Cognitive Achievement Production in Madagascar: A Value-Added Model Approach

Frédéric Aubery[†]

David E. Sahn[‡]

July, 2019

Abstract

Recent evidence from developing countries has raised concerns about the capacity of education systems to translate schooling into cognitive skills. The aim of this paper is to measure the contribution of an additional year of schooling on skills, as measured by mathematics and French test scores, for a cohort of young adults in Madagascar as they progress from adolescence to adulthood. The panel dimension of our data set allows for the estimation of a value-added model of learning achievement, in which the introduction of early test scores in the production function aims to reduce the potential for omitted variable bias. Additionally, we address the challenge of the endogeneity of additional schooling between survey rounds by using an instrumental variable approach. We demonstrate that cognitive skills continue to increase markedly among adolescents who remain in school. The value-added of an additional year of schooling at adolescence ranges from 0.15 to 0.26 standard deviations, depending of the estimation method used. Furthermore, our results provide evidence that adolescence is the period when the gap in skills widens as students with higher cognitive skills in early adolescence complete more grades and accumulate more skills in their transition to adulthood.[§]

Keywords: Education, cognitive achievement, value-added models, Madagascar

JEL codes: I21, O15, J24

[†] World Bank. Corresponding author. Email: faubery@worldbank.org

[‡] Cornell University, IZA.

[§] This document is an output from a project funded by the UK Department for International Development (DFID) and the Institute for the Study of Labor (IZA) for the benefit of developing countries. The views expressed are not necessarily those of DFID or IZA.

1. Introduction

The rise in enrollment rates experienced by most developing countries over the previous decade has shifted the policy debate from the quantity of education to issues of quality, in terms of the objective of improving human capital outcomes. The limited evidence from developing countries suggests that the gain in cognitive ability from schooling is quite limited (Pritchett 2013; Jones et al. 2014; Pritchett and Sandefur 2017), and the learning levels in school are far lower than OECD countries, according to international comparisons (Sandefur 2018). The *World Development Report 2018* (World Bank 2018) refers to a "learning crisis" in schools and stresses the need for research to more systematically measure learning outcomes. The viewpoint that schools are ineffective in imparting skills and knowledge has led some to argue this assertion as an explanation for low economic returns to education observed in sub-Saharan Africa (Lassibille and Tan 2005; Bigsten et al. 2000; Söderbom et al. 2006; Kuepie et al. 2009). Thus, more attention needs to be given to the role of cognitive skills, rather than educational attainment per se, especially in terms of determining labor market outcomes and economic development (Boissiere et al. 1985; Hanushek and Woessmann 2008). Indeed, the number of years of schooling is an imperfect measure of human capital that only reflects the quantity of schooling, neglecting how school quality and family background affect learning and cognition.

It is the issue of the extent to which schooling translates into effective human capital, measured by cognitive achievement, which motivates our paper. Specifically, we focus on measuring the gain in knowledge of an additional year of schooling in the low-income island nation of Madagascar. Estimating the complex process of human capital accumulation requires a full history of parental and school inputs, as well as detailed information on an individual's characteristics (Todd and Wolpin 2003, 2007). Some of the factors that influence cognitive outcomes of children, like parents' preferences or individual innate abilities, remain unobservable to the researcher. Others, like the timing of schooling or attendance, result from choices made by parents or the student herself, based on the perception of a child's potential, opportunities in the labor market, as well as the quality and cost of available schools. Under the reasonable assumption that unobserved heterogeneity is correlated with the use and quality of specific

inputs (in the case of this study, an additional year of schooling), a simple ordinary least squares (OLS) model of test score outcomes would not produce consistent estimates of covariates and, in particular, the determination of the impact of years of schooling on cognitive outcomes. The demands in terms of data required to deal with this heterogeneity remain an important obstacle to the estimation of a cognitive skills production function, and thus, much of the literature on the determinants of human capital is dedicated to identifying ways to avoid bias and draw causal inference.

One solution to this problem, which has been proposed in the literature, is to exploit exogenous variation in schooling in quasi-experimental designs. In high-income country settings, a handful of studies have provided evidence that schooling translates into cognitive skills. Banks and Mazzonna (2012), for instance, exploited a reform of the compulsory school age in the late 1940s in England to investigate the long-term effect of an additional year of schooling on the production of human capital and demonstrated that schooling has long-lasting effects on the stock of cognitive skills observed at older age. Similarly, a couple of recent studies have exploited exogenous variation in instructional time, due to a random assignment of a test date (Carlsson et al. 2015) or weather-related school closings (Hansen 2011), to demonstrate that even a small variation in schooling can significantly affect learning outcomes, therefore, contributing to the debate on the optimal length of a school year.

In developing countries, despite an extensive literature on the impact of educational interventions on schooling and learning (Kremer et al. 2013; Ganimian and Murnane 2016), there is little systematic assessment of the productivity of formal schooling. Indeed, interventions that succeed in improving enrollment and attendance often fail at increasing cognitive skills (Ponce and Bedi 2010; Filmer and Schady 2014), casting doubts on the capacity of schools to impart skills to the youth of developing countries. Nevertheless, a couple of studies using quasi-experimental designs have recently echoed findings from high-income countries. Using results on an admission test to a Kenyan public secondary school as a cutoff in a regression discontinuity approach, Ozier (2016) demonstrated that completing secondary school has a large positive impact on the accumulation of human capital and labor market outcomes later in life. In the context of Indonesia, Parinduri (2014) investigated the effect of a longer

school year on educational attainment and labor outcomes. Although the author found limited impacts on language skills, results suggest that a six-month increase in schooling led to a lower repetition rate and a higher probability of working in the formal sector.

Panel data offers perhaps the best opportunity to model the dynamic dimension of learning. Among the most noteworthy efforts in this regard is the work of Cunha et al. (2010) who used panel data to identify optimal periods of intervention, i.e., the periods when the use of specific inputs are more efficient or, conversely, when negative shocks are the most detrimental. The availability of panel test scores also allows for the use of individual fixed effects, thus capturing time-invariant unobservable characteristics, and for the estimation of value-added models of learning achievement (hereafter, VAMs). In the estimation, a measure of prior achievement is introduced in the production function with the aim of controlling for the complete history of observable and unobservable past inputs, reducing the potential for omitted variable bias (Todd and Wolpin 2003, 2007; Andrabi et al. 2011; Sass et al. 2014). Although widely used in US literature to assess the estimation of the impact of teacher and classroom characteristics on the gain in skills of their students over an academic year (Hanushek 1986; Rothstein 2010; Harris and Sass 2011), the value-added methodology has only recently been used in low-income countries settings, due to the increasing availability of panel test scores (Andrabi et al. 2011; Singh 2015a, 2015b; Glewwe et al. 2017).

In this study, we use the VAM methodology to estimate the extent to which an additional year of schooling contributes to the acquisition of cognitive skills among a cohort of young people born in the late 1980s in Madagascar. More precisely, we take advantage of a long-term panel data set to assess the role of formal schooling on the gain in skills, as measured by mathematics and French test scores, during the years that the cohort progresses from adolescence (when the cohort was 13 to 17 years of age) to adulthood (20 to 24 years old). The young adults in our sample had completed 8.2 years of education on average at the time they were last surveyed in 2012, and experienced 2.8 additional years of schooling during the interval from 2004 to 2012, between survey rounds. In OLS estimates, we find that, controlling for prior achievement, the value-added of an additional year of schooling at adolescence

ranges from 0.15 to 0.16 standard deviations. The estimated parameters are remarkably stable across specifications and robust to a potential omitted variable bias. As VAMs remain vulnerable to unobserved heterogeneity, we address the challenge of the endogeneity of additional schooling between survey rounds by using an instrumental variable (IV) approach. Controlling for the average quality of local schools and regional fixed effects, we use the presence of a higher secondary school in the community at adolescence and the number of years since the first primary school was built (proxies for the diffusion of investment in education infrastructure at the local level) as instruments for additional schooling. Interestingly, Two-Stage Least Squares (2SLS) estimates of the impact of schooling on skills, which we interpret as the local average treatment effect of schooling for the youth who have been induced into more schooling because of the availability of school infrastructure and long-term investment in school infrastructure in the community, yield greater parameters than OLS estimates. Consistent with Glick and Sahn (2009), family background is also an important determinant of cognitive ability, but the effect is primarily indirect through school attainment rather than direct, as shown by the modest effect of parents' education on skills, conditional on grade achievement.

This paper brings together two strands of literature. First, our findings contribute to the debate on the productivity of schooling in developing countries (Behrman et al. 2014; Singh 2015b; Ozier 2016). We estimate the causal effect of schooling on the accumulation of skills and provide evidence that schools successfully impart cognitive skills. In this regard, our study is similar to Singh (2015b), in which the author used the VAM methodology to estimate the differences in the productivity of years of schooling among primary school aged children in Peru and Vietnam. Our paper differs from this study by focusing mainly on the impact of secondary schooling on the production of skills, as post-primary education accounts for three-quarters of the additional schooling in our sample during the interval between the two surveys. Second, our paper relates to the literature on the transition to adulthood. We demonstrate that cognitive skills continue to increase markedly among adolescents who remain in school. Furthermore, our results provide evidence that adolescence is the period when the gap in skills widens, as students with higher cognitive skills in early adolescence complete more grades and accumulate more skills in their transition to adulthood.

The remainder of the paper is structured as follows. In the next section, after having briefly described the specificities of the Malagasy context, we detail the panel data set used in the analysis. Section 3 presents the conceptual framework of the production of cognitive skills. Section 4 sets out the estimation strategy. We report the results in Section 5. Finally, we conclude and discuss possible policy implications of our findings in Section 6.

2. Context and data

2.1. The Malagasy context

The challenges faced by the education sector in Madagascar are in many ways similar to the rest of sub-Saharan Africa. Following the abolition of school fees in 2002 and soon followed by a policy to provide school supplies to all primary school children, the gross primary enrollment rate substantially increased (from 99% in 2000 to 148% in 2015, according to the World Development Indicators (World Bank 2017) and is now above the mean for sub-Saharan Africa (98%). This rise in enrollment was closely linked to a large increase in the number of primary schools.[1] Additionally, there has been a trend toward a growing number of private schools, implying that parents in some communities have more than one primary schooling option for their children, even available in rural communities. Improvement in enrollment numbers, especially at the primary level, is tempered by the heterogeneous quality of school infrastructure. Multi-grade classes, overcrowded classrooms, and schools with incomplete cycles are common. Also, associations of parents have been given license to hire teachers locally when needed. Although this policy has facilitated the expansion of primary schools in remote areas, despite the shortage of trained teachers, the educational and pedagogical skills of those hired at the local level, often without any formal credentials, is worrying.

Schooling in Madagascar is characterized by high repetition and dropout rates and low primary

[1] During the school year 2010–2011, the Ministry of Education, recorded 21,877 public primary schools. There were only 12, 730 schools recorded 10 years before.

completion rates (Michaelowa 2001; World Bank 2008), all of which are tangible signs that the Malagasy education system fails at efficiently transmitting human capital to youth. Low quality secondary schooling is particularly worrisome, as only a small proportion of those who do complete primary school enroll in secondary school (World Bank 2008). In sharp contrast with primary schooling, the secondary school gross enrollment rate is only 38% (in 2014) and remains below the regional mean, with gross tertiary enrollment being only 4.2%. This low transition rate may in part be attributed to the high average distance to the nearest secondary school (especially, in rural areas where the number of lower and higher secondary schools remains limited) and to the direct and opportunity costs associated with secondary schooling. It may also reflect the poor performance of many of those who complete primary school on the national end-of-cycle exam required to advance to secondary school.

2.2. Data

Our analysis is based on a long-term panel data set, designed to investigate the transition to adulthood of a cohort of young people born in the late 1980s. Members of this cohort were initially interviewed when they were young adolescents (13 to 17 years old) in the *Progression through School and Academic Performance in Madagascar Study* (2004–05), and then resurveyed seven years later (*Madagascar Life Course Transition of Young Adults Survey*, 2011–12) as young adults (20 to 24 years old). The national sample used in this paper was comprised of 73 randomly selected communities, defined by being catchment areas for primary schools, from 50 of the country's 116 districts/*fivondrana* that geographically were located in the six regions of the country. The sample communities were randomly selected from a larger education study, the *Programme d'Analyse des Systèmes Educatifs de la CONFEMEN* (PASEC), which was conducted in 1998 in 120 clusters. The original 1998 PASEC survey targeted primary school pupils in communities with larger-than-average schools; the 2004–05 and 2012 surveys expanded the sample by adding new cohort members and their households in 25 rural communities with small primary schools to the randomly selected 48 clusters that were in the 1998 sample.[2] We focus only on the last two rounds of survey (2004–05 and 2011–12) and consider the former

[2] "Small" was defined as a school having fewer students than the national median of about 140. See Glick et al. (2011) for a detailed description of the 2004–05 survey.

as our baseline.[3] Nearly 89% of the targeted cohort members sampled in 2004–05 were re-interviewed in 2012, which makes the attrition rate remarkably small over such a long duration, as compared to other individual-based panel data surveys collected in sub-Saharan Africa.[4]

As previously noted, grade attainment is not sufficient to analyze the formation of human capital and, when available, tests scores offer a more direct and precise measurement of knowledge and cognitive skills. In our study, cohort members were tested in 2004–5 and 2011–12 for their knowledge in mathematics and French, making the data set particularly well suited for studying the production of cognitive skills during adolescence. Our analytical sample consists of 962 young adults who took at least one of the tests during each of the two rounds of survey. Additionally, the household surveys were accompanied by school- and community-based surveys that provide details on the nature and characteristics of existing infrastructure. In total, 140 schools were surveyed in 2011–12, in the 73 clusters where the survey was conducted. Information was gathered on the experience and credentials of the school principal, management of the school, the number of teachers, their qualifications and pedagogical practices, as well as building and classroom conditions.

2.3. Cognitive tests

The tests were designed by specialists from the Ministry of Education with the aim of obtaining a comparable measure of cognitive achievement among a heterogeneous population. Tests were identical for each cohort member, regardless of their grade achievement, starting with questions on basic knowledge and getting progressively more complicated.[5] Furthermore, tests were composed of an oral

[3] Restricting our sample to the cohort members interviewed three times would considerably reduce its statistical power and would raise issues about representativeness and selectivity.
[4] Missing cohort members are broadly similar to those who have been interviewed. We do not observe significant mean differences in most characteristics, with the notable exception of grade achievement at baseline. Interviewed cohort members had completed 0.4 more years of schooling at baseline. We do not observe any significant difference in test scores.
[5] As Banerjee et al. (2017) emphasized, traditional skills assessment instruments derived from formal schooling curricula may underestimate the level of skills of low-educated youth. By framing arithmetic problems into market transactions, they demonstrate that Indian children working in informal markets display higher levels of mathematics skills than previously assessed with traditional tests. While the tests used our analysis were designed by specialists from the Ministry of Education on the basis of formal schooling curricula, they include questions phrased as problems of everyday life, especially among the easier questions.

and a written part, allowing illiterate cohort members (a common situation, even after several years of schooling) to take the test, and administered at home, which mitigates somewhat the concern that the scores may be biased by intra-classroom correlation. Overall, tests contain 59 items at baseline and 57 at endline, including 16 overlapping questions. On average, young adults in our sample answered 43% of questions correctly. Internal consistency of each test, as expressed by the Cronbach's alpha, is above the recommended standard, ranging from 0.82 to 0.92. Two caveats are worth noting. First, the tests used in this study are not standard and, therefore, cannot be directly compared to international benchmarks or to the grade-specific curriculum. Second, at adulthood, skills were sometimes measured years after cohort members had left school. Therefore, test scores might be affected by skills depreciation since leaving school.

Test scores are generated using item response theory (IRT) methodology to minimize the effect of the tests structure on the scores. As opposed to simply scoring the percentage of correct answers, in which questions are weighted equally regardless of difficulty, IRT allows questions (items) to be weighted according to their difficulty and degree of discrimination. Intuitively, a correct answer to an easy question (a question most of the population answered correctly) is weighted less than a correct answer to a difficult question.[6] As a consequence, though they remain highly correlated with percentage-correct scores, IRT scores gives a more precise measure of cognitive skills as two individuals with the same percentage of correct answers may obtain varying IRT scores, depending on the set of questions they answered correctly. IRT scores are standardized with mean zero and standard deviation of one.

3. Descriptive statistics

The panel encompasses the period in the life course when young people and their families make decisions regarding investments in education: although 83 percent of young people in our sample were enrolled at school at baseline, only 23 percent were still enrolled at endline (Table 2).[7] During the

[6] See Das and Zajonc (2010) for an overview of the item response theory methodology.
[7] It is worth noting that the fact that nearly a quarter of our sample was still enrolled at school implies that our analysis underestimates the gap in skills between low- and high-educated individuals at adulthood.

interval between survey rounds, the share of the cohort who completed lower secondary schooling increased from 5 to 51 percent; 28 percent of the cohort completed upper secondary schooling; and the level of schooling rose from 5.4 to 8.2 years of schooling (Tables 2 and 3). On average, the cohort members completed 2.8 additional years of schooling between rounds, although this number masks much heterogeneity. Adolescents in the lower tail of the baseline skills distribution (i.e., first quartile of the baseline mathematics test score) completed only 1.7 additional years of schooling, while adolescents in the top quartile—who had already completed more years of schooling at baseline—completed 3.9 years on average (Figure 1), highlighting the fact that adolescence is a time when the gap in educational attainment among the population widens greatly. Three-quarters of the additional years of schooling between rounds are in the post-primary grades (half are low secondary grades and one-quarter are high secondary grades).

The heterogeneity in educational achievement partially reflects inequality in family backgrounds. As observed by Glick et al. (2011), educational achievement in Madagascar is positively associated with the household's wealth and parent's education (Table 3). The gradients between both wealth and parental education, and grade attainment, have persisted over the two rounds of the survey. Cohort members whose mothers had no education are likely to have experienced 1.9 additional years of schooling between the two surveys, while cohort members whose mothers reached at least secondary school experienced 4.1 additional years of schooling. Similarly, young adults originating from households in the richest quartile of the wealth distribution have completed twice as many years of schooling as young adults raised in households in the lowest wealth quartile. Although these relationships cannot be interpreted as causal, it suggests a strong intergenerational transmission of disparities in education.

The lack of gender bias in the educational outcomes at adulthood, in contrast to what is usually observed in the African context, is worth noting. We observe only limited differences between male and female cohort members in terms of their highest grade completed (respectively, 8.2 and 8.1 years of schooling). With the notable exception of young adults from disadvantaged backgrounds, among which males have

completed 0.7 more years of schooling than females, the gender gap is not significant in our sample. However, the difference in the dynamics of progression at adolescence by gender is worth noting: male cohort members, who were enrolled in lower grades on average at baseline, caught up with females during their transition to adulthood. In other words, females completed significantly fewer grades between rounds. This finding is consistent with the high prevalence of early pregnancy among Malagasy adolescents, leading to early dropouts (Herrera and Sahn 2018). Indeed, 13.5% of women in our sample mentioned being pregnant as a reason for dropping out of school; this number rose to 19.6%, when the sample was restricted to women with primary school education and above.

Echoing the concerns raised in the *World Development Report 2018*, the level of skills observed among our cohort of Malagasy young adults is worrisome. With the aim of illustrating the lack of numeracy and literacy skills in a less abstract way than a test score, Table 4 displays the percentage of correct answers to four basic questions from the mathematics and reading tests by level of education. On average, less than half of the sample was able to extract the correct information from a short paragraph and only two-thirds were able to solve a two-digit division problem. The statistics are particularly worrying for cohort members with primary education and lower: 39% of those who have not completed primary school scored no points out of nineteen on the reading test; 36% of cohort members with primary education were able to solve an addition/subtraction problem; only 54% were able to construct a sentence with five words.

As illustrated by Figure 2, there is a strong positive association between test scores and the level of schooling. There is a 2.04 standard deviation difference between cohort members with an incomplete primary education and those who have completed upper secondary schooling. The distribution of cognitive skills displays the same patterns observed in grade achievement: at both periods, test scores are positively associated with parents' levels of education and household's wealth and negatively associated with remoteness of the community from which the cohort members originated from (Table 5). The correlation between scores within rounds is strong and highly significant (0.70 at baseline and 0.81 at endline), indicating that cohort members who performed better at math tend to have a higher

score in French, too. Though, this result is partly driven by heterogeneity in grade attainment, correlation coefficients remain high for any given level of schooling (respectively, 0.53, 0.55, and 0.58 for young adults with 6, 10, and 13 years of education). We also observe a strong intertemporal correlation between scores, 0.50 and 0.52 for math and French scores, respectively, reflecting a persistent effect of past knowledge on learning achievement.

4. Conceptual framework

Following Todd and Wolpin (2003), we consider a simple cumulative model of human capital, as in the manner of Behrman and Birdsall (1983), in which we distinguish between the quantity and quality of schooling. In this model, the cognitive achievement of a student *i* at time *t*, A_{it}, is expressed as a function of years of schooling, $S_i(t)$, school quality, $Q_i(t)$, family inputs, $F_i(t)$, and a time-invariant student endowment, μ_{i0}, defined in the initial period. The subscript *t* refers to a specific period in the life of an individual when inputs enter into the production of cognitive skills and at the end of which achievement is measured. At each period, achievement at time *t* can be expressed as a function of present and past inputs, where the impact of inputs on the production is allowed to vary depending on the period:

$$A_{it} = g_t[S_i(t), Q_i(t), F_i(t), \mu_{i0}] \quad (1)$$

Years of schooling, the quality of schooling, and family inputs depend on choices made by teachers, parents, and the student herself, with the aim of maximizing the student's cognitive achievement. It can be depicted by the following equations:

$$S_{it} = \theta_t[A_i(t), W_i(t), F_i(t), Q_i(t), \mu_{i0}] \quad (2)$$

$$Q_{it} = \omega_t[A_i(t), W_i(t), \mu_{i0}] \quad (3)$$

$$F_{it} = \varphi_t[A_i(t), W_i(t), Q_i(t), \mu_{i0}], \quad (4)$$

where $W_i(t)$ represents the household resources.[8] At each period, the decision to invest in human capital, through more schooling or attending a better school, occurs in response to the observation of the previous level of achievement and initial endowment. For instance, parents may invest more in the education of students with a higher ability to translate schooling into skills. Conversely, they may decide to adjust their level of family inputs for less endowed students. At the school level, inputs may also respond to the student's achievement if, for instance, better teachers were assigned to less endowed students or if they allocated more of their time to them. School quality plays an important role in the allocation of family inputs and schooling decisions. Indeed, the schooling decision is presumably taken according to the expected gain in achievement from an additional year of schooling, which greatly depends on school quality, as observed by parents. If their perception of school quality is low, parents may compensate with more family inputs, by selecting a different school, and in some cases, by having a direct impact on school quality.

Three points thus stand out from this model of human capital accumulation. First, the estimation of Eq. (1) requires taking into account the entire history of inputs. Failure to do so would lead to inconsistent estimates of the impact of contemporaneous inputs. Second, the impact of school and family inputs, as estimated by Eq. (1), are expressed net of the impact on years of schooling. Lastly, and perhaps most importantly for our analysis, initial endowment (μ_{i0}) plays a crucial role in the production of cognitive achievement, as well as in the allocation of inputs, raising concerns about potential endogeneity issues. Methods to address these concerns are presented in the next section.

5. Estimation strategy

5.1. Cross-sectional and value-added models

In this paper, we investigate the contribution of schooling to the accumulation of cognitive skills, as

[8] In this simple model, household resources are considered exogenous, but this assumption may be violated, especially in developing countries, if the household income depends on child labor.

indicated by French and mathematics test scores measured at adulthood. We present two distinct models of the production of cognitive skills. First, we estimate a simple cross-sectional model in which cognitive achievement is expressed as a function of the highest grade attained, as well as contemporaneous and past inputs. In a second model, we take advantage of the panel dimension of our database to estimate the contribution of an additional year of schooling at adolescence to the stock of skills, conditional on initial achievement. We consider two periods in the production of cognitive skills of the individual, i. The first period, ($t = 1$), corresponds to the accumulation of skills that happens before the baseline survey, from early childhood to adolescence. The second period, ($t = 2$), corresponds to the transition period from adolescence to young adulthood. Cognitive achievement, A_{it}, is measured at the end of both periods. As depicted by Eq. (1), years of schooling, school quality, and family inputs enter the production function at each period.

In the absence of test score panel data, cognitive achievement at adulthood is expressed as a function of the highest grade attained and the whole history of contemporaneous and past inputs. The empirical model is given by:

$$A_{i2} = \beta_1 S_{i2} + \beta_2 X_{i1} + \beta_3 X_{i2} + \varphi_c + \mu_{i0} + \varepsilon_{i2} \qquad (5)$$

where A_{i2} is a measure of cognitive skills at adulthood and S_{i2}, the highest grade attained. X_{i1} and $X_{i2}$are vectors of family inputs and student's characteristics entering the production function at both periods. We use parents' education, household wealth, and household size as proxies for parental pedagogical inputs.[9] Economic shocks in both periods are included to capture the effect of unexpected variation in the household resources. The student's characteristics include variables such as gender, age at the time

[9] Following the methodology of Sahn and Stifel (2003), the wealth index is based on the ownership of durable goods (such as a radio, TV, bicycle, or a lamp), the source of drinking water, and type of toilet facilities. As household size might reflect a quantity-quality trade-off in numbers of children, we ran the models without this variable and with an alternative measure of family size that only includes adult members. Results were not sensitive to these modifications.

of testing, birth order, and adult height.[10] Additionally, φ_c is a vector of community fixed effects to control for the specificity and quality of the local educational system at baseline. Lastly, μ_{i0} is the cohort member's time-invariant (unobservable) endowment, and ε_{i2} a random disturbance term. Following the sampling strategy, standard errors are clustered at the community level.

The estimation of this cross-sectional model raises two concerns, both of which plague the vast majority of empirical studies that model education attainment and cognitive outcomes (Todd and Wolpin, 2003). First, in order to provide consistent estimates, the model requires the entire history of observable and unobservable inputs. In the likely case that omitted factors are correlated with years of schooling and other inputs, ordinary least squares would yield biased estimates of the contribution of schooling to the stock of skills. The second concern relates to the student unobservable endowment, which has an impact on both education attainment and cognitive outcomes. If students with high ability are more able to progress through grades and to translate schooling into skills, then failure to account for innate ability would lead to biased estimates of the effect of schooling on skills.

Our approach to handling the problem of unobserved endogeneity is to take advantage of the panel dimension of our data set to estimate a value-added model in which a measure of prior achievement is introduced in the production function, with the aim of capturing the effect of the complete history of observable and unobservable inputs that entered the production of skills in the first period.[11] The model is given by:

$$A_{i2} = \gamma A_{i1} + \beta_1(S_{i2} - S_{i1}) + \beta_2 X_{i1} + \beta_3 X_{i2} + \varphi_c + \mu_{i0} + \varepsilon_{i2} \qquad (6)$$

[10] Adult height is standardized by gender. It is introduced to control for nutritional deficiencies that could have affected the student's growth. This parameter should be interpreted cautiously, as we do not control for its potential endogeneity.

[11] See Todd and Wolpin (2003), Andrabi et al. (2011), or Sass et al. (2014) for a detailed description of the value-added model methodology.

where A_{i1} is the stock of skills measured at the end of the first period.[12] Intuitively, in this model, the parameter, β_1, estimates the gain in achievement associated with an additional year of schooling ($S_{i2} - S_{i1}$) at adolescence, conditional on the stock of skills estimated at baseline.[13] The inclusion of the lagged score reduces the potential for omitted variable bias by controlling for factors that shaped a student's cognitive achievement until the end of the first period. In the growing literature on the validity of the value-added methodology, a handful of studies have provided evidence that VAMs, when appropriately specified, yield consistent estimates, as compared to quasi-experimental and data-intensive methods (Andrabi et al. 2011; Chetty et al. 2014; Deming 2014; Singh 2015a, 2015b; Ozier 2016).

Nevertheless, the value-added methodology remains vulnerable to the endogeneity of additional schooling. As noted by Todd and Wolpin (2003), the conditions under which the estimation of Eq. (6) by OLS provides consistent estimates of the effect of schooling on cognitive achievement are likely to be violated. To yield unbiased parameters, OLS estimates require additional schooling to be uncorrelated with the student's unobserved endowment, conditional on prior achievement. However, as Rothstein (2010) and Singh (2015a) point out, the lagged score may not capture the entire history of unobserved factors that determined school attainment. Therefore, as the decision to pursue schooling at adolescence is not random and likely to be taken in accordance with the student's performance and innate ability, additional schooling at adolescence should be treated as endogenous. The direction of the bias is unknown. A positive correlation between the student endowment and both the gain in skills and additional schooling would create an upward bias of the effect of schooling on skills. Conversely, measurement errors, a common occurrence in skills assessment surveys, are likely to bias the parameter downward.

[12] In the literature, the lagged score is sometimes introduced in the left-hand side of the equation ($A_{it} - A_{it-1}$). This specification is more restricting, as it posits that $\gamma = 1$, which implies that prior achievement has a complete persistent effect on final achievement. Moreover, Hanushek et al. (2008a) argue that this restriction is not appropriate if the two tests are not on the same scale, as in our case.

[13] In the VAM specification, because the small variation in the time between survey rounds (the standard deviation is 3.8 months) is positively correlated with additional schooling, we control for the number of months between the two rounds.

5.2. Instrumental variable approach

To address the endogeneity of additional schooling in the VAMs, we use an instrumental variable approach. Facing a similar challenge, Behrman et al. (2014) used a set of instruments that includes the death of a parent, the salary in the manufacturing industry, a student–teacher ratio, and the availability of a lower secondary school. Our identification strategy focuses on the role of the supply side of schooling options in the community in determining education attainment at the baseline. Many studies have demonstrated how schooling decisions respond to the availability and distance to primary and secondary school (Duflo 2001; Filmer 2007; Zhang 2018). Mukhopadhyay and Sahoo (2016) showed that the effect of better access to secondary schools can already be found in primary school enrollment and attendance. Evidence from Madagascar, where the low population density is a major challenge to the provision of education, also suggests that distance to secondary schools plays a key role in the schooling decisions for young children of primary school age (Glick and Sahn 2006).

In this paper, we use the presence of a high school in the community and the number of years since the first primary school was built as exogenous instruments.[14] We interpret the two indicators as proxies for the diffusion of public investment in education at the local level. At baseline, when cohort members were 13 to 17 years old and less than 1% of them had completed lower secondary schooling, a high school was available in only 43% of the sampled communities. There were at least two high schools in a quarter of the sampled communities. The average number of years that a primary school was available in the commune was 43 years. In 10% of the sampled communities, the first primary school opened 17 years or less before baseline. For adolescents, the lack of a secondary school in the commune implies a higher distance to the nearest school and, thus, a higher cost of attendance, and is expected to have a large disincentive impact on enrollment and progression even through primary, especially in the Malagasy context where parents might be reluctant to let their children leave the household.

[14] By design, there was at least one primary school in every sampled community. Both instruments refer to the situation at the time of the baseline survey.

The two-stage least square (2SLS) model is given by the following equation:

$$A_{i2} = \gamma A_{i1} + \beta_1(S_{i2} - S_{i1}) + \beta_2 X_{i1} + \beta_3 X_{i2} + \beta_4 Q_{i1} + \beta_5 Q_{i2} + \varphi_r + \mu_{i0} + \varepsilon_{i2} \quad (7)$$

In this specification, as instruments are computed at the community level, 2SLS regressions are estimated with regional fixed effects, φ_r, and a set of variables controlling for the average quality of schools in the community, $Q_{i1}$and Q_{i2}, entering the production function at both periods. The validity of our identification rests on the assumption that instruments do not have a direct effect on cognitive outcomes, conditional on the quality of the school supply, prior achievement, and other covariates included in the regression. Though instruments capture the availability of schools rather than their quality, it is essential to control for the average quality of schools in the community. The school quality vector includes an index of primary schools' infrastructures and equipment, the share of primary teachers with high school education, peers' performances relative to the national mean and a community remoteness index.[15] In order to deal with the potential endogeneity of these school quality indexes, they are expressed as community averages (if more than one school) and are also measured at baseline.

Two potential concerns should be noted. First, school placement is not random and likely to respond to education demand. Second, as parents have the choice to settle closer to secondary schools, parents with higher preferences for education may be more likely to live in areas where a high school is available. At the time of the baseline survey, a large majority of adolescents (89%) had always lived in the commune where they were interviewed, but we cannot rule out that the parents of the remaining 11% moved into

[15] The primary schools' infrastructures and equipment index were created using a factor analysis. The school characteristics used in the computation include the share of classrooms with a blackboard, a student per book ratio, the presence of latrines, separate toilet facilities for girls, an infirmary, a sports playground, and whether electricity and drinking water are available. Peers' performances are computed as the average gain in score between rounds in the community standardized by grade at baseline (one's own personal gain in score is excluded from the computation). The remoteness index is based on the availability of health services, banks, post offices, schools, markets, and the access to roads. Because of missing data on key variables in some communities, our analytical sample is restricted to 68 clusters.

a new commune in order to get access to secondary school infrastructures.[16] Moreover, among the 9.6% of cohort members who were not living with their parents at baseline, only 22% of them (less than 2% of the sample) have lived in more than one commune since their birth, suggesting that child fostering related to the access of school infrastructures is not common in our sample. Additionally, through the inclusion of the secondary school instrument, we greatly mitigate any concerns with the potential for a direct impact on learning by increasing the time required for travel and thus reducing time for schoolwork. Instead, this instrument that captures the presence of a secondary school is expected to act primarily as an inducement to complete primary school and continue to the secondary level. That is, the possibility that access to secondary will also have some direct effects on skill acquisition is very small given the that few of the cohort members in the sample have completed primary school and had any secondary school education.

6. Results

In this section, we report results from the estimation of a cognitive skills production function, providing evidence that formal schooling imparts skills in Madagascar. After briefly presenting results from the cross-sectional model, we turn to the VAM estimates, our preferred specification. As a robustness check, we assess the sensitivity of our results to omitted variable bias and present results from an instrumental variable approach. While our focus is on the productivity of schooling in improving human capital, we also investigate the role of family background and other covariates in the production of skills.

6.1. Effect of schooling on cognitive skills

We begin with the estimate of the cross-sectional model in which the stock of skills at adulthood is explained by the highest grade completed. The first two columns of Table 6 display OLS estimates of

[16] Unfortunately, the baseline survey does not provide the reason why households have migrated. The follow-up survey does include a migration module that asks respondents why they moved. While a third of the migrants have moved for reasons related to schooling overall, this is the case for only 15% of those who moved before the baseline survey.

the cross-sectional model using community fixed effects to control for the quality of the environment in the cohort member's location of origin. Conditional on family background, one year of schooling is associated with a 0.17 standard deviation increase in both French and mathematics test scores. As previously mentioned, the schooling parameter estimated with OLS cannot be interpreted as causal, because estimations fail to control for past inputs and unobserved heterogeneity that are likely to bias the results.

We next turn to the value-added models in which the introduction of the lagged score as a covariate is expected to capture the unobserved heterogeneity which played a role in the production of skills before baseline. Results are presented in columns 3 and 4 of Table 6. Interestingly, OLS estimates yield schooling parameters of similar magnitude as the ones estimated in the cross-sectional model: conditional on initial achievement, an additional year of schooling at adolescence increases the stock of skills by 0.15 to 0.16 standard deviations. As shown in Tables 7 and 8, the parameters are remarkably stable across specifications when we include variables that capture family background, students' characteristics and economic shocks experienced before baseline and in-between surveys. Depending on the score and specification, the value-added of an additional year of schooling is highly significant and ranges from 0.15 to 0.18 standard deviations. Echoing recent findings from developing countries (Ozier 2016; Singh 2015b), these results provide evidence that, despite its low quality, formal schooling translates into cognitive skills in Madagascar. The results also highlight a persistent effect of schooling on skills among cohort members who took the test at adulthood, several years after most of them had left school.

Despite the introduction of prior achievement in the model to control for the effect of past inputs, the potential for omitted variable bias remains a concern for the analysis. Many factors that enter the production function, such as innate ability or personality traits, remain mostly unobservable and may bias the estimates of the productivity of schooling. Following the methodology developed by Oster (2017), we test the robustness of our results to omitted variable bias. Oster's idea is to exploit changes in coefficients and R-squared with the introduction of covariates in the models to compute a lower bound

for the parameter of interest, under a set of assumptions about the degree of selection on unobservables. Table 9 reports the results of this calculation for the VAMs.[17] Under the assumption that selection on observables and unobservables is proportional, and for a R_{max} set to 1.3 R-squared, as recommended by Oster (2017), the identified set for both models does not include zero. The lower bound is respectively 0.12 and 0.09 standard deviations for French and mathematics. Under the same assumptions, the omitted variables would have to be 1.64 to 2.06 times as important for test scores than control variables to eliminate the effect of additional schooling entirely, which is unlikely. In other words, the effect of additional schooling on the accumulation of cognitive skills could not be entirely attributed to the influence of unobservables.

6.2. Instrumental variable estimates

As mentioned above, VAMs remain vulnerable to the endogeneity of additional schooling, despite the introduction of previous achievement in the model, (Todd and Wolpin 2003; Rothstein 2010). As a robustness check, we present Two-Stages Least Squares estimates (2SLS) of the VAMs, where we use the presence of a high school in the community and the number of years since the first school was built as exogenous instruments to address the potential endogeneity of additional schooling. The first-stage estimates (presented in Table A.1 in the appendix) indicate that the instruments perform well. In both models, instruments used in the 2SLS estimates have a strong predictive power on schooling: living close to a high school or in a community where the presence of a primary school has been established for a longer duration of time have a large effect on grade attainment and additional schooling at adolescence. In all specifications, instruments are highly significant and the F-tests of excluded instruments are above 10. Additionally, the Kleibergen–Paap test statistics reject the hypothesis that the instruments are weak. Neither of the instruments appear to be redundant in predicting additional schooling. Furthermore, in all cases we fail to reject the overidentification test (Hansen J test).

[17] Results are computed using the Stata command *psacalc* provided by Oster (2017). The baseline model only includes controls for gender, age, lagged score and number of months between the two rounds of surveys. The full controls include parents' education, household wealth quartile dummies, gender of household head, household size, birth order and economic shocks. Results are similar when using no control at all in the baseline model.

Results are presented in tables 10 and 11 and show that, conditional on initial achievement, an additional year of schooling at adolescence increases the stock of cognitive skills at adulthood by 0.24 standard deviations, for both tests, in the specification in which all covariates are included. These results are highly significant and robust to a variety of other specifications; as with OLS results, the parameter associated with additional schooling remains stable as covariates are progressively introduced in the estimates.

We speculate that the larger coefficients in the instrumental variables models capture the local average treatment effect, that is, the effect of schooling on skills for those who have been induced into more schooling because of the availability of a high-school and long-term investments in school infrastructure in the communities in which they reside. In other words, cohort members who have been incentivized to attain more schooling because of the availability of school infrastructure (the compliers) have experienced larger returns to education, as measured by skills, than the rest of the population. One interpretation is that the "never-takers" (cohort members who are less likely to progress through school regardless of the availability of school infrastructure) have lower preferences for education and, therefore, may acquire fewer skills at school for a given year of schooling.[18] Interestingly, this result and interpretation is consistent with what has been reported in the returns to schooling literature where coefficients estimated with an instrumental variable have exceeded coefficients generated from OLS models (Oreopoulos, 2006).

Nevertheless, the coefficient in instrumental variable models are not statistically different from the ones estimated using OLS, and the endogeneity test suggests that additional schooling is not endogenous in the VAMs. Therefore, we consider the VAMs estimated with OLS to be the most appropriate specification to estimate the effect of additional schooling on skills in our setting, as it shows the average effect of additional schooling among the population rather than a local average treatment effect.

[18] Conversely, "Always-takers" (cohort members whose decision to progress through school is not influenced by the availability of school infrastructure) are expected to have higher preference for education. In the Malagasy context, however, the share of "never-takers" is likely to exceed the share of "always-takers".

6.3. Heterogeneity of the effect of additional schooling

We focus on OLS estimates to explore the heterogeneity of the effect of schooling on skills. First, we investigate whether the productivity of an additional year of schooling differs by level of schooling. To answer this question, we distinguish the number of additional years of schooling by the school level in which the additional schooling was achieved. As mentioned in the descriptive statistics section, half of the additional years of schooling between rounds are low secondary grades, and one-quarter are high secondary grades. Only 3.8% of additional years of schooling were achieved in higher-education. Results, presented in Table 12 suggest that the value-added of additional schooling differs by school cycle, with higher productivity of schooling being observed in high secondary and above. In the French score model, an additional year of schooling in lower secondary, high secondary and higher education are respectively associated with a 0.13, 0.23 and 0.31 standard deviation increases in skills. Coefficients are statistically different from each other. Even though we see this result as evidence of heterogeneity in the productivity of schooling, we do not give it a causal interpretation as it may illustrate higher productivity for cohort members who select into higher level of schooling. Additionally, the result may partly reflect a lower skills depreciation after dropping out of school for cohort members with higher grade achievement, even though the models include controls for initial achievement and the time between the two rounds of survey.

6.4. Dynamic in the accumulation of skills

VAMs estimates also shed light on the dynamic in the accumulation of skills. The lagged score coefficients, which estimates the role of prior achievement in the production of skills, are 0.20 to 0.25 of a standard deviation for French and Math, respectively, in our preferred specification (columns 3 and 4 of Table 6), and the magnitude of the effect of prior achievement decreases as covariates are progressively introduced in the regression (Tables 7 and 8). Consistent with the VAM literature (Andrabi et al. 2011; Singh 2015b), this suggests that persistence of previous knowledge is low (a coefficient of 1 would imply perfect persistence).This finding has to be put in perspective with the length of the time

span between the two surveys (7 years), and a potential depreciation of the stock of skills for those who left school in between rounds. As test scores are a noisy measurement of cognitive skills, coefficients in lagged score are also likely to suffer from an attenuation bias.

Moreover, as illustrated in Appendix Table A.2, which present OLS estimates of the determinants of additional schooling at adolescence, prior achievement is a strong predictor of additional schooling between rounds: for a given level of education at baseline and family background, a one standard deviation of test score is associated with half a year of additional schooling. Taken together, those results indicate that adolescents who had accumulated more skills at baseline performed better in the transition to adulthood, showing evidence that the gap in skills widens at adolescence and that there is no catch-up phenomenon for adolescents who lagged behind in school.

6.5. Effect of family background and other covariates

Although schooling appears to be the main vehicle for the acquisition of cognitive skills, family background and resources have both a direct and an indirect effect on skills formation. Consistent with Glick and Sahn (2009), the main effect of family background operates through grade attainment. This can be seen in Appendix Table A.2, which shows that both parents' education and wealth are strong predictors of additional schooling at adolescence, even after controlling for baseline school achievement. Having a father with 6 years of education is associated with an additional half a year of schooling at adolescence, as compared to having a father with no education. In addition to this indirect effect, the VAMs also display evidence of a direct effect of family background on skills accumulation at adolescence, conditional on schooling and prior achievement. Although the positive direct effect of parent's education remains modest, we observe a large and significant direct wealth effect on skills: conditional on schooling and prior achievement, young adults who were residing in households in the richest wealth quartile when they were adolescents have scores 0.36 standard deviations higher in the VAM estimates (Table 6), as compared to the young adults who were from households in the lowest wealth quintile when they were adolescents. The result that economic disadvantage among youth

translates into disparities in educational achievement is consistent with other studies from Madagascar (Fernald et al. 2011; Glick et al. 2011).

Furthermore, we test the complementarity of schooling and family background by interacting additional schooling with parent's education and family wealth. A positive interaction would be evidence of synergism, with cohort members from more favorable backgrounds receiving more advantage from schooling, while a negative interaction would suggest that these inputs are substitutes. The results, presented in Tables 13 and 14, support the substitute hypothesis. The coefficients of the interaction between schooling and both parents' education and household assets index are negative, though the effect size is of small magnitude.

In addition to wealth, as measured by an asset index, fluctuations in the household incomes can have detrimental effects on the accumulation of skills. In the absence of efficient credit markets, school attendance is likely to be disrupted by shocks, as households may use child labor to cope with unexpected drops in income (Jacoby and Skoufias 1997; Gubert and Robilliard 2008; Glick et al. 2016; Senne 2014). Our VAM estimates provide mixed evidence on the effects of economic shocks on learning.[19] Consistent with this literature, economic shocks that occurred prior to the baseline survey, when cohort members were pre-adolescent children, appear to have long lasting effects among the young adults in our sample. The number of self-reported, unusually severe harvests shocks experienced by households before the baseline survey had a negative impact on the French test score, while unusually good harvests shocks had a positive impact on the math score. More surprisingly, however, none of the economic shocks experienced after baseline had a significant impact on skills acquisition. One potential explanation is that more recent shocks are less likely to affect schooling outcomes as they happened at a time when fewer cohort members were enrolled.

[19] Shocks are measured by the number of self-reported unusually severe harvests, unusually good harvests or good performance of family enterprises and livestock loss. Respectively, 28% and 46% of the sample had experienced at least one unusually bad harvest before the baseline survey and before the endline survey.

While the primary focus of our research is on the effect of schooling, as well as family background on cognition, we are also interested in the important role played by early childhood health in schooling and skills.[20] Following the literature (Case and Paxson 2008; Vogl 2014; LaFave and Thomas 2017), we use adult stature as a proxy for long-term nutritional status, generally and, more specifically, the most vulnerable period in terms of growth retardation—in utero and through 24 months of age. We therefore include the height of the young adults in our models, which has the additional benefit of controlling for the confounding effect that health-related human capital might have on our primary question of interest. First, we note that the introduction of height in the regressions has no effect on the schooling coefficients (Tables 7 and 8). One note of caution is that while the relationship of health and education outcomes is of considerable interest, it must be interpreted carefully, as both nutrition and education outcomes are determined by parents' choices, on the basis of their assessments of and preferences for their children's welfare, which is widely unobservable. Like Alderman et al. (2006), OLS estimates of the determinants of years of schooling show that childhood health status – as measured by height – is positively associated with completed schooling as an adult (results are not shown). However, in the VAM specification, adult height is not associated with additional schooling, suggesting that long-term nutrition matters more at younger ages than at adolescence. While the relationship between height and cognitive outcomes has been reported in many papers (Case and Paxson 2008; Lundborg et al. 2014; Vogl 2014; LaFave and Thomas 2017), the positive parameter estimate on height is not statistically significant in our models. Moreover, as expressed by the non-significant interaction term between additional schooling and adult height, the productivity of schooling does not differ by nutritional status (Tables 13 and 14).

The results also display evidence of a gender gap in the production of cognitive skills. The cross-sectional estimates suggest that, for a given level of schooling, male cohort members outperform females in mathematics, but not in French (Table 6). The same pattern is observed in the VAM estimates where the male dummy is associated with 0.09 standard deviation increase in the mathematics score, about

[20] See, for example, Almond and Mazumdar (2013) and Currie and Vogl (2013) for excellent reviews on this topic. In addition, there is a substantial literature, primarily from developed countries, which looks at the development of human capital before the age of five on a range of later in life outcomes (Currie and Almond 2011).

half of the effect of an additional year of schooling. This finding is consistent with Dickerson et al. 2015 that documented an average gender gap in skills of 0.1 standard deviations among primary school students in a set of African countries.[21] However, we do not observe a significant interaction between gender and additional schooling (Tables 13 and 14), suggesting that the observed gap in the mathematics test score is not attributable to a lower productivity of schooling among women. Lastly, this gender gap in skills occurs in addition to the gender disparities in progression through school at adolescence: the gender gap in skills widens in the transition to adulthood, as male cohort members completed 0.36 to 0.42 more years of schooling than females, for a given level of prior achievement (Table A.2). This could possibly be a result of a greater reluctance of parents to allow their adolescent daughters to leave home to attend secondary school, as well as other reasons such as early pregnancies interrupting schooling among teenage girls.

7. Conclusion

In this paper, we provide evidence that schooling contributes to higher test scores, demonstrating the importance of schooling in the context of skills accumulation in the period of transition from teenage years to adulthood in Madagascar. Using the value-added methodology, we estimate that an additional year of schooling at adolescence increases mathematics and French test scores by 0.15 and 0.16 standard deviations respectively, in specifications that control for family background, students' characteristics and economic shocks. This finding is robust to a potential omitted variable bias, as the lower bound is respectively 0.09 and 0.12 standard deviations for mathematics and French. Furthermore, instrumental variable estimates, used as a robustness check, yield parameters of higher magnitude (0.24 standard deviations), which we interpret as a local average treatment effect, confirming the role of schooling in the accumulation of skills. These results are concordant with recent studies in low-income countries that find a large long-lasting effect of schooling on cognitive skills (Parinduri 2014; Singh 2015b; Ozier

[21] In their VAM specification with data from Madagascar, they estimate a gender gap of 0.068 standard deviations, just below our own estimates.

2016). We contribute to this literature by focusing on additional schooling among teenagers who are transitioning to adulthood. As the additional schooling between survey rounds for three-quarters of the cohort happened in low and high secondary grades, our findings largely reflect the productivity of post-primary grades.

Further progress in schooling appears to be the main vehicle for the acquisition of skills. Consistent with Glick and Sahn (2009), we find that, while family background is an important determinant of cognitive ability, its effect is primarily indirect—through school attainment—rather than direct, as shown by the modest effect of parents' education and assets on skills, conditional on grade achievement.

That being said, adolescence is the period when the skills gap widens as a consequence of the dynamics of additional schooling. There is no catch-up phenomenon for those who have not mastered basic literacy and numeracy skills by early adolescence. Indeed, adolescents who have accumulated more cognitive skills at early adolescence complete more grades in their transition to adulthood. The progression through grades at adolescence depends largely on family background, suggesting that disparities in education and skills replicate economic disparities.

Since schooling is a powerful device to increase the average level of cognitive skills in a population, attention should be given to efforts to delay dropping out of school, through policies that increase the availability and access to secondary school. Perhaps, most important, while there is considerable concern about the low quality of schooling in poor countries such as Madagascar and the current focus in the education sector is on quality improvements rather than increasing enrollments, our study provides strong evidence that schooling, despite its low quality, is the main mechanism for achieving better cognitive skills among young adults.

Lastly, two caveats are worth noting while interpreting the productivity of schooling estimated in this paper. First, our analysis is not based on a standard international test (such as the PISA or PASEC tests), which prevents us from making a comparison between the distribution of skills in our sample and

international benchmarks. Furthermore, although the tests were designed on the basis of the formal curriculum, scores do not inform whether the observed level of skills matches the French and mathematics curriculum for each grade. In that respect, as shown in the descriptive statistics section and consistent with other studies (Michaelowa 2001; Glick et al. 2011), learning remains low in Madagascar. Second, our findings are limited to the production of general skills, but the production of technical skills is of great concern in African economies, where the share of the manufacturing sector is growing. Technical training programs, such as apprenticeships and vocational training, are gaining popularity among the youth in sub-Saharan countries, especially among low-educated youth (Filmer and Fox 2014). Further research should investigate the role of general skills in the decision and opportunities to undertake technical training and the production of technical skills.

References

Alderman, H., Hoddinott, J., and Kinsey, B. 2006. "Long term consequences of early childhood malnutrition." *Oxford Economic Papers* 58(3): 450–74.

Almond, D., and Mazumder, B. 2013. "Fetal origins and parental responses." *Annual Review of Economics* 5(1): 37–56.

Andrabi, T., Das, J., Khwaja, A. I., and Zajonc, T. 2011. "Do value-added estimates add value? Accounting for learning dynamics." *American Economic Journal: Applied Economics* 3 (3): 29–54.

Banerjee, A.V., Bhattacharjee, S., Chattopadhyay, R., and Ganimian, A. J. 2017. "The untapped math skills of working children in India: Evidence, possible explanations, and implications." Working paper, New York University.

Banks, J., and Mazzonna, F. 2012. "The effect of education on old age cognitive abilities: Evidence from a regression discontinuity design." *Economic Journal 122*(560): 418–48.

Behrman, J. R., and Birdsall, N. 1983. "The quality of schooling: Quantity alone is misleading." *American Economic Review* 73(5): 928–46.

Behrman, J. R., Hoddinott, J., Maluccio, J. A., Soler-Hampejsek, E., Behrman, E. L., Martorell, R., Ramírez-Zea, M., and Stein, A. D. 2014. "What determines adult cognitive skills? Influences of pre-school, school, and post-school experiences in Guatemala." *Latin American Economic Review* 23(1): 1–32.

Bigsten, A., Isaksson, A., Söderbom, M., Collier, P., Zeufack, A., Dercon, S., Fafchamps, M., Gunning, J. W., Teal, F., Appleton, S., and Gauthier, B. 2000. "Rates of return on physical and human capital in Africa's manufacturing sector." *Economic Development and Cultural Change* 48(4): 801–27.

Boissiere, M., Knight, J. B., and Sabot, R. H. 1985. "Earnings, schooling, ability, and cognitive skills." *American Economic Review* 75(5): 1016–30.

Carlsson, M., Dahl, G. B., Öckert, B., and Rooth, D. O. 2015. "The effect of schooling on cognitive skills." *Review of Economics and Statistics* 97(3): 533–47.

Case, A., and Paxson, C. 2008. "Stature and status: Height, ability, and labor market outcomes." *Journal of Political Economy* 116(3): 499–532.

Chetty, R., Friedman, J. N., and Rockoff, J. E. 2014. "Measuring the impacts of teachers I: Evaluating bias in teacher value-added estimates." *American Economic Review* 104(9): 2593–632.

Currie, J., and Almond, D. 2011. "Human capital development before age five." In *Handbook of Labor Economics*, Vol. 4, Part B, edited by D. Card and O. Ashenfelter, 1315–1486. Amsterdam: Elsevier.

Currie, J., and Vogl, T. 2013. "Early-life health and adult circumstance in developing countries." *Annual. Review of Economics* 5(1): 1–36.

Cunha, F., Heckman, J. J., and Schennach, S. M. 2010. "Estimating the technology of cognitive and noncognitive skill formation." *Econometrica* 78(3): 883–931.

Das, J., and Zajonc, T. 2010. "India shining and Bharat drowning: Comparing two Indian states to the worldwide distribution in mathematics achievement." *Journal of Development Economics* 92(2): 175–87.

Deming, D. J. 2014. "Using school choice lotteries to test measures of school effectiveness." *American Economic Review* 104 (5): 406–11.

Dickerson, A., McIntosh, S. and Valente, C., 2015. Do the maths: An analysis of the gender gap in mathematics in Africa. *Economics of Education Review*, *46*, pp.1-22.

Duflo, E. 2001. "Schooling and labor market consequences of school construction in Indonesia: Evidence from an unusual policy experiment." *American Economic Review* 91 (4): 795–813.

Fernald, L. C., Weber, A., Galasso, E. and Ratsifandrihamanana, L. 2011. "Socioeconomic gradients and child development in a very low income population: Evidence from Madagascar." *Developmental Science* 14 (4): 832–47.

Filmer, D. 2007. "If you build it, will they come? School availability and school enrolment in 21 poor countries." *Journal of Development Studies* 43 (5): 901–28.

Filmer, D., and Fox, L. 2014. *Youth Employment in Sub-Saharan Africa*. Washington, DC: World Bank Publications.

Filmer, D., and Schady, N. 2014. "The medium-term effects of scholarships in a low-income

country." *Journal of Human Resources* 49(3): 663–94.

Ganimian, A. J., and Murnane, R. J. 2016. "Improving education in developing countries: Lessons from rigorous impact evaluations." *Review of Educational Research* 86(3): 719–55.

Glewwe, P., Krutikova, S., and Rolleston, C. 2017. "Do schools reinforce or reduce learning gaps between advantaged and disadvantaged students? Evidence from Vietnam and Peru." *Economic Development and Cultural Change* 65(4): 699–739.

Glick, P., Randrianarisoa, J. C., and Sahn, D. E. 2011. "Family background, school characteristics, and children's cognitive achievement in Madagascar." *Education Economics* 19 (4): 363–96.

Glick, P., and Sahn, D. E. 2006. "The demand for primary schooling in Madagascar: Price, quality, and the choice between public and private providers." *Journal of Development Economics* 79 (1): 118–145.

Glick, P., and Sahn, D. E. 2009. "Cognitive skills among children in Senegal: Disentangling the roles of schooling and family background." *Economics of Education Review* 28(2): 178–88.

Glick, P. J., Sahn, D. E., and Walker, T. F. 2016. "Household shocks and education investments in Madagascar." *Oxford Bulletin of Economics and Statistics* 78 (6): 792–813.

Gubert, F., and Robilliard, A. S. 2008. "Risk and schooling decisions in rural Madagascar: A panel data-analysis." *Journal of African Economies* 17(2): 207–38.

Hansen, B. 2011. "School year length and student performance: Quasi-experimental evidence." Department of Economics, University of Oregon.

Hanushek, E. A. 1986. The economics of schooling: Production and efficiency in public schools. *Journal of Economic Literature* 24(3): 1141–77.

Hanushek, E. A., Lavy, V. C., and Hitomi, K. 2008. "Do students care about school quality? Determinants of dropout behavior in developing countries." *Journal of Human Capital* 2(1): 69–105.

Hanushek, E. A., and Woessmann, L. 2008. "The role of cognitive skills in economic development." *Journal of Economic Literature* 46(3): 607–68.

Harris, D. N., and Sass, T. R. 2011. "Teacher training, teacher quality and student achievement." *Journal of Public Economics* 95(7–8): 798–812.

Herrera, C., and Sahn, D. 2018. "The impact of early childbearing on schooling and cognitive skills among young women in Madagascar." *Demography* 55(2): 643–68.

Jacoby, H. G., and Skoufias, E. 1997. "Risk, financial markets, and human capital in a developing country." *The Review of Economic Studies* 64(3): 311–35.

Jones, S., Schipper, Y., Ruto, S., and Rajani, R., 2014. "Can your child read and count? Measuring learning outcomes in East Africa." *Journal of African Economies* 23(5): 643–72.

Kremer, M., Brannen, C., and Glennerster, R. 2013. "The challenge of education and learning in the developing world." *Science* 340(6130): 297–300.

Kuepie, M., Nordman, C. J., and Roubaud, F. 2009. "Education and earnings in urban West Africa." *Journal of Comparative Economics* 37(3): 491–515.

LaFave, D., and Thomas, D. 2017. "Height and cognition at work: Labor market productivity in a low income setting." *Economics & Human Biology* 25: 52–64.

Lassibille, G., and Tan, J. P. 2005. "The returns to education in Rwanda." *Journal of African Economies* 14(1): 92–116.

Lundborg, P., Nystedt, P., and Rooth, D.O. 2014. "Height and earnings: The role of cognitive and noncognitive skills." *Journal of Human Resources* 49(1): 141–66.

Michaelowa, K. 2001. "Primary education quality in francophone Sub-Saharan Africa: Determinants of learning achievement and efficiency considerations." *World Development* 29(10): 1699–716.

Mukhopadhyay, A., and Sahoo, S. 2016. "Does access to secondary education affect primary schooling? Evidence from India." *Economics of Education Review* 54: 124–42.

Oreopoulos, P., 2006. Estimating average and local average treatment effects of education when compulsory schooling laws really matter. *American Economic Review*, *96*(1), pp.152-175.

Oster, E., 2017. Unobservable selection and coefficient stability: Theory and evidence. *Journal of Business & Economic Statistics*, pp.1-18.

Ozier, O., 2016. "The impact of secondary schooling in Kenya: A regression discontinuity analysis." *Journal of Human Resources* 53(1): 157–88.

Parinduri, R. A., 2014. "Do children spend too much time in schools? Evidence from a longer school year in Indonesia." *Economics of Education Review* 41: 89–104.

Ponce, J., and Bedi, A. S. 2010. "The impact of a cash transfer program on cognitive achievement: The Bono de Desarrollo Humano of Ecuador." *Economics of Education Review* 29(1): 116–25.

Pritchett, L. 2013. *The Rebirth of Education: Schooling Ain't Learning*. Washington, DC: Center for Global Development.

Pritchett, L., and Sandefur, J. 2017. "Girls' schooling and women's literacy: schooling targets alone won't reach learning goals." CGD Policy Paper 104, Center for Global Development, Washington, DC.,

Rothstein, J. 2010. "Teacher quality in educational production: Tracking, decay, and student achievement." *Quarterly Journal of Economics* 125(1): 175–214.

Sahn, D. E., and Stifel, D. 2003. "Exploring alternative measures of welfare in the absence of expenditure data." *Review of Income and Wealth* 49(4): 463–89.

Sandefur, J. 2018. "Internationally comparable mathematics scores for fourteen African countries." *Economics of Education Review* 62: 267–86.

Sass, T. R., Semykina, A., and Harris, D. N. 2014. "Value-added models and the measurement of teacher productivity." *Economics of Education Review* 38: 9–23.

Senne, J. N. 2014. "Death and schooling decisions over the short and long run in rural Madagascar." *Journal of Population Economics* 27(2): 497–528.

Singh, A. 2015a. "Private school effects in urban and rural India: Panel estimates at primary and secondary school ages." *Journal of Development Economics* 113:16–32.

Singh, A., 2015b. "Learning more with every year: School year productivity and international learning divergence." CESifo Area Conference on the Economics of Education, CESifo Group, Munich, September 1–2.

Söderbom, M., Teal, F., Wambugu, A., and Kahyarara, G. 2006. "The dynamics of returns to education in Kenyan and Tanzanian manufacturing." *Oxford Bulletin of Economics and Statistics* 68(3): 261–88.

Todd, P. E., and Wolpin, K. I. 2003. "On the specification and estimation of the production function for cognitive achievement." *Economic Journal* 113(485): F3–F33.

Todd, P. E., and 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.

Vogl, T. S. 2014. "Height, skills, and labor market outcomes in Mexico." *Journal of Development Economics* 107: 84–96.

World Bank 2008. "The challenge of expanding secondary education and training in Madagascar." Working Paper No. 141, World Bank, Washington, DC.

World Bank. 2018. *World Development Report 2018: Learning to Realize Education's Promise*. Washington, DC: World Bank.

Zhang, S., 2018. Effects of High School Closure on Education and Labor Market Outcomes in Rural China. *Economic Development and Cultural Change*, *67*(1), pp.171-191.

FIGURES

Figure 1. Grade progression between rounds by quartile of mathematics scores at adolescence

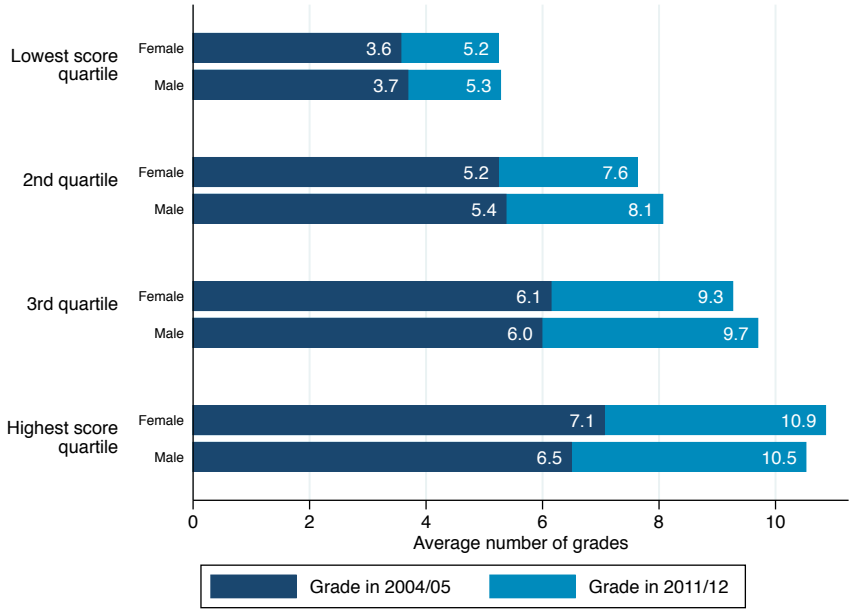
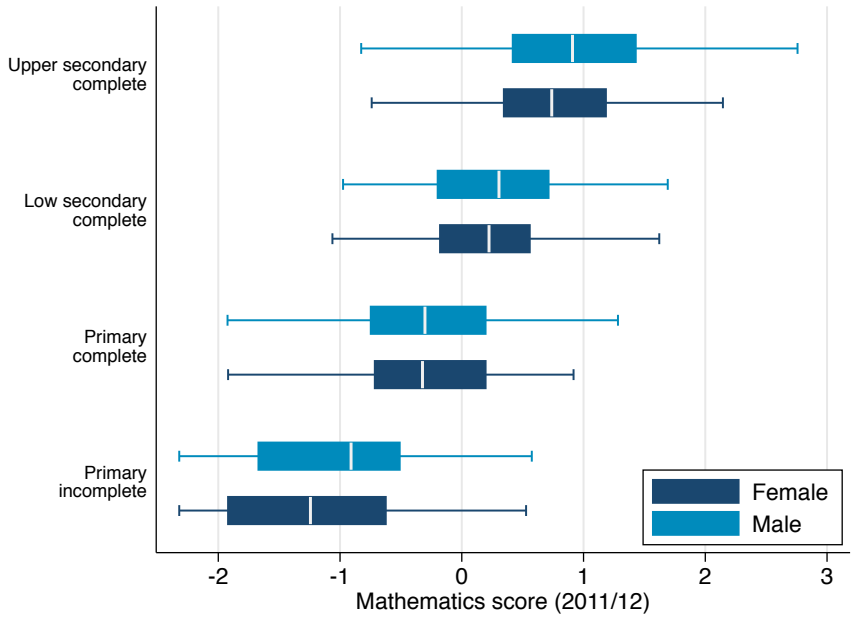


Figure 2. Distribution of the mathematics test score at adulthood by level of education and gender



Notes: The mathematics test score was generated using the item response theory methodology and standardized with mean 0 and standard deviation of 1. The relationship between level of education and the distribution of the French test score displays a similar pattern.

TABLES

Table 1. Summary statistics

VARIABLES	*Mean*	*Std. Dev.*	*Min*	*Max*	*Obs.*
Cohort members' characteristics					
Gender (male =1)	0.48	0.50	0	1	962
Age at endline	21.8	1.20	19	25	962
Years of schooling at baseline	5.37	2.07	0	13	962
Years of schooling at endline	8.16	3.50	1	16	962
Additional schooling between rounds	2.84	2.25	0	10	962
French score at baseline	0.01	1.00	–2.86	3.02	905
Math score at baseline	0.03	0.99	–2.65	2.83	813
French score at endline	0.03	0.98	–1.88	2.21	913
Math score at endline	0.03	0.98	–2.32	3.00	924
Height in centimeters	160.0	8.02	120	190	943
Birth order	2.21	1.32	1	9	941
Family background					
Mother's education (in years)	4.93	3.58	0	17	956
Father's education (in years)	5.54	3.90	0	17	956
Female-headed household	0.20	0.40	0	1	962
Household size at baseline	6.59	2.29	2	15	962
Shock experienced by households					
No. of unusually good harvests before 2004	0.45	0.85	0	5	962
No. of unusually bad harvests before 2004	0.43	0.82	0	5	962
No. of unusually good harvests since 2004	0.61	0.92	0	5	962
No. of unusually bad harvests since 2004	0.73	1.04	0	5	962
No. of years of good family business performance since 2004	0.26	0.73	0	5	962
No. of years of poor family business performance since 2004	0.25	0.70	0	5	962
No. of years with livestock loss since 2004	0.33	0.60	0	5	962
Community and primary schools' characteristics					
Remoteness index	2.36	1.33	1	5	948
Primary school facility and teaching material index	0.01	0.69	–1.35	1.80	962
Proportion of teachers with higher secondary education	0.54	0.28	0	1	962
Presence of a high school in the community	0.43	0.50	0	1	962
Number of years since the first primary school was built	45.9	23.4	2.5	102	962

Table 2. Dynamics of education outcomes between survey rounds

	WHOLE SAMPLE		*FEMALES*		*MALES*	
	Baseline	*Endline*	*Baseline*	*Endline*	*Baseline*	*Endline*
Age	14.6	21.8	14.6	21.8	14.6	21.8
% enrolled at school	0.83	0.23	0.82	0.22	0.86	0.26
% completed primary	0.70	0.81	0.71	0.81	0.68	0.82
% completed low secondary	0.05	0.51	0.05	0.50	0.05	0.51
% completed high secondary	0.00	0.28	0.00	0.26	0.00	0.30
Observations	962		505		457	

Table 3. Progression through grades from adolescence to adulthood by family background

	Years of schooling		*Additional schooling*	*Obs.*
	2004/05	*2011*		
Sample mean	5.37	8.16	2.84	962
Standard deviation	2.07	3.50	2.25	
Gender				
Female	5.47	8.08	2.66	505
Male	5.26	8.24	3.03	457
Wealth index quartiles (at baseline)				
1 (poorest households)	3.69	5.24	1.64	219
2	5.17	7.83	2.69	241
3	5.68	8.66	3.01	249
4 (wealthiest households)	6.66	10.46	3.83	253
Mother's education				
No education	3.88	5.70	1.89	132
Some primary	4.87	7.15	2.35	335
Completed primary	5.30	7.99	2.72	162
Some lower secondary	5.94	9.26	3.34	132
Completed lower secondary and above	6.89	10.91	4.06	195
Standardized adult height quartiles				
1 (shorter individuals)	4.95	7.43	2.52	258
2	5.38	8.23	2.93	247
3	5.55	8.43	2.93	230
4 (taller individuals)	5.64	8.64	3.01	227
Remoteness index (at baseline)				
1 (less remote)	6.11	9.41	3.34	350
2	5.79	8.74	2.98	212
3	4.94	7.21	2.33	150
4	4.67	7.23	2.62	163
5 (more remote)	3.73	5.30	1.63	73
Baseline French score quartile				
1 (lowest score)	3.81	5.48	1.79	221
2	5.04	7.44	2.43	225
3	6.07	9.39	3.34	228
4 (higher score)	6.90	10.73	3.86	231

Notes: Additional schooling is the number of additional years of schooling achieved between the two survey rounds. Wealth quartiles are ranked from the poorest (1) to the wealthiest. Standardized adult height quartiles ranks individuals from the shorter ones (1) to the taller ones (4). The remoteness index ranks households from the less remote (1) to the more remote (5). Baseline French score quartiles ranks from the lowest score (1) to the higher score.

Table 4. Percentage of correct answers to three sample questions from the tests

% of correct answers	*Sample questions*				*Obs.*
	20/4	*26 – (12 + 6)*	*Five words sentence in the right order*	*Correct information extracted from a paragraph*	
By level of education at adulthood					
Not completed primary school	0.31	0.27	0.21	0.14	150
Completed primary school	0.59	0.36	0.54	0.33	278
Completed low secondary school	0.72	0.54	0.86	0.46	218
Completed high school and above	0.91	0.72	0.99	0.84	267
Total	0.66	0.48	0.68	0.46	913

Notes: The first question was given orally. The remaining ones are from the written part of the test. Both mathematics questions were formulated as problems within a context. The number of observations refers to the written French test.

Table 5. Baseline and endline test scores by family background

	BASELINE		*ENDLINE*	
	French	*Math*	*French*	*Math*
IRT scores by:				
Wealth index quartiles (at baseline)				
1 (Poorest households)	-0.66	-0.82	-0.79	-0.77
2	-0.21	-0.11	-0.19	-0.12
3	0.16	0.23	0.18	0.23
4 (Wealthiest households)	0.60	0.56	0.68	0.57
Mother's education				
No education	-0.60	-0.60	-0.64	-0.65
Some primary	-0.23	-0.23	-0.27	-0.27
Completed primary	-0.11	-0.09	-0.06	0.04
Some lower secondary	0.29	0.30	0.26	0.32
Completed lower sec. and above	0.68	0.65	0.75	0.64
Standardized adult height quartiles				
1 (shorter individuals)	-0.14	-0.08	-0.14	-0.15
2	0.00	-0.02	-0.08	-0.05
3	0.09	-0.02	0.06	0.08
4 (taller individuals)	0.07	0.13	0.19	0.14
Remoteness index (at baseline)				
1 (Less remote)	0.39	0.39	0.31	0.28
2	0.17	0.20	0.23	0.24
3	-0.41	-0.40	-0.26	-0.31
4	-0.49	-0.52	-0.24	-0.20
5 (more remote)	-0.46	-0.58	-1.07	-0.84
Observations	936	844	950	961

Notes: Wealth quartiles are ranked from the poorest (1) to the wealthiest. Standardized adult height quartiles ranks individuals from the shorter ones (1) to the taller ones (4). The remoteness index ranks households from the less remote (1) to the more remote (5).

Table 6. Cross-sectional and value-added model of cognitive achievement for math and French / OLS

	CROSS-SECTIONAL MODEL		*VALUE-ADDED MODEL*	
	FRENCH	*MATH*	*FRENCH*	*MATH*
VARIABLES	*(1)*	*(2)*	*(3)*	*(4)*
Years of schooling	0.173***	0.165***		
	(0.009)	(0.012)		
Additional schooling			0.163***	0.148***
			(0.012)	(0.014)
Lagged score			0.200***	0.248***
			(0.037)	(0.040)
Gender (male=1)	-0.005	0.118**	-0.008	0.088*
	(0.043)	(0.046)	(0.049)	(0.050)
Mother's education	0.008	0.017*	0.014*	0.024**
	(0.006)	(0.009)	(0.007)	(0.010)
Father's education	0.012	-0.004	0.021***	0.006
	(0.007)	(0.009)	(0.007)	(0.009)
Household wealth quartile (2nd)	0.020	0.049	0.081	0.081
	(0.080)	(0.084)	(0.090)	(0.089)
Household wealth quartile (3rd)	0.136	0.179*	0.192*	0.192*
	(0.094)	(0.103)	(0.101)	(0.103)
Household wealth quartile (4th)	0.219**	0.263**	0.356***	0.358***
	(0.106)	(0.105)	(0.112)	(0.101)
Female headed household (baseline)	0.124**	0.109*	0.071	0.053
	(0.055)	(0.062)	(0.060)	(0.062)
Household size (baseline)	-0.007	0.003	-0.020	-0.009
	(0.012)	(0.014)	(0.012)	(0.015)
Birth order	0.008	-0.016	0.026	0.009
	(0.020)	(0.020)	(0.020)	(0.021)
Good harvests before baseline	0.083**	0.065*	0.072	0.073*
	(0.039)	(0.039)	(0.044)	(0.043)
Bad harvests before baseline	-0.096***	-0.054	-0.098**	-0.063
	(0.034)	(0.043)	(0.037)	(0.045)
Good harvests since baseline	-0.003	-0.007	-0.007	-0.003
	(0.026)	(0.036)	(0.028)	(0.037)
Bad harvests since baseline	-0.010	0.036	-0.014	0.031
	(0.023)	(0.024)	(0.024)	(0.025)
Good years family business since baseline	0.054*	-0.003	0.044	-0.031
	(0.030)	(0.033)	(0.033)	(0.040)
Bad years family business since baseline	0.010	-0.036	0.009	-0.030
	(0.027)	(0.027)	(0.029)	(0.028)
Livestock loss since baseline	0.000	-0.034	0.019	-0.022
	(0.034)	(0.042)	(0.039)	(0.041)
Constant	-1.676***	-1.614***	3.111	-3.675
	(0.125)	(0.160)	(7.524)	(9.052)
Observations	842	774	842	774
R-squared	0.499	0.429	0.413	0.372
Number of clusters	69	63	69	63
F-test	69.91	27.11	32.22	16.30

Notes: Standard errors (in parentheses) are clustered at the community level. Dependent variables are French and math IRT scores, standardized with mean 0 and standard deviation of 1. The first two columns display OLS estimates of the cross-sectional model. Columns (3) and (4) display OLS estimates of the value-added model. Both models are estimated with community fixed effects. Household wealth refers to the household where a cohort member was living at baseline. Wealth quartiles are ranked from the poorest (first quartile, omitted) to the wealthiest (fourth quartile). Control variables not shown are adolescent's age and number of months between the two rounds of surveys (in the VAM model).
Significance: *** p < 0.01, ** p < 0.05, * p < 0.1

Table 7. Alternative specifications of the value-added model OLS estimates (French score)

	FRENCH SCORE (OLS)				
VARIABLES	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
Additional schooling	0.179***	0.166***	0.163***	0.163***	0.162***
	(0.012)	(0.012)	(0.013)	(0.012)	(0.012)
Lagged score	0.258***	0.210***	0.198***	0.200***	0.199***
	(0.037)	(0.038)	(0.037)	(0.037)	(0.038)
Gender (male=1)	-0.007	-0.007	-0.009	-0.008	-0.009
	(0.048)	(0.049)	(0.049)	(0.049)	(0.049)
Mother's education		0.018**	0.013*	0.014*	0.015**
		(0.007)	(0.007)	(0.007)	(0.007)
Father's education		0.028***	0.022***	0.021***	0.021***
		(0.007)	(0.007)	(0.007)	(0.007)
Household wealth quartile (2nd)			0.081	0.081	0.079
			(0.094)	(0.090)	(0.090)
Household wealth quartile (3rd)			0.191*	0.192*	0.190*
			(0.101)	(0.101)	(0.100)
Household wealth quartile (4th)			0.351***	0.356***	0.349***
			(0.113)	(0.112)	(0.110)
Female headed household (baseline)			0.074	0.071	0.071
			(0.061)	(0.060)	(0.061)
Household size (baseline)			-0.020	-0.020	-0.019
			(0.012)	(0.012)	(0.012)
Birth order			0.023	0.026	0.025
			(0.020)	(0.020)	(0.020)
Good harvests before baseline				0.072	0.072
				(0.044)	(0.044)
Bad harvests before baseline				-0.098**	-0.097**
				(0.037)	(0.038)
Good harvests since baseline				-0.007	-0.007
				(0.028)	(0.028)
Bad harvests since baseline				-0.014	-0.012
				(0.024)	(0.024)
Good years family business since baseline				0.044	0.043
				(0.033)	(0.034)
Bad years family business since baseline				0.009	0.007
				(0.029)	(0.028)
Livestock loss since baseline				0.019	0.019
				(0.039)	(0.039)
Standardized height					0.020
					(0.028)
Constant	6.059	3.776	3.518	3.111	3.446
	(7.665)	(7.884)	(7.394)	(7.524)	(7.487)
Observations	842	842	842	842	842
R-squared	0.361	0.386	0.405	0.413	0.414
Number of clusters	69	69	69	69	69
F-test	50.77	50.18	38.50	32.22	32.19

Notes: Ordinary Least Squares estimates with community fixed effects. Standard errors (in parentheses) are clustered at the community level. Dependent variable is the French IRT score, standardized with mean 0 and standard deviation of 1. Household wealth refers to the household where a cohort member was living at baseline. Wealth quartiles are ranked from the poorest (first quartile, omitted) to the wealthiest (fourth quartile). Control variables not shown are adolescent's age dummies and number of months between the two rounds of surveys.
Significance: *** p < 0.01, ** p < 0.05, * p < 0.1

Table 8. Alternative specifications of the value-added model OLS estimates (math score)

	MATH SCORE (OLS)				
VARIABLES	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
Additional schooling	0.165***	0.151***	0.149***	0.148***	0.148***
	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)
Lagged score	0.288***	0.254***	0.244***	0.248***	0.248***
	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)
Gender (male=1)	0.100**	0.102**	0.094*	0.088*	0.088*
	(0.048)	(0.048)	(0.047)	(0.050)	(0.050)
Mother's education		0.029***	0.024**	0.024**	0.024**
		(0.010)	(0.010)	(0.010)	(0.010)
Father's education		0.014	0.007	0.006	0.006
		(0.009)	(0.009)	(0.009)	(0.009)
Household wealth quartile (2nd)			0.078	0.081	0.081
			(0.093)	(0.089)	(0.089)
Household wealth quartile (3rd)			0.192*	0.192*	0.192*
			(0.105)	(0.103)	(0.103)
Household wealth quartile (4th)			0.361***	0.358***	0.357***
			(0.100)	(0.101)	(0.100)
Female headed household (baseline)			0.051	0.053	0.053
			(0.063)	(0.062)	(0.062)
Household size (baseline)			-0.011	-0.009	-0.009
			(0.015)	(0.015)	(0.015)
Birth order			0.010	0.009	0.009
			(0.020)	(0.021)	(0.021)
Good harvests before baseline				0.073*	0.073*
				(0.043)	(0.043)
Bad harvests before baseline				-0.063	-0.063
				(0.045)	(0.045)
Good harvests since baseline				-0.003	-0.003
				(0.037)	(0.037)
Bad harvests since baseline				0.031	0.031
				(0.025)	(0.025)
Good years family business since baseline				-0.031	-0.031
				(0.040)	(0.040)
Bad years family business since baseline				-0.030	-0.030
				(0.028)	(0.028)
Livestock loss since baseline				-0.022	-0.022
				(0.041)	(0.041)
Standardized height					0.002
					(0.025)
Constant	-3.630	-5.224	-4.539	-3.675	-3.637
	(9.873)	(9.676)	(9.307)	(9.052)	(8.961)
Observations	774	774	774	774	774
R-squared	0.332	0.352	0.366	0.372	0.372
Number of clusters	63	63	63	63	63
F-test	31.16	28.10	21.18	16.30	15.63

Notes: Ordinary Least Squares estimates with community fixed effects. Standard errors (in parentheses) are clustered at the community level. Dependent variable is the math IRT score, standardized with mean 0 and standard deviation of 1. Household wealth refers to the household where a cohort member was living at baseline. Wealth quartiles are ranked from the poorest (first quartile, omitted) to the wealthiest (fourth quartile). Control variables not shown are adolescent's age dummies and number of months between the two rounds of surveys.
Significance: *** p < 0.01, ** p < 0.05, * p < 0.1

Table 9. Robustness to omitted variable bias

	FRENCH SCORE *(1)*	*MATH SCORE* *(2)*
Baseline effect		
Additional schooling	0.179***	0.165***
	(0.012)	(0.013)
R-squared	[0.361]	[0.332]
Controlled effect		
Additional schooling	0.163***	0.148***
	(0.012)	(0.014)
R-squared	[0.413]	[0.372]
Identified set	[0.117,0.163]	[0.089,0.148]
δ for β=0	2.06	1.64

Notes: Standard errors (in parentheses) are clustered at the community level. Results of the control and baseline models are from OLS regressions with community fixed effects. Baseline effects only include controls for gender, age, lagged score and number of months between the two rounds of surveys. The full controls include parents' education, household wealth quartile dummies, gender of household head, household size, birth order and economic shocks. The identified set includes the value of β in the controlled model (upper bound) and the value of β calculated for $R_{max} = 1.3R^2$, under the assumption that selection on observables and unobservables is proportional. Results are computed using the Stata command psacalc provided by Oster (2017).
Significance: *** p < 0.01, ** p < 0.05, * p < 0.1

Table 10. 2SLS estimates of the value-added model of cognitive achievement for French

	FRENCH SCORE (2SLS)				
VARIABLES	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
Additional schooling	0.291***	0.281***	0.264**	0.237**	0.233**
	(0.109)	(0.106)	(0.109)	(0.108)	(0.107)
Lagged score	0.238***	0.195***	0.180**	0.193***	0.192***
	(0.092)	(0.075)	(0.072)	(0.072)	(0.072)
Covariates included					
Gender and age	Yes	Yes	Yes	Yes	Yes
Parents' education		Yes	Yes	Yes	Yes
Family background			Yes	Yes	Yes
Shocks				Yes	Yes
Standardized height					Yes
Observations	827	827	827	827	827
R-squared	0.537	0.555	0.583	0.606	0.609
F-test	40.10	47.17	48.39	49.04	49.99
F-test of excluded instruments	15.48	13.90	10.74	11.18	10.91
Kleibergen-Paap test stat.	15.52	13.84	12.20	12.30	12.11
P-value Hansen test	0.288	0.255	0.551	0.453	0.429

Notes: Two-Stages Least Squares estimates. Standard errors (in parentheses) are clustered at the community level. Dependent variable is the French IRT score, standardized with mean 0 and standard deviation of 1. Household wealth refers to the household where a cohort member was living at baseline. Wealth quartiles are ranked from the poorest (first quartile, omitted) to the wealthiest (fourth quartile). Control variables not shown are adolescent's age, regional dummies; number of months between the two rounds of surveys. The instruments for additional schooling are the presence of a high school in the community at baseline and the number of years since the first primary school was built.
Significance: *** p < 0.01, ** p < 0.05, * p < 0.1

Table 11. 2SLS estimates of the value-added model of cognitive achievement for mathematics

	MATH SCORE (2SLS)				
VARIABLES	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
Additional schooling	0.281***	0.278***	0.255**	0.238**	0.237**
	(0.095)	(0.097)	(0.101)	(0.102)	(0.102)
Lagged score	0.245***	0.219***	0.206***	0.218***	0.218***
	(0.083)	(0.070)	(0.068)	(0.069)	(0.069)
Covariates included					
Gender and age	Yes	Yes	Yes	Yes	Yes
Parents' education		Yes	Yes	Yes	Yes
Family background			Yes	Yes	Yes
Shocks				Yes	Yes
Standardized height					Yes
Observations	757	757	757	757	757
R-squared	0.473	0.481	0.515	0.531	0.532
F-test	36.43	40.74	50.24	49.55	49.74
F-test of excluded instruments	16.01	17.05	14.76	17.03	16.79
Kleibergen-Paap test stat.	15.59	14.81	14.78	15.79	15.42
P-value Hansen test	0.332	0.278	0.496	0.528	0.519

Notes: Two-Stages Least Squares estimates. Standard errors (in parentheses) are clustered at the community level. Dependent variable is the math IRT score, standardized with mean 0 and standard deviation of 1. Household wealth refers to the household where a cohort member was living at baseline. Wealth quartiles are ranked from the poorest (first quartile, omitted) to the wealthiest (fourth quartile). Control variables not shown are adolescent's age, regional dummies; number of months between the two rounds of surveys. The instruments for additional schooling are the presence of a high school in the community at baseline and the number of years since the first primary school was built.
Significance: *** p < 0.01, ** p < 0.05, * p < 0.1

Table 12. Value-added model of cognitive achievement for math and French with additional schooling by level

VARIABLES	*FRENCH SCORE (1)*	*MATH SCORE (2)*
Additional schooling (prim. school)	-0.034	0.000
	(0.035)	(0.045)
Additional schooling (low secondary)	0.130***	0.127***
	(0.017)	(0.025)
Additional schooling (high secondary)	0.225***	0.184***
	(0.020)	(0.022)
Additional schooling (higher educ.)	0.308***	0.280***
	(0.053)	(0.064)
Lagged score	0.128***	0.198***
	(0.034)	(0.041)
Constant	-0.614	-6.763
	(7.150)	(9.251)
Observations	842	774
R-squared	0.452	0.391
Number of clusters	69	63
F-test	43.10	20.63

Notes: Ordinary Least Squares estimates with community fixed effects. Standard errors (in parentheses) are clustered at the community level. Dependent variables are French and math IRT scores, standardized with mean 0 and standard deviation of 1. Both estimates include covariates used in the specification presented in table 6.
Significance: *** p < 0.01, ** p < 0.05, * p < 0.1

Table 13. Value-added model of cognitive achievement for French, with interactions

	FRENCH SCORE (OLS)				
VARIABLES	(1)	(2)	(3)	(4)	(5)
Additional schooling	0.202***	0.198***	0.164***	0.167***	0.163***
	(0.020)	(0.021)	(0.011)	(0.019)	(0.012)
Lagged score	0.198***	0.195***	0.196***	0.195***	0.193***
	(0.039)	(0.039)	(0.039)	(0.039)	(0.039)
Gender (male=1)	-0.011	-0.009	-0.005	0.018	-0.008
	(0.049)	(0.049)	(0.049)	(0.082)	(0.049)
Mother's education	0.043***	0.019**	0.016**	0.017**	0.017**
	(0.011)	(0.007)	(0.007)	(0.008)	(0.008)
Father's education	0.027***	0.047***	0.026***	0.026***	0.025***
	(0.008)	(0.012)	(0.008)	(0.008)	(0.008)
Household assets index	0.084**	0.093**	0.159*	0.083**	0.080**
	(0.039)	(0.039)	(0.089)	(0.039)	(0.040)
Standardized height					0.022
					(0.041)
Additional schooling * Mother's education	-0.008***				
	(0.003)				
Additional schooling * Father's education		-0.006**			
		(0.003)			
Additional schooling * Household assets index			-0.019		
			(0.017)		
Additional schooling * Gender				-0.009	
				(0.022)	
Additional schooling * Standardized height					0.000
					(0.011)
Constant	2.886	2.303	1.897	1.754	2.167
	(7.607)	(7.485)	(7.640)	(7.625)	(7.647)
Observations	842	842	842	842	842
R-squared	0.410	0.409	0.407	0.404	0.405
Number of clusters	69	69	69	69	69
F-test	31.31	33.57	33.45	31.18	30.59

Notes: Ordinary Least Squares estimates with community fixed effects. Standard errors (in parentheses) are clustered at the community level. Dependent variable is the French IRT scores, standardized with mean 0 and standard deviation of 1. The household assets index is a continuous variable. Control variables not shown are adolescent's age, number of months between the two rounds of surveys, female headed household dummy, household size, birth order and shocks.
Significance: *** p < 0.01, ** p < 0.05, * p < 0.1

Table 14. Value-added model of cognitive achievement for math, with interactions

	MATH SCORE (OLS)				
VARIABLES	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
Additional schooling	0.206***	0.223***	0.149***	0.144***	0.147***
	(0.022)	(0.023)	(0.013)	(0.017)	(0.014)
Lagged score	0.254***	0.244***	0.247***	0.250***	0.252***
	(0.040)	(0.040)	(0.040)	(0.041)	(0.041)
Gender (male=1)	0.095*	0.090*	0.102**	0.086	0.101*
	(0.049)	(0.049)	(0.051)	(0.096)	(0.051)
Mother's education	0.070***	0.031***	0.026**	0.027**	0.027**
	(0.015)	(0.010)	(0.010)	(0.010)	(0.010)
Father's education	0.011	0.057***	0.009	0.009	0.009
	(0.009)	(0.014)	(0.009)	(0.009)	(0.009)
Household assets index	0.115***	0.131***	0.282***	0.110***	0.113***
	(0.037)	(0.037)	(0.060)	(0.039)	(0.039)
Standardized height					0.026
					(0.035)
Additional schooling * Mother's education	-0.012***				
	(0.003)				
Additional schooling * Father's education		-0.014***			
		(0.004)			
Additional schooling * Household assets index			-0.041***		
			(0.012)		
Additional schooling * Gender				0.004	
				(0.026)	
Additional schooling * Standardized height					-0.010
					(0.012)
Constant	-3.188	-4.483	-2.994	-4.573	-4.620
	(9.722)	(9.525)	(9.134)	(9.346)	(9.026)
Observations	774	774	774	774	774
R-squared	0.380	0.388	0.378	0.367	0.368
Number of clusters	63	63	63	63	63
F-test	16.43	19.22	17.27	18.37	15.96

Notes: Ordinary Least Squares estimates with community fixed effects. Standard errors (in parentheses) are clustered at the community level. Dependent variable is the math IRT scores, standardized with mean 0 and standard deviation of 1. The household assets index is a continuous variable. Control variables not shown are adolescent's age, number of months between the two rounds of surveys, female headed household dummy, household size, birth order and shocks.
Significance: *** p < 0.01, ** p < 0.05, * p < 0.1

APPENDIX

Table A.1. First stage – Value-added model

Dependent variable: Additional schooling	*French score model (1)*	*Math score model (2)*
Presence of a high school at baseline (dummy)	0.359**	0.519***
	(0.167)	(0.157)
Presence of primary school (years)	0.009***	0.010***
	(0.002)	(0.002)
Lagged score	0.462***	0.484***
	(0.095)	(0.107)
Gender (male =1)	0.421***	0.364***
	(0.121)	(0.131)
Mother's education	0.044*	0.087***
	(0.025)	(0.026)
Father's education	0.070**	0.070**
	(0.026)	(0.028)
Household wealth quartile (2nd)	0.630***	0.454**
	(0.194)	(0.205)
Household wealth quartile (3rd)	0.513*	0.273
	(0.275)	(0.273)
Household wealth quartile (4th)	0.798**	0.508
	(0.304)	(0.318)
Female headed household (baseline)	-0.269	-0.244
	(0.180)	(0.193)
Household size (baseline)	-0.027	-0.018
	(0.041)	(0.044)
Birth order	0.142**	0.193**
	(0.071)	(0.079)
Good harvests before baseline	-0.101	-0.086
	(0.106)	(0.105)
Bad harvests before baseline	-0.006	0.076
	(0.137)	(0.123)
Good harvests since baseline	-0.131*	-0.143*
	(0.072)	(0.078)
Bad harvests since baseline	0.113	0.145
	(0.095)	(0.095)
Good years family business since baseline	-0.082	-0.173
	(0.148)	(0.143)
Bad years family business since baseline	-0.102	-0.003
	(0.128)	(0.117)
Livestock loss since baseline	0.034	-0.039
	(0.126)	(0.156)
Remoteness index (baseline)	-0.014	-0.056
	(0.071)	(0.073)
Primary school infrastructure quality	0.075	0.018
	(0.133)	(0.111)
Primary school teachers' education	-0.091	0.011
	(0.226)	(0.249)
Peers' average gain in score	0.242	0.219
	(0.179)	(0.154)
Constant	0.002	1.199
	(2.712)	(2.551)
Observations	827	757
R-squared	0.302	0.323
F-test	33.080	31.100
F-test of excluded instruments	11.18	17.03

Notes: Standard errors (in parentheses) are clustered at the community level. Dependent variable is additional schooling between survey rounds. The table displays first stage results for the French score model (column 1) and the math score model (column 2). Household wealth refers to the household where a cohort member was living at baseline. Wealth quartiles are ranked from the poorest (first quartile, omitted) to the wealthiest (fourth quartile). Control variables not shown are adolescent's age, regional dummies; number of months between the two rounds of surveys.
Significance: *** p < 0.01, ** p < 0.05, * p < 0.1

Table A.2. Determinants of additional schooling at adolescence

	Dependent variable: Additional schooling			
VARIABLES	(1)	(2)	(3)	(4)
Lagged score (French)	0.441***	0.528***		
	(0.113)	(0.127)		
Lagged score (math)			0.392***	0.458***
			(0.109)	(0.132)
Years of education at baseline		-0.108		-0.082
		(0.067)		(0.071)
Gender (male=1)	0.421***	0.410***	0.380***	0.363***
	(0.116)	(0.112)	(0.129)	(0.126)
Age at baseline	-0.513***	-0.497***	-0.509***	-0.495***
	(0.063)	(0.061)	(0.065)	(0.065)
Number of months between survey rounds	0.519*	0.488*	0.600*	0.557*
	(0.283)	(0.281)	(0.314)	(0.311)
Mother's education	0.033	0.039	0.081***	0.086***
	(0.026)	(0.025)	(0.028)	(0.028)
Father's education	0.085***	0.091***	0.078***	0.083***
	(0.026)	(0.026)	(0.027)	(0.028)
Household wealth quartile (2nd)	0.595**	0.631***	0.437*	0.460*
	(0.225)	(0.227)	(0.234)	(0.235)
Household wealth quartile (3rd)	0.424	0.462	0.202	0.226
	(0.299)	(0.305)	(0.285)	(0.287)
Household wealth quartile (4th)	0.648**	0.731**	0.400	0.460
	(0.318)	(0.319)	(0.333)	(0.334)
Female headed household (baseline)	-0.202	-0.227	-0.180	-0.200
	(0.182)	(0.181)	(0.195)	(0.196)
Household size (baseline)	-0.024	-0.031	-0.020	-0.026
	(0.042)	(0.043)	(0.047)	(0.048)
Birth order	0.136*	0.145**	0.179**	0.190**
	(0.069)	(0.069)	(0.077)	(0.078)
Good harvests before baseline	-0.049	-0.057	-0.061	-0.060
	(0.109)	(0.107)	(0.108)	(0.108)
Bad harvests before baseline	-0.055	-0.054	0.093	0.091
	(0.135)	(0.134)	(0.127)	(0.127)
Good harvests since baseline	-0.129	-0.129	-0.123	-0.119
	(0.081)	(0.082)	(0.085)	(0.085)
Bad harvests since baseline	0.094	0.091	0.136	0.131
	(0.090)	(0.089)	(0.100)	(0.099)
Good years family business since baseline	-0.069	-0.075	-0.172	-0.183
	(0.145)	(0.148)	(0.137)	(0.139)
Bad years family business since baseline	-0.076	-0.078	0.034	0.034
	(0.140)	(0.139)	(0.109)	(0.109)
Livestock loss since baseline	0.046	0.057	0.055	0.058
	(0.138)	(0.136)	(0.163)	(0.162)
Constant	-36.929	-33.894	-44.376	-40.378
	(25.003)	(24.817)	(27.724)	(27.452)
Observations	842	842	774	774
R-squared	0.195	0.200	0.212	0.215
Number of clusters	69	69	63	63
F-test	16.07	16.50	16.32	16.22

Notes: Ordinary Least Squares estimates with community fixed effects. Standard errors (in parentheses) are clustered at the community level. Dependent variable is the number of additional schooling since the baseline survey.
Significance: *** p < 0.01, ** p < 0.05, * p < 0.1