RGA and lagged PCE are instrumented with two-period lagged RGA, two-period lagged PCE, two-period lagged PCE*two-period lagged rainfall, two-period lagged rainfall*mother's schooling
- In column (5), first-differences in lagged RGA are instrumented with two-period lagged RGA, two-period lagged rainfall*mother's schooling, first-differenced lagged pce is now treated as exogenous
- Also included in the regressions are dummy variables capturing missing observations for each particular variable (parental schooling, land, household composition variables) where the missing observation was imputed by the sample mean

We follow two variants of the GMM estimation strategy - Arellano-Bond (1991), and Arellano-Bover (1995). Using the approach outlined in Arellano-Bond (1991) we instrument the first-differences in lagged schooling outcome (enrollment and relative grade attainment) variable and lagged PCE with the two-period lagged schooling outcome variable and two period lagged PCE respectively, assuming that the measurement error in schooling outcomes and household pce are serially uncorrelated over time.

Following an Arellano-Bover (1995) estimation strategy, we instrument the levels in lagged PCE and lagged schooling outcome variables with the first-differences in lagged PCE and first-differences in lagged schooling outcome (enrollment and relative grade attainment) variables under the assumption that the first-differenced variables are uncorrelated with the time-invariant unobservables.

Lastly, we follow another variant of the Arellano-Bond estimator, where we instrument the first-differences in lagged PCE and first-differences in lagged schooling outcome (enrollment and relative grade attainment) variables with the two period lagged schooling outcome, two period lagged PCE interacted with the two period lagged rainfall and the two period lagged rainfall interacted with mothers schooling.

All the three estimation strategies above rely on the assumption of lack of first-order and second-order serial correlation in the error terms for the schooling outcome. This is a strong assumption and may not necessarily be satisfied in a dynamic panel model [Deaton (1997), Blundell and Bond (1998)]. However, little can be done to test this assumption with a short panel and especially when we do not have instruments that do not rely on this assumption.

Our preferred IV estimates on the lagged dependent variable are reported in columns 2 and 4 of tables 14 and 15. The parameter estimate on the lagged outcome variable indicates a strong positive association between current schooling outcomes and lagged schooling outcomes. The dynamic enrollment regressions indicate that a child who was enrolled in the last period is 32 percentage points more more likely to be enrolled today compared to a child who was not enrolled in the last period. This suggests that even a one time policy initiative taken on the part of governments will translate into continued enrollments in subsequent periods as captured by the coefficient estimate on the lagged enrollment variable.

We find that the OLS coefficient estimates from the dynamic enrollment regressions in table 14 are quite close to results obtained using the different GMM estimation strategies. This suggests that the usual upward bias expected in the OLS coefficient estimate of the lagged dependent variable is offset by the downward bias caused by random measurement error thereby bringing the total bias close to zero. It is also possible that the bias caused by random measurement error is minimal and the ability bias[23] is not an important source of bias in estimating dynamic enrollment regressions.

We also examine the relationship between current relative grade attainments and lagged relative grade attainments. The dynamic regression results for relative grade attainments are reported in table 15. We find that higher grade attainment in the last period is positively associated with grade attainment in the current period. Our preferred estimate of 0.25 on lagged relative grade attainment indicates that lower the relative grades accumulated in the initial period the greater will be the difference between actual grades and potential grades accumulated over life course.

The OLS coefficient estimate on the lagged dependent variable reduces from 0.62 to 0.25 (table 15) following an Arellano-Bond estimation strategy. Measurement error in grade attainments is much lower as compared to data on enrollments as extensive effort was put into ensuring that the data on completed grades of schooling was consistent. However, for enrollment we had to simply rely on the individual's responses and little double checks were possible to reduce measurement error. The low measurement error and potentially higher source of 'ability bias' in grade attainments, makes OLS coefficient estimates of lagged relative grade attainment biased upwards. First-differencing eliminates the sources of the upward bias on the lagged dependent variable as noted in the parameter estimates obtained using Arellano-Bond.

[23] Ability bias is the bias caused due to the correlation between an individual's innate ability and his lagged schooling outcome. Children with higher ability are more likely to be enrolled in school. They are also likely to accumulate higher completed grades of schooling on an average.

In column 5 of tables 14 and 15, we also report coefficient estimates on lagged schooling outcome variable using our preferred Arellano-Bond type estimation strategy, now treating lagged pce as exogenous. We also report the difference on the coefficient estimates obtained on lagged pce in tables 14 and 15 as obtained from specifying a Hausman specification test to test if the assumption of treating lagged pce as exogenous is valid. The coefficient estimates on the difference in lagged pce reported in table 14 and 15 are 0.01 and 0.008 respectively and neither of these estimates are statistically significant, which implies that the null of exogeneity of the lagged consumption variable is not rejected.

The IV estimates obtained in the dynamic specifications are valid and satisfy the conditions necessary for a valid instruments outlined earlier. The F statistic on the excluded instruments and Hansen J statistic appended at the end of tables 14 and 15 satisfy the conditions necessary for valid instruments.

The coefficients on the lagged schooling outcome variables indicate a strong positive relationship between current and lagged schooling outcome variables. The magnitude and the sources of bias between the OLS and IV estimates differ by the measure of schooling outcome used in the regression specification. For example: the OLS estimate are close to the other IV estimates found in the enrollment regressions and the OLS estimates are quite different from the IV estimates found in the relative grade attainment regressions.

6 Concluding remarks

In this paper, we examine the impact of individual level and household level characteristics in determining current schooling outcomes as measured by enrollment status and relative grade attainments using both cross-sectional and panel data methods. Mother's education, father's education, log of real per capita consumption expenditure, age of the child and gender of the child are identified as some important determinants of schooling outcomes.

We find the role played by household income in explaining the improvements in schooling attainments is strong and has substantially increased during the last decade. Our preferred estimates on household income increase from 0.06 in 1994 to 0.17 in 2004 capturing the role played by income during both low income periods (1994) and high income periods (2004) as indicated by the coefficient estimates. We treat PCE as being endogenously determined and show that inference based on OLS estimates of household income can be potentially misleading. The IV coefficient estimates of income reported in this paper is also robust to the problem of weak instruments.

In addition to the static regressions we also estimate a dynamic conditional schooling demand function to determine the association between current schooling and lagged schooling. We find strong associations between current schooling and lagged schooling outcomes which are omitted from the static regression results. We find that children who were enrolled in the last period is 32 percentage points more likely to be enrolled today as compared to children who were not enrolled in the last period. We also find that individual's past schooling resources contributes towards child's current relative grade attainments. The coefficient on the lagged schooling outcome explains for the continued improvements in schooling outcomes. The coefficient estimates reported in the dynamic specification are robust to omitted variables and measurement error in data, under the assumption that random time-varying error term in the schooling outcome variable is serially uncorrelated over time.

The coefficient estimates reported in this paper are robust to a number of methodological and econometric concerns. We have shown earlier in the paper that the results obtained are not likely to be contaminated by concerns regarding non-random sample selection, attrition, omitted variables bias, measurement error bias, and weak instrument bias.

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Appendix

Table 16: Determinants of sample attrition

Covariates	(1) OLS attrition	(2) OLS attrition	(3) OLS attrition
Completed grades of schooling	-0.0128*		
	(0.007)		
Relative grades attained		-0.0713***	
		(0.02)	
Enrollment status			0.0343
			(0.03)
Mother's schooling	0.0577	0.0579	0.0497
	(0.04)	(0.04)	(0.04)
Father's schooling	-0.0284	-0.0263	-0.0380
	(0.02)	(0.02)	(0.02)
Log of real pce	-0.0322**	-0.0321**	-0.0349**
	(0.01)	(0.01)	(0.01)
Male dummy	0.0201	0.0237	0.0191
	(0.05)	(0.05)	(0.05)
dummy=1 if the child	-0.1085*	-0.1080*	-0.1116**
is 8 years	(0.05)	(0.05)	(0.05)
dummy=1 if the child	-0.1548***	-0.1565***	-0.1600***
is 9 years	(0.05)	(0.05)	(0.05)
dummy=1 if the child	-0.1246**	-0.1276**	-0.1299**
is 10 years	(0.05)	(0.05)	(0.05)
dummy=1 if the child	-0.2469***	-0.2515***	-0.2561***
is 11 years	(0.06)	(0.05)	(0.06)
dummy=1 if the child	-0.2770***	-0.2824***	-0.2902***
is 12 years	(0.05)	(0.05)	(0.05)
dummy=1 if the child	-0.3551***	-0.3623***	-0.3733***
is 13 years	(0.05)	(0.05)	(0.05)
dummy=1 if the child	-0.3586***	-0.3670***	-0.3731***
is 14 years	(0.05)	(0.05)	(0.05)
Male dummy*dummy=1 if	0.1018	0.0975	0.1036
the child is 8 years	(0.08)	(0.08)	(0.08)
Male dummy*dummy=1 if	0.1308	0.1292	0.1293
the child is 9 years	(0.08)	(0.08)	(0.08)
Male dummy*dummy=1 if	0.0404	0.0376	0.0376
the child is 10 years	(0.07)	(0.07)	(0.07)
Male dummy*dummy=1 if	0.1428*	0.1404*	0.1360
the child is 11 years	(0.08)	(0.08)	(0.08)
Male dummy*dummy=1 if	0.1850**	0.1818**	0.1849**
the child is 12 years	(0.07)	(0.07)	(0.07)
Male dummy*dummy=1 if	0.2466***	0.2422***	0.2456***
the child is 13 years	(0.07)	(0.07)	(0.07)
Male dummy *dummy=1 if	0.0964	0.0902	0.0817
the child is 14 years	(0.07)	(0.07)	(0.07)
Number of adult males	-0.0132	-0.0135	-0.0142
	(0.009)	(0.009)	(0.009)

Number of adult females	-0.0135	-0.0140	-0.0121
	(0.01)	(0.01)	(0.01)
Mother's age	-0.0006	-0.0005	-0.0012
	(0.001)	(0.0008)	(0.001)
Age of the head	-0.0012	-0.0012	-0.0006
of the household	(0.0008)	(0.001)	(0.0008)
Village fixed-effects	Yes	Yes	Yes
observations	2047	2047	2047

- Source: ERHS - 1994; robust standard errors reported in the parenthesis

- *** significant at 1%, ** significant at 5%, * significant at 10%

- Attrition = 1 if the individual can be followed through the 1994, 1999, and 2004 waves of the ERHS and zero otherwise

Table 17: Preferred IV estimates for Boys from 1994, 1999, 2004

Covariates	(1) IV Enroll 1994	(2) IV RGA 1994	(3) IV Enroll 1999	(4) IV RGA 1999	(5) IV Enroll 2004	(6) IV RGA 2004
Mother's schooling	0.0909*	-0.0368	0.0883	0.0198	0.0446	0.0823
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)
Father's schooling	0.1331***	0.1243***	0.1208***	0.1292***	0.0904**	0.0821*
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Log (real pce)	0.0106	0.1051	-0.0830	-0.0352	0.2111*	0.0844
	(0.09)	(0.08)	(0.18)	(0.14)	(0.11)	(0.10)
dummy=1 if the	0.0121	-0.0474	0.2381***	-0.0466	0.1916***	0.0743
child is 8 years	(0.02)	(0.07)	(0.05)	(0.11)	(0.05)	(0.07)
dummy=1 if the	0.0662*	0.0221	0.2574***	-0.0871	0.2829***	0.0062
child is 9 years	(0.03)	(0.07)	(0.05)	(0.11)	(0.05)	(0.06)
dummy=1 if the	0.1064***	-0.0089	0.4878***	0.0066	0.3503***	0.0788
child is 10 years	(0.03)	(0.07)	(0.05)	(0.10)	(0.05)	(0.06)
dummy=1 if the	0.1758***	0.0394	0.4895***	-0.0111	0.4618***	0.0960
child is 11 years	(0.03)	(0.07)	(0.05)	(0.10)	(0.06)	(0.06)
dummy=1 if the	0.1504***	0.0055	0.4808***	0.0224	0.4820***	0.0743
child is 12 years	(0.03)	(0.07)	(0.05)	(0.11)	(0.05)	(0.06)
dummy=1 if the	0.1897***	0.0134	0.4259***	-0.0298	0.5015***	0.1390**
child is 13 years	(0.04)	(0.07)	(0.06)	(0.10)	(0.05)	(0.06)
dummy=1 if the	0.2567***	0.0857	0.4930***	-0.0368	0.6088***	0.1619***
child is 14 years	(0.04)	(0.07)	(0.05)	(0.10)	(0.05)	(0.06)
Number of adult	0.0044	-0.0181	-0.0035	0.0211	-0.0001	0.0075
males	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
Number of adult	-0.0127	-0.0090	0.0084	-0.0289	0.0379*	-0.0023
females	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Mother's age	0.0019	-0.0000	-0.0052***	-0.0021	-0.0038*	-0.0024
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
Age of the head	-0.0011	0.0004	-0.0014	0.0023	-0.0012	0.0021
of the household	(0.0008)	(0.0006)	(0.001)	(0.002)	(0.001)	(0.001)
Village fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes

- Robust standard errors in parentheses
- *** significant at 1%; ** significant at 5%; * significant at 10%
- In columns 2-6, PCE is instrumented with land, livestock units, and rainfall*land
- omitted age dummy - 7 years

- Also included in the regressions are dummy variables capturing missing observations for each particular variable (parental schooling, land, household composition) variables where the missing observation was imputed by the sample mean

Table 18: Preferred IV estimates for girls from 1994, 1999, 2004

Covariates	(1) IV Enroll 1994	(2) IV RGA 1994	(3) IV Enroll 1999	(4) IV RGA 1999	(5) IV Enroll 2004	(6) IV RGA 2004
Mother's schooling	0.1242**	0.1477*	-0.0091	-0.0449	0.0784	0.0733*
	(0.06)	(0.07)	(0.06)	(0.06)	(0.05)	(0.04)
Father's schooling	0.0820***	0.0999***	0.0143	0.0346	0.0443	-0.0034
	(0.02)	(0.02)	(0.05)	(0.04)	(0.04)	(0.03)
Log (real pce)	-0.0709	0.1260*	0.6134***	0.4948**	0.1599	0.1183
	(0.06)	(0.06)	(0.20)	(0.20)	(0.12)	(0.10)
dummy=1 if the	0.0303	0.0363	0.0691	-0.2054**	0.1234**	0.0450
child is 8 years	(0.02)	(0.04)	(0.07)	(0.08)	(0.05)	(0.07)
dummy=1 if the	0.0738***	0.0108	0.1554**	-0.1265	0.2564***	0.0068
child is 9 years	(0.02)	(0.04)	(0.06)	(0.08)	(0.05)	(0.06)
dummy=1 if the	0.0563**	0.0054	0.1322*	-0.2411***	0.4184***	0.1083*
child is 10 years	(0.02)	(0.03)	(0.07)	(0.08)	(0.05)	(0.06)
dummy=1 if the	0.0897**	0.0313	0.1625*	-0.2246**	0.4252***	0.1089
child is 11 years	(0.03)	(0.04)	(0.08)	(0.08)	(0.06)	(0.07)
dummy=1 if the	0.1362***	0.0250	0.2978***	-0.1686**	0.5184***	0.1553**
child is 12 years	(0.03)	(0.04)	(0.06)	(0.08)	(0.05)	(0.06)
dummy=1 if the	0.1894***	0.0550	0.2761***	-0.2043**	0.4522***	0.0792
child is 13 years	(0.03)	(0.04)	(0.07)	(0.08)	(0.06)	(0.06)
dummy=1 if the	0.0787**	0.0294	0.2492***	-0.2244**	0.4240***	0.1270**
child is 14 years	(0.03)	(0.04)	(0.07)	(0.08)	(0.05)	(0.06)
Number of adult	0.0217*	0.0115	0.0280	0.0413**	-0.0008	-0.0177
males	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Number of adult	-0.0223**	-0.0093	0.0551**	0.0173	0.0535*	0.0423*
females	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Mother's age	0.0003	0.0014	-0.0032	0.0004	-0.0001	0.0004
	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)
Age of the head	0.0006	0.0001	-0.0001	0.00001	-0.0010	-0.0009
of the household	(0.0008)	(0.0007)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	1014	1014	935	935	786	786
Village fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes

- Robust standard errors in parentheses
- *** significant at 1%; ** significant at 5%; * significant at 10%
- p-values are reported for the F statistic on the excluded instruments and the Hansen J statistic
- In columns 2-6, PCE is instrumented with land, livestock units, and rainfall*land
- omitted age dummy - 7 years
- Also included in the regressions are dummy variables capturing missing observations for each particular variable (parental schooling, land, household composition variables) where the missing observation was imputed by the sample mean