Journal of Development Economics 99 (2012) 292–299

Contents lists available at SciVerse ScienceDirect

Journal of Development Economics

journal homepage: www.elsevier.com/locate/devec

Long-term impact of investments in early schooling — Empirical evidence from rural Ethiopia

Subha Mani [a,*], John Hoddinott [b], John Strauss [c]

[a] Fordham University, United States
[b] International Food Policy Research Institute, United States
[c] University of Southern California, United States

A R T I C L E I N F O

Article history:
Received 6 November 2009
Received in revised form 9 March 2012
Accepted 17 March 2012

JEL classification:
I21
012
C23

Keywords:
Schooling
Path dependence
Panel data
Dynamic model

A B S T R A C T

We examine the cumulative impact of early schooling investments on later schooling outcomes using enrollment status and relative grade attainment as short-run and long-run measures of schooling. Using a child-level longitudinal data set from rural Ethiopia, we estimate a dynamic conditional schooling demand function where the coefficient estimate on the lagged dependent variable captures the impact of all previous period schooling inputs and resources. We find that a child who is enrolled in the prior period is 33 percentage points more likely to be enrolled currently. These lagged effects are stronger for girls and for children from higher income households.

1. Introduction

In much of the developing world, households reside in risky environments. In the absence of full insurance or other smoothing mechanisms, the realization of these shocks, leads to losses of utility. As Alderman et al. (2006), Dercon (2005)and others have noted, the importance of these losses from a policy perspective depends partly on whether such shocks induce path dependence. That is, do transitory shocks have permanent consequences? Or put another way, is past history destiny?

In the last ten years, a series of papers have demonstrated that in the context of one dimension of human capital, health, does indeed demonstrate path dependence (e.g. the now vast literature on fetal origins, Barker, 1994). Maccini and Yang (2009) have shown that rainfall in the year and district of birth in Indonesia have long-run effects, on both attained adult height for men and women and on completed years of schooling for women. Alderman et al. (2006) have shown that early childhood health as measured by height, has lasting effects on the level of schooling completed, among children in rural Zimbabwe. Hoddinott and Kinsey (2001) and Mani (2011) find evidence of path dependence in child heights, in rural Zimbabwe and Indonesia respectively.

Schooling outcomes – such as the decision to continue or withdraw from school, or to enroll having previously not enrolled in school – would seem to be intimately linked to past schooling decisions which themselves were influenced by prior community, school and home resources. The "value-added" specification of human capital accumulation, in which the impact of lagged schooling inputs is captured (Boardman and Murnane, 1979; Hanushek, 1979; Jacob et al., 2010; Kane and Staiger, 2008; Rothstein, 2010; Todd and Wolpin, 2003, 2007) has been used in the context of developed countries to explore these and related issues for one schooling measure, primarily test scores. In the fourth volume of the *Handbook of Development Economics*, Orazem and King (2008) write, "Longitudinal analysis of cognitive attainment is needed to establish whether lost human capital from transitory increases in child labor or school absences due to adverse income shocks is reversible or permanent" (p 3550).[1]

This paper contributes evidence on this issue by — (a) using the value-added specification of human capital accumulation to capture the cumulative impact of past schooling inputs and resources on future schooling outcomes–enrollment status and relative grade attainment, short-run and long-run indicators of schooling; (b) estimating a dynamic conditional schooling demand function that replaces the endogenous schooling inputs with exogenous observables and accounts for

* Corresponding author at: Department of Economics, Fordham University, 441 East Fordham Road, Bronx, NY 10458, United States. Tel. +1 2154504000.
E-mail address: smani@fordham.edu (S. Mani).

[1] Andrabi et al. (2011) are the only exception who have analyzed the impact of lagged test scores on current test score using data from a developing country, Pakistan.

0304-3878/$ – see front matter © 2012 Elsevier B.V. All rights reserved.
doi:10.1016/j.jdeveco.2012.03.002

the problem of missing school inputs; (c) drawing on estimation strategies that address the potential correlations between lagged schooling outcome and unobserved endowments; (d) tests for the assumption of no serial correlation in dynamic panel data models and (e) captures some heterogeneity in path dependence. It does using data from rural Ethiopia, a poor African country with low (though rising) levels of grade attainment.[2]

We find that a child who is enrolled in the last period is 33 percentage points more likely to be enrolled today compared to his counterpart who was not enrolled five years ago. We also find that past levels of relative grade attainment affect current levels of this outcome. About 25% of current progression comes from lagged progression, that is, one-fourth of schooling progression depends on the individual's relative grade attainment 5 years ago. Both findings suggest that there is significant path dependence in schooling outcomes. This path dependence in schooling also varies with background characteristics, and is much stronger for girls than boys, and for children from high income compared to lower income households.

The paper is organized as follows. Section 2 describes the data, Section 3 outlines the empirical specification, the results are discussed in Section 4 and concluding remarks follow in Section 5.

2. Data

The data used here is taken from the 1994, 1999, and 2004 waves of the Ethiopian Rural Household Survey (ERHS). The ERHS began in 1989, when a survey team visited 6 Peasant Associations in Central and Southern Ethiopia.[3] The survey was expanded in 1994 to include an additional nine Peasant Associations, yielding a sample of 1477 households. As part of the survey re-design and extension that took place in 1994, the samples in the original six villages were adjusted so as to be representative at the village level. The nine additional PAs were selected to better account for the diversity in the farming systems found in Ethiopia. The sample was stratified within each village to ensure that a representative number of landless and female-headed households were included. As Dercon and Hoddinott (2004) explain, the population shares within the sample are broadly consistent with the population shares in the three main sedentary farming systems in existence at that time. In fact, the sample sizes in each village were chosen so as to approximate a self-weighting sample, when considered in terms of farming system: each person (approximately) represents the same number of persons found in the main farming systems as of 1994. However, because the ERHS comprises only 15 localities in a country of thousands of villages, Dercon and Hoddinott (2004) have stressed that extrapolation from these results should be done with care.

The ERHS provides extensive information on household composition, income, consumption expenditure, farm and non-farm assets, ownership and value of land and livestock units, anthropometrics, harvest use and schooling outcomes. The ERHS only collected detailed community level information in 1997 and 2004.

In all survey rounds, except 1995, data was also collected on children's school enrollment status and grade attainment. In addition, to avoid complications arising from the irregular spacing of survey rounds, we construct a child-level longitudinal data set that follows children aged 7 to 14 years (i.e., those of primary school age) in 1994 through the 1999 and 2004 waves of the ERHS. The 1994, 1999 and 2004 rounds were fielded in approximately the same months thereby avoiding seasonality concerns. Sample averages on enrollment and relative grade attainment stratified by age, year and gender are reported in panels A and B of Table 1. We find that average enrollments and relative grade attainment have all consistently increased during 1994–2004 for both male and female children capturing rapid progress in schooling outcomes during this period in rural Ethiopia.

As with any longitudinal data set, there are always concerns regarding selective sample attrition. Household level attrition is minimal in the ERHS; only 13% of the sample was lost between 1994 and 2004. This partly reflects the relative immobility of the sample (it is difficult to obtain land if households migrate) and partly a high degree of institutional continuity in the development of these surveys (see Dercon et al. (2006)).

We observe 1849 primary school age children in 1994, of which, only 1400 children can be followed through the 1999 wave of the ERHS and of these 1400, only 795 can be followed finally through the 2004 wave of the ERHS. We are able to follow only 43% of our original sample. This immediately raises concerns about selection related attrition bias. Panels A and B depict average enrollment and relative grade attainment stratified by age, gender and year for our full panel sample (N = 795). Whereas, panels C and D of Table 1, depict average enrollment and relative grade attainment stratified by age, year and gender for the available sample in each year (N = 1849 in 1994, N = 1400 in 1999 and N = 795 in 2004). A comparison of averages reported in panels A and C of Table 1, shows that average enrollments for all ages and years remain almost identical between the panel sample (panel A) and the available sample (panel C), except for older female, whose enrollments remain systematically higher in the panel sample. A similar pattern emerges while comparing average relative grade attainment reported in panels B and D of Table 1. Overall, it appears that children followed through all three waves have on average only marginally higher enrollment and relative grade attainment compared to children dropping out of the sample over time.

Table 1
Enrollment and relative grade attainment by age and gender.

Age in years in 1994	1994		1999		2004	
	Male	Female	Male	Female	Male	Female
Panel A: Enrollment — Final panel sample						
Child is 7–8 years	0.05	0.02	0.53	0.50	0.62	0.54
Child is 9–10 years	0.14	0.08	0.55	0.46	0.60	0.47
Child is 11–12 years	0.25	0.16	0.57	0.49	0.48	0.44
Child is 13–14 years	0.25	0.29	0.42	0.41	0.39	0.32
Panel B: Relative grade attainment — Final panel sample						
Child is 7–8 years	0.07	0.05	0.30	0.24	0.35	0.28
Child is 9–10 years	0.12	0.07	0.25	0.22	0.36	0.30
Child is 11–12 years	0.17	0.11	0.32	0.22	0.42	0.33
Child is 13–14 years	0.14	0.19	0.25	0.24	0.39	0.35
Sample size	465	330	465	330	465	330
Panel C: Enrollment — Available sample						
Child is 7–8 years	0.05	0.02	0.51	0.44	0.62	0.54
Child is 9–10 years	0.13	0.08	0.53	0.41	0.60	0.47
Child is 11–12 years	0.19	0.12	0.51	0.39	0.48	0.44
Child is 13–14 years	0.26	0.18	0.38	0.31	0.39	0.32
Panel D: Relative grade attainment — Available sample						
Child is 7–8 years	0.08	0.05	0.28	0.21	0.35	0.28
Child is 9–10 years	0.13	0.08	0.26	0.21	0.36	0.30
Child is 11–12 years	0.15	0.12	0.27	0.20	0.42	0.33
Child is 13–14 years	0.19	0.13	0.27	0.18	0.39	0.35
Sample size	938	911	734	666	465	330

Notes: Sample averages for enrollment and relative grade attainment are reported by age, gender and year. Panels A and B use N = 795 (the panel sample) for all three waves — 1994, 1999 and 2004. The sample averages for enrollment and relative grade attainment reported in panels C and D use data on 1849 individuals in 1994, and 1400 in 1999 (losing 449 children between 1994 and 1999), and 795 in 2004 (losing 605 children between 1999 and 2004).

[2] To our knowledge, previously only Behrman et al. (2005) have used experimental data from Mexico to assess the impact of a Conditional Cash Transfer program, *PROGRESA*, on schooling outcomes using 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 past period enrollments and current period enrollment status, but, does not account for socioeconomic factors or individual specific unobservables that affect a child's complete trajectory of current and future schooling outcomes.

[3] At the time of the 1994 survey, the smallest administrative unit in Ethiopia was called a 'peasant association', which is sometimes equivalent to one village or a cluster of villages. We use "villages" and "peasant association" interchangeably.

This would raise some concerns about positive selection, that is, children with higher baseline enrollment and relative grade attainment are more easily traceable compared to their less able counterparts. However, the lack of significance on the baseline outcome variables in the linear probability model of attrition presented and discussed later diminishes some of these concerns.

To understand the demographic composition of the children dropping out over time, Table A2 in the Appendix, depicts the distribution of the sample by age and gender. Three observations emerge from Table A2: (a) attrition is higher among older children, (b) attrition rates are higher among females and (c) attrition is higher among adolescent females capturing their exit from the household possibly related to marriage, which is common at these young ages in Ethiopia [Ezra and Kiros, 2001; Fafchamps and Quisumbing, 2005]. In 2004, the ERHS also collected detailed information on the reason why an individual left the household between the 1999 and 2004 waves. We use this data to categorize our missing respondents in 2004. The top four reported reasons for leaving the sample are — (a) marriage (45%), (b) look for work/job (17.2%), (c) study/go to school (10%) and (d) other (28.42%). Since, child level attrition is relatively high in our sample, we return to this issue later in the paper.

3. Empirical specification

Our objective is to determine the extent to which early disruptions in enrollment and progression affect schooling outcomes in later periods, that is, to determine how short-run decisions in enrollment and attainment affect the child's entire trajectory of future schooling outcomes. We can address this objective by either, specifying a dynamic schooling production function or, a dynamic conditional schooling demand function. While economists have more frequently estimated the dynamic schooling production function (Andrabi et al., 2011; Todd and Wolpin, 2007); Todd and Wolpin (2003) remain skeptical about the general benefits of estimating the dynamic (value-added) schooling production function as it relies on a number of strong assumptions, such as — (a) the schooling production function must not vary with age (or at least for the ages used to implement the model), (b) the coefficient estimates on the school inputs must be geometrically declining, (c) the rate of decline for all school inputs must be identical (for example: should not differ by age), (d) the impact of innate ability must be geometrically declining at the same rate as the school input effects, (e) the one-period lagged test score sufficiently captures the contribution of all previous period inputs and unobservables and (f) to obtain unbiased estimates, one has to assume that all the omitted schooling inputs are uncorrelated with the included inputs and the lagged test score.

In this paper, we estimate a dynamic conditional schooling demand function. This is less restrictive than the dynamic schooling production function specified in Todd and Wolpin (2003). First, the dynamic conditional schooling demand function replaces the endogenous schooling inputs with exogenous observables and accounts for the problem of missing school inputs and endogenous school inputs in an effective manner. Second, the dynamic conditional schooling demand function imposes no particular structure on the way in which schooling inputs must be related to schooling outcomes. Though it shares with the dynamic schooling production function that conditional upon one-period lagged schooling outcome and current schooling inputs, further lags of schooling inputs should have no impact on current schooling.

Following Strauss and Thomas (2008), we estimate a dynamic conditional schooling demand function:

$$S_{it} = \beta_0 + \beta_1 S_{it-1} + \sum_{j=2}^{R} \beta_j X_{ijt} + \sum_{j=1}^{S} \gamma_j Z_{ij} + \varepsilon_i + \varepsilon_h + \varepsilon_{vt} + \varepsilon_{it}. \tag{1}$$

In Eq. (1), schooling outcomes in the current period are regressed on lagged schooling outcome controlling for a full set of time-varying and time-invariant demographic and socioeconomic characteristics. The coefficient estimate on the lagged dependent variable, β_1, captures the extent of path dependence in schooling outcomes. A coefficient estimate of one indicates full path dependence, a coefficient estimate of zero indicates no path dependence and a coefficient estimate between zero and one captures partial path dependence. Path dependence implies that short-term shocks will have long-term impacts; no path dependence implies that short-term shocks have no long-term consequences on schooling outcomes.

S_{it} reflects two outcome variables of interest — enrollment status and relative grade attainment of child i at time t. Enrollment status is defined as a dummy variable which takes a value 1 if the child is enrolled in school at the time of the survey, zero otherwise.[4] Relative grade attainment is defined as actual grades divided by potential grades where potential grades is calculated as total number of grades accumulated had the individual completed one grade of schooling by age 7 and continued to accumulate an additional grade of schooling in each subsequent year. Table 2 provides descriptive statistics for these outcomes as well as the regressors used in the empirical specification.

The Xs and the Zs capture time-varying and time-invariant characteristics respectively. At the individual level, we control for age of the child, male dummy, mother's age and measure of parental schooling. The male dummy equals one if male, 0 if female capturing gender specific differences in schooling outcomes. We use lagged age in years, and this is further interacted with male dummy to capture age and age-gender specific differences in schooling outcomes.[5] The majority of parents in this region have no formal schooling and so, we characterize parental schooling using dummy variables, where the dummy variable takes a value one if the mother (father) has at least one grade of formal schooling, zero otherwise. Mother's age (in years) is included in the regressions to capture mother's experience and knowledge.

Household level regressors include number of adult (>18 years) males and number of adult (>18 years) females capturing household demographic composition. Age of the head of the household is included to capture household experience and life-cycle position. These demographic composition variables are specified in lags to avoid potential biases associated with treating household demographic composition as exogenous. Current period demographic composition may be correlated with household specific time-invariant unobservables that are correlated with current and lagged schooling outcomes.

Our final sample of children, belong to households that depend on land and agricultural output for survival. Thus, it would be important to control for household level shocks that specifically affect their agricultural output. To account for this, our dynamic specification also controls for household's exposure to a drought using a dummy variable. This shock variable is constructed using data from the 2004 wave of the ERHS. Households in 2004 were asked to respond to the following question *"In what years did these [drought, flood, erosion…] shocks occur?"* Households were also asked to list the three worst shock years, in descending order of severity. We use combination of these two questions to construct our measure of drought shock, which takes a value 1 if the household reported to suffer from drought shock in the year t and zero otherwise.[6]

Both formal and informal credit markets are poorly developed in rural Ethiopia. To capture such borrowing constraints faced by the household, we include a lagged measure of log of household's real

[4] Some children are enrolled in religious schools. Our interest is limited to measuring human capital accumulated through learning subjects like mathematics, science and social science; none of which is taught in religious schools. For this reason, we treat children enrolled in religious schools as not enrolled.

[5] We have examined non-linearity between enrollment and lagged age by specifying a spline variable with an age cut-off at 15 years; the chi-squared test does not reject the null of linearity between enrollment and lagged age. For consistency, we control for lagged age in years in levels in the relative grade attainment equation.

[6] While droughts are covariate shocks, as Dercon, Hoddinott and Woldehanna (2005, Table 2) note, they do not necessarily affect all households within a village, nor are there effects uniform across households (Dercon et al., 2005, Table 3).

Table 2
Descriptive statistics.

	Mean (Std. dev)
Completed grades of schooling	2.28 (2.80)
Enrollment, Enrollment = 1 if currently enrolled in school and 0 otherwise	0.38 (0.48)
Relative grade attainment (actual grade/potential grade given age)	0.24 (0.28)
Household size	7.66 (2.62)
Log real per capita household consumption expenditure (PCE)	3.96 (0.76)
Drought Shock	0.05 (0.21)
Mother's schooling	0.08 (0.27)
Father's schooling	0.23 (0.42)
Male dummy	0.58 (0.49)
Age (years)	15.11 (4.55)
No. of adult males	1.79 (1.12)
No. of adult females	1.79 (1.01)
Mother's age	42.41 (10.06)
Age of the head of the household	51.34 (12.01)

Notes: N = 795*3 = 2385

per capita consumption expenditure as an additional explanatory variable.[7]

There are four unobservables in Eq. (1), ε_i, ε_h, ε_{vt} and ε_{it}; ε_i captures individual specific time-invariant unobservables such as child's innate ability to perform well in school; ε_{vt} captures village specific time-varying unobservables such as rainfall shocks and prices of schooling inputs and home inputs; ε_h captures household specific time-invariant unobservables such as parental preferences towards schooling and their time preferences; and ε_{it} is the random time-varying unobservable that is unknown to both the individual and the econometrician at date t. Note that an OLS estimate of β_1 is likely to be biased and inconsistent due to the presence of time-invariant unobservables such as child's innate ability to perform well in school, parental preferences towards schooling and community's political connections; all of which are likely to be correlated with the lagged schooling outcome, S_{it-1} [Blundell and Bond (1998); Deaton (1997); Wooldridge (2002)].

4. Results

4.1. Dynamic regression results

Results of the dynamic enrollment and relative grade attainment regressions are reported in Table 3. In addition to estimating an OLS version of Eq. (1), we also estimate a first-differenced version of Eq. (1) as specified below:

$$\Delta S_{it} = \beta_1 \Delta S_{it-1} + \sum_{j=2}^{R} \beta_j \Delta X_{ijt} + \Delta\varepsilon_{it}. \tag{2}$$

In all specifications, we include village by survey round dummy variables. This controls for all time varying shocks and changes in prices and environmental factors, both negative (drought) and positive (improvements in infrastructure), at the village level. In our first-difference specifications, this also allows for village specific time trends. By including village by survey round dummies we remove all sources of time-invariant and time-varying unobservables common at the village level. Inference may also be affected by the presence of time-varying shocks that are common within smaller geographic clusters of a village. We use unique codes that classify households in a village into smaller geographic clusters defined as "neighborhoods". The standard errors reported here are additionally clustered at the neighborhood level. Any remaining source of heteroskedasticity is addressed using White's robust standard errors.

Eq. (2) is estimated using an Arellano and Bond (1991) estimator where first-differenced lagged schooling enrollment (relative grade attainment) is instrumented using twice-lagged schooling enrollment (relative grade attainment), two-period lagged dummy for drought shock, and two-period lagged dummy for drought shock interacted with the age of the head of the household two-periods ago. Note

Table 3
Determinants of enrollment and relative grade attainment.

Covariates	Enrollment		Relative grade Attainment	
	(1) OLS	(2) Arellano–Bond	(3) OLS	(4) Arellano–Bond
Lagged enrollment	0.26*** (0.02)	0.33*** (0.09)		
Lagged relative grade attainment			0.42*** (0.02)	0.24*** (0.07)
Lagged log real per capita consumption expenditure	0.008 (0.015)	−0.033 (0.02)	0.017** (0.008)	0.007 (0.008)
Drought shock	−0.020 (0.052)	−0.031 (0.06)	−0.009 (0.015)	0.0005 (0.01)
Male dummy	0.072 (0.10)		0.015 (0.03)	
Lag age in years	−0.028*** (0.007)	−0.35 (0.48)	−0.003 (0.003)	−0.065 (0.10)
Lag age in years × male dummy	−0.0005 (0.007)	−0.0037 (0.008)	0.003 (0.002)	0.003 (0.002)
Mother's schooling	0.10*** (0.03)		0.033** (0.017)	
Father's schooling	0.07** (0.03)		0.030** (0.014)	
Number of adult males, lagged	0.0019 (0.013)	−0.024 (0.02)	0.013*** (0.005)	0.015*** (0.006)
Number of adult females, lagged	0.029** (0.013)	0.015 (0.02)	0.009 (0.006)	0.017*** (0.007)
Mother's age, lagged	−0.0023* (0.001)	−0.0016 (0.0019)	−0.0008 (0.0006)	−0.0005 (0.0009)
Age of household head, lagged	−0.0017 (0.0012)	−0.0045 (0.002)	−0.0007 (0.0005)	0.0006 (0.0009)
Village × survey round dummy variables included	Yes	Yes	Yes	Yes
Kleibergen–Paap F statistic		78.73***		127.26***
Hansen J statistic		2.50 (0.28)		0.041 (0.97)
C statistic		1.42 (0.99)		0.028 (0.86)
Hausman test statistic		0.59 (1.13)		−0.045 (0.26)

Notes: Robust standard errors adjusted for clustering at the neighborhood level in parentheses. Sample size is 1590 (column 1 and 3) and 795 (columns 2 and 4). OLS estimates are reported in columns (1) and (3). In column (2), first-differenced lagged enrollment is instrumented with two-period lagged dummy for drought shock, two-period lagged dummy for drought shock interacted with the age of the head of the household two-periods ago, and two-period lagged enrollment. In column (4), first-differenced lagged relative grade attainment is instrumented with two-period lagged dummy for drought shock, two-period lagged dummy for drought shock interacted with the age of the head of the household two-periods ago, and two-period lagged relative grade attainment. For the Hansen J statistic and the C statistic, p-values are reported in the parentheses.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

[7] We also find that our results are robust to excluding the lagged per capita consumption expenditure variable from the regressions.

that the consistency of our estimates hinges upon the assumptions of zero first- and second-order serial correlation in the levels of error terms and no measurement error in the school enrollment and relative grade attainment variables. We assess the validity of these assumptions below.[8]

The OLS estimate of β_1 obtained from the dynamic enrollment regression is 0.26 (Table 3, column 1) and is significant at the 1% level indicating a positive association between lagged enrollment and current enrollment. The OLS estimate of β_1 obtained in the dynamic relative grade attainment regression is 0.42 (Table 3, column 3). Columns 2 and 4 in Table 3 report the results of estimating Eq. (2) using the Arellano–Bond estimator. Column (2) shows that if a child was enrolled in the previous period, (s)he is 33 percentage points more likely to be enrolled today relative to a child not previously enrolled,[9] a considerable degree of path dependence given that five years elapse between these survey rounds. We also find path dependence in relative grade attainment. The Arellano–Bond estimate on relative grade attainment is reported in column 4, Table 3 shows that approximately 25% of grade progression depend upon relative grades accumulated five years ago. We are able to reject the null of "no path dependence" at the 1% significance level for both enrollment and relative grade attainment, suggesting that short-run factors have long-term effects on schooling in rural Ethiopia.

4.2. Disaggregations

The degree of path dependence in these schooling outcomes may differ by economic or demographic group. We explore such differences here using the Arellano–Bond estimator. Table 4 shows that boys and girls have very different degrees of path dependence in school enrollment, with path dependence being much stronger for girls than boys (0.60 compared to 0.23, significant at 1%). We also consider whether differences exist based on initial income. We construct a lowess plot based on a locally weighted regression of enrollment on initial log of real per capita consumption expenditure (pce) and find a large change in the slope for households with log expenditure greater than four. We use four as our cutoff for log pce and find that children from households with higher baseline per capita expenditures have a much higher degree of path dependence than children from poorer households (0.70 compared to 0.02, with the former significant at the 1% level and the latter not even significant at the 10% level).

We offer some speculations as to why these differences exist. The higher degree of path dependence observed among girls may reflect the possibility that girls are withdrawn from school due to permanent changes in life such as marriage. By contrast, boys may be withdrawn following a shock experienced by the household, but are returned to school subsequently. Another possible explanation is sample attrition, if more able girls are likely to be followed (less likely to marry at a young age) this could result in higher path dependence among girls.

Why might we observe differences in path dependence across children in different income classes? In the case of school enrollment, path dependence implies that if a child is withdrawn from school in the previous period, she is less likely to be enrolled in the current period. It also means that if she is enrolled in the last period, she is more likely to be enrolled in the current period. We hypothesize that in the case of poorer households, shocks experienced between periods cause children's enrollment to fluctuate, entering and exiting repeatedly. By contrast, in less poor households, these negative shocks do not generate such short-term fluctuations, and this is reflected in our results by greater path dependence over longer periods of time. One piece of evidence that captures this idea is the covariance of drop out across children in the same household. For households with two or more children, we compute conditional probabilities. We find that the probability that a child will drop out of school between two consecutive periods conditional upon one or more children dropping out of the household is 15% among poor households and 9% among less-poor households. This is consistent with the findings on path dependence. Shocks experienced by poor households cause all children to cycle in and out of school thereby reducing path dependence with enrollment five years ago. By contrast, path dependence for children in less poor households is affected more by circumstances specific to the child.

These results for current enrollments are replicated for our measure of relative grade attainment, as shown in Table 4. The percentage point differences are not quite as large, but they still exist and are in the same direction as enrollments.

Table 4
Determinants of enrollment and relative grade attainment by selected disaggregations.

Disaggregation	Enrollment		Relative grade attainment	
	Parameter estimate (standard error)	Sample size	Parameter estimate (standard error)	Sample size
Boys	0.23*** (0.09)	465	0.17*** (0.05)	465
Girls	0.60*** (0.17)	330	0.35*** (0.13)	330
Poor households	0.02 (0.10)	519	0.23*** (0.08)	519
Less poor households	0.70*** (0.20)	276	0.24** (0.12)	276

Notes: Robust standard errors adjusted for clustering at the neighborhood level in parentheses. The coefficient estimates on the lagged dependent variable are reported from following the Arellano–Bond estimation strategy. The full set of RHS variables that appear in Table 3 are also controlled in these specifications. Lowess plot was used between enrollment and two-period lagged PCE to determine the cut-off point at which the sample should be stratified.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

4.3. Robustness

It is well known that if the correlation between the endogenous regressor and the instruments is weak, IV estimates remain inconsistent [Murray (2006)]. We use the Kleibergen–Paap Wald rk F statistic to test for weak instruments. This test statistic is robust in the presence of heteroskedasticity, autocorrelation and clustering [Kleibergen and Paap, 2006]. The Kleibergen–Paap Wald rk F statistic on the excluded instruments reported in our Arellano–Bond estimates is always above 10, satisfying the Staiger and Stock (2003) rule of thumb rejecting the null of weak correlation between the instruments and the endogenous regressor.[10]

Arellano and Bond (1991) stress that in their estimator, using a twice lagged dependent variable (here S_{it-2}) as an instrument for first-differenced lagged dependent variable (ΔS_{it-1}) is valid only if $E(\Delta\varepsilon_{it}, S_{it-2})=0$, that is, the errors in the levels specification are serially uncorrelated over time. To test for second-order serial correlation in the levels residuals, Arellano and Bond (1991, pp. 282) suggest using an m2 statistic. However, this requires a minimum of five rounds of data and the ERHS has only four rounds of schooling data (1994, 1997, 1999 and 2004 but not 1995). Instead, we use,

[8] We choose the Arellano–Bond estimator for the following reasons: (1) Our FD-GMM estimates that do not rely on the no-serial correlation assumption in the level residuals have low first-stage F statistic on the excluded instruments, which raises concerns about inconsistent estimates; and (2) while the FD-GMM estimator relies on weaker assumptions, in our data we do not find that we can reject the null that the Arellano–Bond and FD-GMM estimators are equal, see robustness section for discussion.

[9] We also estimate our preferred specification reported in columns 2 and 4 of Table 3 also treating the first-differenced (FD) lagged PCE as endogenous using two-period lagged PCE as additional IVs. We test for the exogeneity of the FD-lagged PCE in these specifications and find that at a 10% significance level we cannot reject the null of exogeneity of the FD-lagged PCE.

[10] The Staiger and Stock (2003) rule of thumb is approximately a 5% significance test that the worst relative (IV to OLS) bias would be 10% or less [see Table 1, p 39 (Staiger and Stock, 2003)].

Table 5
Determinants of enrollment and relative grade attainment using an alternative estimator.

	Enrollment	Relative grade attainment
Covariates	(1) FD-GMM	(2) FD-GMM
Lagged enrollment	0.91(1.13)	
Lagged relative grade attainment		0.19(0.27)
Village × survey round dummy variables included	Yes	Yes
Kleibergen–Paap F statistic	1.24	3.63
Hansen J statistic (P-value)	1.07(0.30)	0.013(0.90)

Notes: Robust standard errors adjusted for clustering at the neighborhood level in parentheses. Sample size is 795 (columns 1 and 2). In columns (1) and (2), both first-differenced lagged enrollment and first-differenced lagged relative grade attainment are instrumented with two-period lagged dummy for drought shock and two-period lagged dummy for drought shock interacted with the age of the head of the household two-periods ago. The full set of RHS variables that appear in Table 3 are also controlled in these specifications. For the Hansen J statistic and the C statistic, p-values are reported in parentheses.

* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

separately, the Hausman specification test and the C statistic also known as a "GMM distance" or "difference-in-Sargan" statistic to test for the no serial correlation assumption in the levels specification (Blundell and Bond, 2000 and Hayashi, 2000).

To construct the Hausman test see Hausman (1978), we need to estimate our preferred models using a variant of the FD-GMM estimator, where first-differenced lagged enrollment is instrumented using a two-period lagged dummy variable for drought shocks and a two-period lagged dummy for drought shock interacted with the age of the head of the household two-periods ago. These estimates do not rely on the assumption of zero first-order and second-order serial correlation in the levels residuals. The results are reported in Table 5, column 1. The coefficient estimate on lagged enrollment is 0.91. The estimated difference on the lagged enrollment coefficient between Table 5, column 1 and Table 3, column 2 is 0.59 (standard error 1.13), not rejecting the null of zero first-order and second-order serial correlation in the levels error terms. A Hausman specification test is also employed comparing the results from the Arellano–Bond type estimator for relative grade attainment reported in column 4, Table 3 to the first-difference GMM estimator reported in column 2, Table 5. The estimated difference on the lagged relative grade attainment is −0.045 (standard error 0.26), also not rejecting the null of zero first-order and second-order serial correlation in the levels error term.

The C-statistic tests of the serial correlation/exogeneity of the two-period lagged enrollment and two-period lagged relative grade attainment variables are reported in columns 2 and 4 of Table 3. The results are consistent with the Hausman test results. At the 10% significance level, we do not reject the null that the two-period lagged enrollment/relative grade attainment is a valid instrument, that is, we cannot reject the null of no first-order and second-order serial correlation in the errors in levels specification. Note that the C-statistic only requires that conditional on first-differencing, the two-period lagged drought shocks and the interaction term between two-period lagged drought shock and age of the head of the household two-periods ago are uncorrelated with the first-differenced error term.

Our test of no serial correlation in the level equation error terms also serves as a test for further back lags of the dependent variable. If Y_{it-2} is on the right hand side of the levels school enrollment equation, and we do not include it as a regressor, then it will be in the equation error term. Y_{it-3} will be in the equation error, lagged one period, creating a correlation between the period t and period t − 1 error terms (since Y_{it-3} affects Y_{it-2} even if only one period lagged dependent variable is included in the true specification). This would result in serial correlation among the error terms in the levels specification with only one period lagged school enrollment, which we fail to reject, as discussed above. The same reasoning applies to relative grade attainment.

Attrition in our panel has important implications for the consistency of our parameter estimates. To see the issues at stake, we write an attrition equation as follows:

$$A_{it} = \gamma_0 + \gamma_1 S_{it-2} + \sum_{j=2}^{R} \gamma_j X_{ijt} + \sum_{j=1}^{S} \alpha_j Z_{ij} + \mu_i + \mu_h + \mu_{vt} + \mu_{it} \quad (3)$$

where A_{it} takes a value 1 if the individual can be followed over time and 0 otherwise. An estimation strategy that relies on first differencing helps us in that it removes the influence of time-invariant unobservables that affect both attrition and our outcome variables.

However, there are myriad sources of time-varying unobservables such as the amount of information the household has on the quality of schooling in the migrating city and the probability of finding a job upon migration, both of which can have a direct impact on both current schooling and the attrition process leaving an important source of attrition bias unaddressed. Since we are unable to remove time-varying unobservables (except the one's that vary at the village level) from our regressions, conditional upon first-differencing, we have to assume that the time-varying unobservables in Eq. (2) are independent of the time-varying

Table 6
Determinants of enrollment and relative grade attainment for the shorter panel.

	Enrollment		Relative grade attainment	
Covariates	(1) OLS	(2) Arellano–Bond	(3) OLS Relative grade attainment	(4) Arellano–Bond Relative grade attainment
Lagged enrollment	0.38*** (0.02)	0.47*** (0.10)		
Lagged relative grade attainment			0.63*** (0.02)	0.21*** (0.05)
Village × survey round dummy variables included	Yes	Yes	Yes	Yes
Kleibergen–Paap F statistic		49.53		90.55
Hansen J statistic		1.40 (0.49)		1.27 (0.52)
C statistic		0.59 (0.44)		1.25 (0.26)

Notes: Robust standard errors adjusted for clustering at the neighborhood level in parentheses. Sample size is 2540 (columns 1 and 3) and 1270 (columns 2, and 4). In columns (2) first-differenced lagged enrollment is instrumented with two-period lagged dummy for drought shock, two-period lagged dummy for drought shock interacted with the age of the head of the household two-periods ago, and two-period lagged enrollment. In column (4), first-differenced lagged relative grade attainment is instrumented with two-period lagged dummy for drought shock, two-period lagged dummy for drought shock interacted with the age of the head of the household two-periods ago, and two-period lagged relative grade attainment. The full set of RHS variables that appear in Table 3 are also controlled in these specifications. For the Hansen J statistic and the C statistic, p-values are reported in parentheses.

* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

unobservables in Eq. (3), that is, E ($\Delta\varepsilon_{it}$, μ_{it}) = 0. We recognize that this is a strong assumption.

That said, to partially allay concerns about this potential source of bias, we note that our coefficient estimate on the lagged dependent variable will not suffer from attrition bias as long as any information realized between periods t and t − 2 that affects the attrition probability is independent of $\Delta\varepsilon_{it}$. S_{it-2} and any further lags can be included in the attrition equation as long as the errors in the levels specification are serially uncorrelated over time. However, the one-period lagged schooling outcome, S_{it-1} must not belong to the attrition equation, that is, Cov (S_{it-1}, A_{it}) = 0, since, by construction Cov ($\Delta\varepsilon_{it}$, S_{it-1}) is not equal to zero [see page 585, Wooldridge, 2002].

Second, we estimated a linear probability model of sample attrition, where attrition is regressed upon baseline characteristics observed in 1994. The attrition regression results outlined in column 1, Table A3 indicate that attrition is unrelated to the endogenous observable, enrollment status. A similar result emerges for relative grade attainment ruling out some potential sources of selection on observables including but not limited to selection based on baseline schooling, gender, age and interactions between baseline schooling with age and gender.

Finally, we re-estimate our preferred specification using a shorter panel where attrition is less pernicious. Specifically, we restrict attention to primary school age children in 1994, observed subsequently in the 1997 and 1999 waves of the ERHS. This has less attrition; we can trace 68% (1270/1849) of the primary school age children in 1994 through the 1999 round. Using this sample with higher re-contact rates, we obtain a coefficient estimate of 0.47 and 0.21 on lagged enrollment and lagged relative grade attainment respectively (see columns 2 and 4 of Table 6). The coefficient estimates on the lagged dependent variables suggest path dependence in both enrollment and grade progression, consistent with the findings reported for the larger panel described in the Results section. The coefficient estimates for lagged enrollment and relative grade attainment are similar between the longer (1994, 1999 and 2004) and shorter panel (1994, 1997 and 1999); this mitigates some but does not eliminate all concerns regarding attrition related selection concerns in the longer panel.

5. Discussion and conclusion

Is current schooling path dependent? This paper attempts to answer this question in the context of a poor developing country. We estimate a dynamic conditional schooling demand function where the coefficient estimate on the lagged dependent variable captures the impact of all previous periods' schooling inputs and resources. The dynamic specification is estimated using longitudinal data on primary school children in rural Ethiopia between 7 and 14 years in 1994 followed through 1999 and 2004. Our estimation strategy addresses concerns regarding omitted variable bias.

We find that the history of schooling inputs and resources, as captured by the lagged dependent variables for enrollment and relative grade attainment, have a strong impact on individual's later schooling outcomes. A child who is enrolled in the last period is 33 percentage points more likely to be enrolled today compared to his counterpart who was not enrolled five years ago. We obtain similar findings using relative grade attainment; grade progression today is affected by grade progression in the past. These impacts are larger for girls and for children from less poor households.

We note that the validity of our results rests in part on our ability to satisfy a number of econometric concerns relating to instrument validity, the absence of first-order and second order serial correlation in the levels and non-random sample attrition. While we provide test statistics that assuage a number of these concerns, particularly given the high level of attrition in these data, some caution is warranted when using these findings. There are intriguing differences when we disaggregate by sex and household consumption level. We have offered some speculations regarding these; further work on these forms part of an agenda for future research.

Mindful of these caveats, this paper provides evidence that, like child health outcomes, schooling outcomes observed at a given point in time are a function of past history. “History matters”.

Acknowledgments

We thank the following organizations for funding the survey work on which this paper is based: SIDA, USAID and the World Food Programme. We thank John Ham, Jeffrey B Nugent, Utteeyo Dasgupta and seminar participants at the PAA meetings for comments and suggestions. We are also grateful to the editor and the two anonymous referees for excellent comments and suggestions. We are thankful to Yisehac Yohannes for clarifying several data related questions. Financial support was provided by the College of Letters, Arts and Sciences, University of Southern California and the International Food Policy Research Institute.

Appendix A

Table A1
First-stage regressions for estimates reported in columns 2 and 4 of Table 3.

Covariates	(1) Column 2, Table 3	(2) Column 4, Table 3
Two-period lagged enrollment	−0.72***	
	(0.04)	
Two-period lagged relative grade attainment		−0.48***
		(0.02)
Two-period lagged drought shock	0.26	0.45**
	(0.79)	(0.20)
Two-period lagged drought shock*two-period lagged age of the head of the household	−0.007	−0.008**
	(0.015)	(0.004)
Lagged log real pce (in first-differences)	0.035*	−0.006
	(0.02)	(0.009)
Drought shock (in first-differences)	−0.003	0.010
	(0.04)	(0.019)
Lag age in years (in first-differences)	−0.011	0.14
	(0.29)	(0.16)
Lag age in years × male dummy (in first-differences)	0.010	0.008***
	(0.007)	(0.003)
No. of adult males, lagged (in first-differences)	0.008	−0.009
	(0.017)	(0.008)
No. of adult females, lagged (in first-differences)	0.018	−0.008
	(0.019)	(0.008)
Mother's age, lagged (in first-differences)	0.0006	0.0003
	(0.002)	(0.001)
Age of the head of the household, lagged (in first-differences)	0.006***	0.0008
	(0.002)	(0.001)
Village × survey round dummy variables included	Yes	Yes

Notes: Robust standard errors adjusted for clustering at the neighborhood level in parentheses. Sample size is 795 (columns 1 and 2).
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table A2
Distribution of the observed sample by age and gender.

Age in years in 1994	1994		1999		2004	
	Male	Female	Male	Female	Male	Female
Child is 7–8 years	239	252	195	206	149	130
Child is 9–10 years	242	256	195	192	129	108
Child is 11–12 years	250	212	184	151	114	61
Child is 13–14 years	207	191	160	117	73	31
Sample size	938	911	734	666	465	330

Notes: We have data on 1849 children in 1994, of which only 1400 can be followed through the 1999 wave of the ERHS. Finally of the 1400 available in 1999, only 795 can be followed through the 2004 wave of the ERHS.

Table A3
Determinants of sample attrition for enrollment and relative grade attainment.

Covariates	(1) Attrition	(2) Attrition
Enrollment	**0.0015**	
	(0.009)	
Child is 9–10 years (dummy = 1)	0.014	0.012
	(0.01)	(0.01)
Child is 11–12 years (dummy = 1)	−0.020	−0.019
	(0.02)	(0.02)
Child is 13–14 years (dummy = 1)	0.002	0.003
	(0.015)	(0.015)
Male dummy	0.011	0.003
	(0.015)	(0.013)
Child is 9–10 years * male dummy	−0.006	0.005
	(0.02)	(0.017)
Child is 11–12 years * male dummy	−0.02	−0.011
	(0.02)	(0.019)
Child is 13–14 years * male dummy	0.017	0.022
	(0.03)	(0.02)
Enrollment * child is 9–10 years	−0.03	
	(0.03)	
Enrollment * child is 11–12 years	−0.03	
	(0.05)	
Enrollment * child is 13–14 years	−0.007	
	(0.06)	
Enrollment * child is 9–10 years * male dummy	0.0004	
	(0.02)	
Enrollment * child is 11–12 years * male dummy	0.02	
	(0.06)	
Enrollment * child is 13–14 years * male dummy	−0.03	
	(0.05)	
Relative grade attainment		**−0.03**
		(0.02)
Relative grade attainment * child is 9–10 years		0.005
		(0.03)
Relative grade attainment * child is 11–12 years		−0.03
		(0.06)
Relative grade attainment * child is 13–14 years		−0.002
		(0.05)
Relative grade attainment * child is 9–10 years * male dummy		0.016
		(0.02)
Relative grade attainment * child is 11–12 years * male dummy		0.058
		(0.05)
Relative grade attainment * child is 13–14 years * male dummy		0.02
		(0.05)
Log of real per capita consumption	0.02***	0.02***
	(0.007)	(0.008)
Drought shock	0.05	0.05
	(0.03)	(0.03)
Mother's schooling	0.018	0.017*
	(0.01)*	(0.01)
Father's schooling	0.010	0.010
	(0.010)	(0.01)
Male dummy	0.11	0.005
	(0.015)	(0.004)
No. of adult males	0.005	0.005
	(0.004)	(0.004)
Mother's age	0.00015	0.00015
	(0.0002)	(0.0002)
No. of adult females	0.001	0.0015
	(0.002)	(0.002)
Age of the head of the household	0.0004*	0.0004*
	(0.0002)	(0.0002)
Village fixed-effects included	Yes	Yes

Notes: Robust standard errors adjusted for clustering at the neighborhood level in parentheses. Sample size is 1849 (columns 1 and 2). Attrition takes the value 1 if the individual was followed during the subsequent waves and 0 otherwise.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Appendix B. Supplementary data

Supplementary data to this article can be found online at doi:10.1016/j.jdeveco.2012.03.002.

References

Alderman, H., Hoddinott, J., Kinsey, B., 2006. Long-term consequences of early childhood malnutrition. Oxford Economic Papers 58 (3), 450–474.
Andrabi, T., Das, J., Khwaja, A.I., Zajonc, T., 2011. Do value-added estimates add value? Accounting for learning dynamics. American Economic Journal: Applied Economics 3 (3), 29–54.
Arellano, M., Bond, S.R., 1991. Some tests of specification for panel data: Monte Carlo evidence and an application of employment equations. Review of Economic Studies 58 (3), 277–297.
Barker, D., 1994. Mothers, Babies and Health in Later Life. BMJ Publishing Group, London.
Behrman, J.R., Sengupta, P., Todd, P., 2005. Progressing through PROGRESSA: an impact assessment of a school subsidy experiment in Rural Mexico. Economic Development and Cultural Change 54 (1), 237–275.
Blundell, R., Bond, S., 1998. Initial conditions and moment restrictions in dynamic panel data models. Journal of Econometrics 87 (1), 115–143.
Blundell, R., Bond, S., 2000. GMM Estimation with persistent panel data: an application to production functions. Econometric Reviews, Taylor and Francis Journals 19 (3), 321–340.
Boardman, A.E., Murnane, R.J., 1979. Using panel data to improve estimates of the determinants of educational achievement. Sociology of Education 52 (2), 113–121.
Deaton, A., 1997. The Analysis of Household Surveys: A Microeconometric Approach in Development Policy. Johns Hopkins University Press, Baltimore.
Dercon, S., 2005. Risk, poverty and vulnerability in Africa. Journal of African Economies 14 (4), 483–488.
Dercon, S., Hoddinott, J., 2004. The Ethiopian Rural Household Survey: Introduction. Mimeo, Washington D.C: International Food Policy Research Institute.
Dercon, S., Hoddinott, J., Woldehanna, T., 2005. Shocks and consumption in 15 Ethiopian villages. Journal of African Economies 14 (4), 559–585.
Dercon, S., Hoddinott, J., Woldehanna, T., 2006. Growth, Poverty and Chronic Poverty in Rural Ethiopia: Evidence from 15 Communities 1994–2004. Background Paper for The Chronic Poverty Report 2008–09. Chronic Poverty Research Centre (CPRC), Manchester, UK.
Ezra, M., Kiros, G.E., 2001. Rural out-migration in the drought prone areas of Ethiopia: a multilevel analysis. International Migration Review 35 (3), 749–771.
Fafchamps, M., Quisumbing, A., 2005. Marriage, bequest and assortive matching in rural Ethiopia. Economic Development and Cultural Change 53 (2), 347–380.
Hanushek, E.A., 1979. Conceptual and empirical issues in the estimation of educational production functions. The Journal of Human Resources 14 (3), 351–388.
Hausman, J.A., 1978. Specification tests in econometrics. Econometrica 46 (6), 1251–1271.
Hayashi, F., 2000. Econometrics, 1st ed. Princeton University Press, Princeton, NJ.
Hoddinott, J., Kinsey, B., 2001. Child growth in the time of drought. Oxford Bulletin of Economics and Statistics 63 (4), 409–436.
Jacob, B.A., Lefgren, L., Sims, D.P., 2010. The persistence of teacher-induced learning. Journal of Human Resources 45 (4), 915–943.
Kane, T.J., Staiger, D.O., 2008. Estimating teacher impacts on student achievement: an experimental evaluation. National Bureau of Economic Research Working Paper 14607.
Kleibergen, F., Paap, R., 2006. Generalized reduced rank tests using the singular value decomposition. Journal of Econometrics 133 (1), 97–126.
Maccini, S., Yang, D., 2009. Under the weather: health, schooling, and economic consequences of early-life rainfall. American Economic Review 99 (3), 1006–1026.
Mani, S., 2011. Is there complete, partial, or no recovery from childhood malnutrition? - Empirical evidence from Indonesia. Oxford Bulletin of Economics and Statistics. doi:10.1111/j.1468-0084.2011.00670.x.
Murray, M.P., 2006. Avoiding invalid instruments and coping with weak instruments. The Journal of Economic Perspectives 20 (4), 111–132.
Orazem, P.F., King, E.M., 2008. Schooling in Developing Countries: The Roles of Supply, Demand and Government Policy. In: Schultz, T.P., Strauss, J. (Eds.), Handbook of Development Economics, vol. 4. North Holland Press, Amsterdam.
Rothstein, J., 2010. Teacher quality in educational production: tracking, decay, and student achievement. Quarterly Journal of Economics 125 (1), 175–214.
Staiger, D., Stock, J.H., 2003. Testing for Weak Instruments in Linear IV Regression, Working Paper Series. Harvard University.
Strauss, J., Thomas, D., 2008. Health Over the Life Course. In: Schultz, T.P., Strauss, J. (Eds.), Handbook of Development Economics, vol. 4. North Holland Press, Amsterdam.
Todd, P.E., Wolpin, K.I., 2003. On the specification and estimation of the production function for cognitive achievement. Economic Journal 113, F3–F33.
Todd, P.E., Wolpin, K.I., 2007. The production of cognitive achievement in children: home, school, and racial test score gaps. Journal of Human Capital 1 (1), 91–136.
Wooldridge, J., 2002. Econometric Analysis of Cross Section and Panel Data. MIT Press, Cambridge.