# Preschool Attendance, Schooling, and Cognitive Skills in East Africa*

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**Abstract**

We study the effects of preschool attendance on children's schooling and cognitive skills in Kenya and Tanzania. We use a within-household estimator and data from nationally representative surveys of school-age children's literacy and numeracy skills, which include retrospective information on preschool attendance. In both countries, school entry rules are not strictly enforced, and children who attend preschool often start primary school late. At ages 7-9, these children have thus attended fewer school grades than their same-aged peers without pre-primary education. However, they catch up over time: at ages 13-16, children who went to preschool have attended about the same number of school grades and score about 0.10 standard deviations higher on standardized tests in both countries. They are also 3 (5) percentage points more likely to achieve basic literacy and numeracy in Kenya (Tanzania).

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## 1. Introduction

School enrollment in Sub-Saharan Africa has increased substantially over the past two decades. However, many students fall behind the curriculum early on, and grade repetition and early dropout are widespread (UNESCO, 2012). Students also learn remarkably little in school: for example, only one in five third-grade students in East Africa has second-grade literacy and numeracy skills, and less than one third of sixth-grade students in Southern and Eastern Africa can solve a simple subtraction problem (Uwezo, 2015; Bietenbeck, Piopiunik, and Wiederhold, 2018).

One possible reason why students in these countries perform so poorly is that they enter school unprepared. Specifically, to the extent that early and later learning are complementary, a lack of education before starting school reduces children's efficiency in learning once they arrive there (Cunha and Heckman, 2007). High-quality preschool programs which prepare children for school are therefore often seen as a promising way to enhance learning outcomes (e.g. World Bank, 2018). In Sub-Saharan Africa, pre-primary education has been expanding rapidly, with the gross enrollment ratio doubling from 15% to 32% between 2000 and 2017.[1] Whether preschools are actually effective at boosting student outcomes in this region is unclear, however, because rigorous empirical evidence is still scarce.

In this paper, we study the effects of preschool attendance on children's schooling and cognitive skills in Kenya and Tanzania. Our empirical analysis draws on data from Uwezo, which conducts nationally representative household surveys of school-age children's education and their literacy and numeracy skills. The surveys also collect retrospective information on preschool attendance, which we can relate to current outcomes of respondents up to 16 years of age. The main part of our investigation focuses on impacts on the highest grade of school attended and a composite test score, which summarizes a child's performance on the standardized literacy and numeracy assessments. The data contain information on these outcomes for more than half a million children across the two countries, independently of whether they are currently enrolled in school or not.

[1] These figures were retrieved from the World Development Indicators (https://data.worldbank.org/indicator/SE.PRE.ENRR) on June 19, 2019.

Our regression framework compares the outcomes of children who did and did not attend preschool. For identification, we rely on within-household differences, thereby controlling for all determinants of outcomes and attendance that vary across families.[2] We argue that the leftover variation between siblings is likely due to changes in the local availability of preschools, which came about because of an expansion of the pre-primary sector during our study period. In support of this claim, we show that even within households, children in later cohorts are much more likely to have attended preschool. To mitigate concerns about endogenous selection from the start, the regressions also control for a variety of predetermined characteristics that still vary between siblings.

The impact of preschool attendance on schooling follows an interesting dynamic pattern. In both Kenya and Tanzania, school entry rules are not strictly enforced, and for reasons discussed below, children who attend preschool often start primary school late. At ages 7-9, these children thus have attended fewer school grades than their same-aged peers without pre-primary education. However, once enrolled in primary school, children who attended preschool progress through grades faster and are less likely to drop out. Eventually, in both countries, they thus catch up and at ages 13-16 have attended about the same number of school grades as their peers.

In terms of cognitive skills, the estimates for the composite test score show that children who went to preschool outperform their peers in the long run. In Kenya, this effect fades in at early ages and soon stabilizes at a gain of about 0.10 standard deviations (SD). In Tanzania, in contrast, children who attended preschool outperform their peers from the age of school start, with long-term gains of the same order of magnitude as in Kenya. Separate regressions moreover reveal that among 13- to 16-year-olds, preschool attendance raises the likelihood of mastering basic, second-grade literacy and numeracy by 3 percentage points in Kenya and by 5 percentage points in Tanzania. Taken together, these findings show that

[2]Similar strategies have been used by Currie and Thomas (1995), Garces, Thomas, and Currie (2002), and Deming (2009) to estimate the impacts of the Head Start program in the United States, and by Berlinski, Galiani, and Manacorda (2008) to estimate the effects of preschool in Uruguay. Throughout the paper, we use the terms "within-household differences" and "between-sibling differences" interchangeably.

there are important benefits from preschool attendance in both countries.

To ensure that these results are not driven by selection, we perform a variety of falsification tests and robustness checks. For example, we show that children who attended preschool do not differentially benefit from other educational inputs such as private tutoring, suggesting that our findings are not due to child-specific investments related to unobserved characteristics. Applying the method developed by Oster (2017), we also judge the importance of omitted variable bias more generally by observing the sensitivity of our regression results to the addition of controls. From this approach, selection on unobserved factors would need to be at least four times as large as selection on observed factors to explain away the long-term impacts on the composite test score.

Our paper contributes to a growing literature on the impacts of preschool education on children's outcomes in developing countries, which has focused mostly on Asia and Latin America and which is reviewed in detail in Nores and Barnett (2010) and in Rao et al. (2014). Using retrospective data on preschool enrollment and the same within-household estimator as we do, Berlinski, Galiani, and Manacorda (2008) find that Uruguayan children who attended preschool accumulate 0.8 more years of education by age 15. Applying the same strategy to Egyptian data, Krafft (2015) finds that preschool attendance leads to an additional 0.4 years of schooling among 18-29 year-olds. Behrman, Cheng, and Todd (2004), Berlinski, Galiani, and Gertler (2009), and Brinkman et al. (2017) similarly document positive short-term effects of preschool attendance on children's cognitive skills in Bolivia, Argentina, and Indonesia, respectively. In contrast, a randomized evaluation of a preschool construction program in Cambodia found negative short-term impacts on test scores of targeted children, a result that is partly explained by a shift from underage enrollment in primary school to enrollment in preschool (Bouguen et al., 2018).[3]

To the best of our knowledge, the only other rigorous study of preschool effects in Sub-Saharan Africa is the paper by Martinez, Naudeau, and

[3]Zuilkowski et al. (2012), Hazarika and Viren (2013), Cortázar (2015), and Aboud, Proulx, and Asrilla (2016) also evaluate the impacts of preschool education on children's learning outcomes in developing countries. Unlike our analysis, these studies do not control for selection into preschool based on unobserved factors.

Pereira (2013), who report on an experimental evaluation of a model preschool program in the Gaza province of Mozambique. The authors find that two years after the start of the program, children were more likely to be enrolled in primary school and had higher cognitive and socio-emotional skills. In contrast to this small-scale evaluation, our study uses nationally representative data on preschool attendance and learning outcomes from two countries. Moreover, unlike most of the previous literature, we are able to examine the longer-term effects of preschool attendance.

## 2. Institutional background

### *2.1. Education and preschools in Kenya*

Basic education in Kenya consists of three years of preschool, eight years of primary school, and four years of secondary school.[4] Preschool comprises three distinct grades – baby class (ages 3-4), nursery (ages 4-5), and pre-unit (ages 5-6) –, even though in practice children of different ages are often taught together in the same classroom. Attendance is not compulsory, and children who do not go to preschool typically stay home to help with household chores instead (Daniel, 2012). In the year after they turn six, all children are supposed to enter primary school, although this rule is not strictly enforced and in practice many children enter primary school late. Primary school has been free of charge since fees were abolished in 2003. At the end of primary school, students take a national leaving exam, which largely determines which secondary schools they can enter.

There are two broad types of preschools in Kenya. First, public preschools are run by the government and are usually attached to a primary school. In rural areas, these preschools are often the only available option. Second, private preschools are owned and run by a variety of providers, including non-governmental and faith-based organizations, community-based associations, and private-for-profit agents. They comprise a diverse range of institutions, including: highly unregulated and unregistered non-formal preschools, which are mainly located in informal urban settlements; formal

[4]The following description of the preschool and school systems in Kenya draws on Tooley, Dixon, and Stanfield (2008), Bidwell, Parry, and Watine (2013), Heyneman and Stern (2014), Edwards Jr., Klees, and Wildish (2015), and Ngware et al. (2016).

private preschool academies in middle and high-income areas; and a small number of exclusive private preschools catering to very high-income households. All preschools (both public and private) charge tuition fees, which vary widely depending on the type of institution attended, with non-formal private preschools charging the lowest fees.

Unlike in many European countries and the United States, where pre-primary education for younger children typically focuses on play, preschool studies in Kenya are highly academic: students sit at desks and listen to the teacher teach in a classroom-like setting. Curricula, while not standardized, tend to emphasize the learning of basic numeracy and literacy skills via memorization and recitation. In contrast, only little attention is paid to the development of socio-emotional skills (Ngware et al., 2016). As children often spend more than 35 hours per week in preschool, most institutions offer a feeding program, which is financed via the tuition fees or via special meal fees (Bidwell, Parry, and Watine, 2013).

Mirroring the diversity in institutional arrangements, the quality of preschools as perceived by parents varies widely (Bidwell, Parry, and Watine, 2013), and is considered low on average by international standards (Ngware et al., 2016). Available metrics of preschool quality show no consistent trends during our study period: on the one hand, the average student-teacher ratio in Kenya rose from 23 to 26 between 2004 and 2014 (in comparison, the ratio in OECD countries stood at 15 in 2014). On the other hand, the share of pre-primary teachers with at least some formal training rose from 70% to 82% during those years. Moreover, the 2000s were characterized by a decline in the quality of primary education, as indicated by an increasing student-teacher ratio, a decreasing share of trained teachers, and decreased government spending per student.[5]

[5]These figures were retrieved from the World Development Indicators (https://data.worldbank.org) on June 27, 2019. No data on the share of pre-primary teachers with formal training are available for OECD countries. No separate data on spending on pre-primary education are available. In primary school, the student-teacher ratio increased from 40 in 2004 to 57 in 2012, the share of trained teachers decreased from 99% in 2003 to 97% in 2009, and government expenditure per primary student decreased from 21% in 2001 to 10% in 2012 (years chosen for data availability). In the 2010s, the student-teacher ratio started decreasing and spending started increasing again, but as will become clear below, the children in our sample were in primary school mostly during the 2000s.

We end our discussion of the institutional context in Kenya by presenting some stylized facts on enrollment.[6] First, pre-primary education has expanded substantially over the last few decades, with 83% of the 2004 cohort attending preschool. Observers ascribe this increased enrollment both to a series of government efforts to increase preschool availability and quality (e.g. by training preschool teachers), and to increased demand due to more mothers entering the workforce (Nganga, 2009; Ngware et al., 2016). Second, late enrollment in primary school is common: for example, in 2013, 14% of 7-year-olds were not yet enrolled in primary school, with the vast majority still attending preschool. A key reason for this is that enrollment in pre-primary education is often late itself, and parents prefer their children to complete preschool before proceeding to primary school (Bidwell, Parry, and Watine, 2013). Third, while school enrollment is high compared to other countries in Sub-Saharan Africa, some dropout occurs, with 5% of 13-year-olds reporting not to be enrolled in school in 2013.

### *2.2. Education and preschools in Tanzania*

Basic education in Tanzania consists of two years of preschool, seven years of primary school, four years of lower ('ordinary') secondary school, and two years of upper ('advanced') secondary school.[7] Children can enter non-compulsory pre-primary education at age 5 and are supposed to start primary school in the year after they turn 7, but this rule is not strictly enforced in practice. At the end of grade 7, they take a school leaving exam which regulates access to public secondary schools. Tuition fees for primary education were abolished in 2002, but secondary schools still levied fees during our study period.

Preschools in Tanzania are predominantly public: in 2016, 95% of preschool students were enrolled in a government-run institution (Presi-

[6] All enrollment statistics in this section are based on data from the nationally representative Uwezo surveys, which we describe in detail in the next section. For the preschool figures, we focus on cohorts who were at least 10 years old in our data in order to account for the frequent late enrollment. For dropout rates, we focus on 13-year-olds, who have typically not completed primary school yet in either country. When reporting statistics for individual years, we choose 2013 as this is the latest year in which nation-wide Uwezo surveys were conducted in both Kenya and Tanzania.

[7] The following description of the preschool and school systems in Tanzania draws on Kweka, Binagi, and Kainamula (1997) and Mtahabwa and Rao (2010).

dent's Office of the United Republic of Tanzania, 2016). These are often attached to a primary school and charge varying tuition fees. Unlike in Kenya, the Tanzanian government has adopted an official preschool curriculum, which emphasizes the development of both cognitive and socio-emotional skills. In practice, however, preschool teachers often have little or no knowledge of the official curriculum and tend to focus on formal instruction in basic literacy and numeracy (Mligo, 2016).

The quality of education in Tanzanian preschools is usually described as being low (e.g. Kweka, Binagi, and Kainamula, 1997; Mtahabwa and Rao, 2010). Student-teacher ratios are high and have increased from 54 to 77 between 2004 and 2014. This reflects a shortage of teachers, due to which pre-primary students are sometimes taught together with older, primary school students in multi-grade classrooms (Mghasse and William, 2016). Countering this trend, the share of preschool teachers with formal training increased from 18% to 36% during the same period. Finally, unlike in Kenya, primary school quality as measured by the student-teacher ratio, the share of trained teachers, and government spending per student appears to have stayed roughly constant during the 2000s and early 2010s.[8]

Like for Kenya, we now present some stylized facts on enrollment. First, there has been an expansion of pre-primary education in Tanzania, with the attendance rate rising from 61% for the 1995 cohort to 69% for the 2004 cohort. This rise is spurred by the Tanzanian government, whose expansion strategy has been to attach pre-primary classrooms to existing primary schools (Kweka, Binagi, and Kainamula, 1997; Mtahabwa and Rao, 2010).[9] Second, children frequently enroll in primary school late, and some never enroll at all: in 2013, for example, 9% of 8-year-olds reported to be enrolled neither in school nor in preschool. Third, dropout during primary school is common, with 12% of 13-year-olds not enrolled in school in 2013.

[8]See footnote 5 for details on the sources of these figures.

[9]In particular, the Tanzanian government's 1995 Education and Training Policy mandated primary schools to establish pre-primary classes in partnership with local communities, with the Ministry of Education and Culture developing the pre-primary curriculum and facilitating teacher training (Mtahabwa and Rao, 2010). While local observers seem to agree that this policy has boosted the expansion of the pre-primary sector, to our knowledge no data exist on the actual number of classrooms formed or teachers trained.

## 3. Data

### *3.1. The Uwezo surveys*

The Uwezo initiative has been conducting large-scale assessments of school-age children's literacy and numeracy skills in Kenya, Tanzania, and Uganda since 2009, with more than 1.3 million children tested until 2014. The assessments are administered as part of repeated cross-sectional household surveys, which are representative at the district level. An important advantage of this design is that skills are measured also for children who are currently not enrolled in school. The surveys collect information from children aged 6-16 (7-16 in Tanzania) on their current enrollment and highest grade attended as well as on a variety of child and household characteristics. Crucially for our purposes, in recent waves respondents were also asked whether they ever attended preschool.

The literacy and numeracy assessments measure core competencies that children should have learned after two years of schooling according to the national curriculum. Literacy tests given in both English and Swahili assess the following four competencies in order of rising difficulty: (1) recognition of letters, (2) recognition of words, (3) reading a paragraph, and (4) reading a short story. Numeracy tests measure the following six competencies in order of rising difficulty: (1) counting (the number of objects on a show card), (2) recognition of numbers, (3) rank ordering of numbers, (4) addition, (5) subtraction, and (6) multiplication. A student's score on each test equals the highest competency level achieved, with a zero indicating that she did not even master the simplest skill assessed. Previous analyses of Uwezo data have shown that even many higher-grade students do not master these second-grade competencies (Jones et al., 2014; Uwezo, 2015).

### *3.2. Variable definitions*

The key explanatory variable in our regressions is an indicator for whether or not a child has attended preschool.[10] In the 2013 and 2014 waves of the Uwezo surveys, we additionally observe for how many years

[10]In the 2011 and 2012 waves, the indicator measures attendance of preschool or nursery, which includes less education-focused child care institutions. Our results are robust to excluding these waves from the sample. See Online Appendix A for an overview of the exact questions on preschool attendance asked in each country and wave.

each child attended preschool, and we use this information below to estimate effects at the intensive margin. Note that the data on preschool attendance are based on retrospectively reported information. This has the major advantage that we can estimate longer-term impacts by relating past enrollment to current outcomes. However, it comes with the drawback that we do not observe any information on the nature or quality of the preschool attended, which limits the analysis of potential mechanisms underlying our findings. Another concern with retrospectively recorded information is the possibility of recall error; specifically, if such recall error systematically depends on preschool attendance, this could bias our estimates (Garces, Thomas, and Currie, 2002). We therefore show in a robustness check that systematic recall error is unlikely to drive our results.

Our main analysis focuses on two outcomes. First, we study the highest school grade attended. We observe this variable both for children who are currently enrolled in school and for those who dropped out, with children who are still in preschool coded as having zero grades attended.[11] Because all our regressions include age dummies, this outcome is best interpreted as a measure of school progression. Second, we construct a composite test score as follows: we first standardize the English, Swahili, and numeracy scores by country, Uwezo survey wave, and age to have mean zero and standard deviation one; we then average these scores for each student and standardize the resulting composite test score again. Finally, in auxiliary analysis, we examine the effects of preschool attendance on indicators for current school enrollment and the possession of second-grade skills, as defined by achieving the highest competency level in the numeracy test and at least one of the two literacy tests.

The control variables include a variety of socio-demographic characteristics, such as age and gender, mother's education, and an index of current household wealth. Moreover, we construct two measures of early-life economic conditions at the district level from external data sources. The

[11] A previous version of this paper (Bietenbeck, Ericsson, and Wamalwa, 2017) reported results for the highest grade completed rather than the highest grade attended. The main advantage of focusing on the highest grade attended is that it allows us to distinguish between first graders and children who are still in preschool, both of which have zero grades completed. Results for both outcomes are qualitatively similar.

first measure is the log of average night light density, which is a proxy for economic activity (Henderson, Storeygard, and Weil, 2012). The second measure consists of two separate dummies for positive and negative rainfall shocks, defined as rainfall above the 80th percentile and below the 20th percentile of the long-term district mean. Rainfall shocks have been used widely as a measure of income shocks in rural economies; see Shah and Steinberg (2017) for a recent example. We allow for differential impacts of economic conditions at different ages by computing our two measures separately at each age between zero and the official school entry age for each child. We provide many more details on the construction of these and all other variables used in the empirical analysis in Online Appendix A.

### *3.3. Sample selection and descriptive statistics*

We use data from all available waves of the Uwezo surveys with information on preschool attendance. These are the 2013 and 2014 waves in Kenya and the four waves conducted between 2011 and 2014 in Tanzania. In Uganda, the only nationally representative Uwezo survey which asked about preschool attendance was conducted in 2013. Unfortunately, this key information is missing for 49% of children in the data for this wave, which led us to exclude Uganda from the analysis (preschool attendance is observed for all children in Kenya and Tanzania). We restrict our attention to children aged 7 and above in Kenya and 8 and above in Tanzania because some younger children were still of preschool age at the time of the survey. In order to ensure that we focus on comparable siblings in our within-household analysis, we also drop any children who report never to have enrolled in preschool or school. After these restrictions, our final sample includes 517,096 children across both countries, of whom 38,685 have a sibling with different preschool status.

Table 1 reports summary statistics for key variables separately for each country. Almost a fifth of children have mothers without any formal education, and more than two thirds live in rural areas. In Kenya, 85% of children attended preschool for an average length of 2.1 years, with the corresponding figures for Tanzania being 62% and 1.3 years. The vast majority are currently enrolled in education, a statistic that is partly due to our focus on children who ever enrolled in preschool or school. On av-

erage, they have attended about four and half grades, but only 58% in Kenya and 43% in Tanzania possess second-grade skills. Finally, Appendix Table 1 presents enrollment statistics and outcome means separately by age. As shown there, children tend to be behind grade for age, with a non-negligible share of the younger school-age children still attending preschool. This finding will be important for the interpretation of our results below.

## 4. Empirical strategy

The main challenge in identifying the causal effects of preschool attendance on later outcomes is that selection into pre-primary education is likely non-random. For example, more educated parents may have a stronger preference for preschool education while also fostering their children's learning in other ways. In this case, any regression that does not control for this selection would yield estimates that are biased upward. To address this challenge, we follow a strand of previous literature (Currie and Thomas, 1995; Garces, Thomas, and Currie, 2002; Berlinski, Galiani, and Manacorda, 2008; Deming, 2009) and estimate models with household fixed effects, thus holding constant all determinants of preschool attendance and outcomes that do not vary between siblings. Our main OLS specification reads:

$$\begin{aligned} Y_{ij} = &\alpha + \beta_1 PRE_{ij} \times Age\ Group_{ij}^{7-9} + \beta_2 PRE_{ij} \times Age\ Group_{ij}^{10-12} \\ &+ \beta_3 PRE_{ij} \times Age\ Group_{ij}^{13-16} + \mathbf{AGE}_{ij}'\gamma + \mathbf{X}_{ij}'\theta + \eta_j + \varepsilon_{ij}. \end{aligned} \tag{1}$$

Here, $i$ denotes individuals and $j$ denotes households, $Y_{ij}$ is the highest grade attended or the composite test score, $PRE_{ij}$ is the indicator for preschool attendance, and $\mathbf{AGE}_{ij}$ is a vector of individual age dummies. $\mathbf{X}_{ij}$ is a vector of controls that includes dummies for birth order and gender and their interactions, dummies for cohort and their interactions with the individual age dummies, and the proxies for early-life economic conditions described above.[12] We allow for dynamic impacts of preschool attendance

[12]In alternative specifications which do not include household fixed effects, we also control for the other socio-demographic characteristics shown in Table 1.

by interacting $PRE_{ij}$ with three age group indicators: $Age\ Group_{ij}^{7-9}$ for ages 7-9, $Age\ Group_{ij}^{10-12}$ for ages 10-12, and $Age\ Group_{ij}^{13-16}$ for ages 13-16. Because these age groups encompass all individuals in our sample, $\beta_1$, $\beta_2$, and $\beta_3$ identify the main effect of preschool attendance for each age group (further below, we also report results from specifications in which $PRE_{ij}$ is interacted with the ten individual age dummies in $\mathbf{AGE}_{ij}$ instead). We weight all of our regressions using the sampling weights provided with the Uwezo data, and cluster standard errors at the district level.

The main parameters of interest in equation 1 are $\beta_1$, $\beta_2$, and $\beta_3$. They identify the age-group specific causal effect of preschool attendance under the assumption that among siblings, selection into preschool is uncorrelated with any other determinants of the outcome.[13] While comparatively weak, this assumption might be violated for several reasons, two of which are particularly salient. First, given that pre-primary education is costly, household income shocks around preschool age may be driving siblings' differential enrollment. Because such income shocks can influence children's educational success also in other ways (e.g. Shah and Steinberg, 2017), this could introduce bias into our estimates. We address this concern by including detailed district-level controls for early-life economic conditions in our regressions. Second, households with limited resources may choose to invest only in children with the "highest potential."[14] In this case, we would expect children who attended preschool to differentially benefit also from other investments such as private tutoring. However, in a robustness check below, we find no evidence of such behavior.

Which factors drive the between-sibling variation in preschool attendance in our data if not income shocks and differential investments based on relative "potential?" We investigate this question in Table 2, which reports results of regressions of the indicator for preschool attendance on

[13]Note that in equation 1, children without siblings with different preschool status do not contribute to the identification of $\beta_1$, $\beta_2$, and $\beta_3$. We nevertheless keep these children in our sample as this increases the precision of our estimates. In a robustness check below, we show that results are qualitatively and quantitatively similar when focusing on the restricted sample of households with within variation in preschool attendance.

[14]Such reinforcing behavior has been found in several previous studies on developing countries, see Almond and Mazumder (2013). Alternatively, compensatory behavior might lead to negative selection into preschool, biasing our estimates downward.

the control variables. Columns 1 and 3 show estimates from a specification without household fixed effects for Kenya and Tanzania, respectively. In both countries, children of educated mothers and from wealthier households are more likely to have attended preschool, underlining the importance of controlling for between-family differences. Columns 2 and 4 show that once household fixed effects are included in the regressions, most of the factors that still vary between siblings are no longer predictive of preschool attendance, including the proxies for early-life economic conditions.[15]

The lower part of Table 2 reports the coefficients on the cohort dummies. There is a marked and nearly monotonic trend in both countries, with later cohorts being significantly more likely to have attended preschool. This trend is especially pronounced in Tanzania, where the attendance rate was much lower than in Kenya at baseline (see Section 2) and where it increased by 15 percentage points over the twelve cohorts in our sample. Our interpretation of these estimates is that the expansion of pre-primary education during our study period led to differences in preschool availability between siblings, which in turn are driving the differences in attendance. As long as these changes in availability are unrelated to changes in other determinants of educational outcomes, this implies that we identify the true causal effects of preschool attendance in the analysis below.[16]

## 5. Results

We now present our main results. The following subsection reports estimates of the effect of preschool attendance on school progression, and the second subsection reports the corresponding impacts on literacy and numeracy skills. We then present results from regressions that probe for heterogeneity of these effects by children and household characteristics.

[15]To avoid cluttering, rather than separate dummies for early-life economic conditions at each age, specifications in Table 2 simply include the number of positive and negative rainfall shocks and the average log night lights before school entry. Results are qualitatively similar if we include the full set of controls instead; in particular, early-life economic conditions appear to be largely orthogonal to preschool attendance. Notably, this is not due to poor measurement, as these variables are highly predictive of children's literacy and numeracy skills (results available upon request).

[16]Ideally, we would like to further investigate this hypothesis using data on preschool openings by district and year. Unfortunately, however, such data do not appear to exist.

After that, we present evidence on a potential mechanism and compare our findings to the results from the previous literature.

### *5.1. Effects of preschool attendance on school progression*

Table 3 shows estimates of the effect of preschool attendance on the highest grade of school attended. Column 1 reports results from parsimonious specifications which only control for age dummies, their interaction with cohort dummies, and district fixed effects. Due to the frequent late enrollment in pre-primary and subsequently primary education, children who went to preschool initially have attended fewer grades than their peers who directly entered primary school. However, these children also progress through grades at a faster pace and eventually overtake their peers: at ages 13-16, they have attended 0.18 more grades in Kenya and 0.31 more grades in Tanzania. Columns 2-4 successively add controls for socio-demographic characteristics, early-life economic conditions, and household fixed effects to the regressions. Consistent with the idea of positive selection into pre-primary education, this tends to reduce the coefficients: from our preferred specification in column 4, children who went to preschool are now estimated to accumulate the same number of grades in Kenya and about 0.1 more grades in Tanzania by ages 13-16.[17]

An interesting question is whether children who went to preschool catch up with their peers in terms of grades completed because they skip more or repeat fewer grades while in school, or because they are less likely to drop out of school. To investigate this issue, column 5 of Table 3 presents estimates of the effect of preschool attendance on current enrollment based

[17]Besides household fixed effects accounting for selection on unobserved factors, there are at least three further potential explanations for the reduction of coefficients between columns 3 and 4. First, attenuation bias due to measurement error in attendance is aggravated in the between-sibling specification, an issue that we discuss in detail below. Second, the inclusion of household fixed effects nets out any positive sibling spillovers. Third, the effects in column 4 are identified only from households with within variation, which might differ from those in the full sample. Investigating this last possibility, we found that households with both attending and non-attending children were larger, poorer, and more likely to be located in a rural area. However, when we restricted the sample to these households only, the inclusion of household fixed effects similarly led to a decline in the estimated long-term benefits of preschool. This suggests that identification based on a different sample is not driving the change in coefficients between columns 3 and 4 of Table 3.

on our preferred specification with household fixed effects. The results indicate that children who went to preschool are indeed more likely to be enrolled, especially at higher ages and in Tanzania. Thus, lower dropout is at least partly underlying the catch-up observed in column 4.[18] Finally, Figure 1 plots estimates from regressions of the highest grade completed and enrollment status in which the effect of preschool attendance is allowed to differ at each age, rather than across age groups. The plots show that the impacts of attendance on these outcomes rise almost monotonically with age, confirming the results from Table 3.

*5.2. Effects of preschool attendance on literacy and numeracy skills*

Table 4 reports estimates of the effect of preschool attendance on children's literacy and numeracy skills. Column 1 shows results from a specification with only basic controls and the composite test score as outcome. In Kenya, children who went to preschool have slightly higher scores than their peers at early ages, and this advantage grows to 0.1 SD for the two older age groups. In contrast, in Tanzania, children with pre-primary education outperform their peers by 0.26 SD already early on, but this difference decreases to 0.22 SD for the group of 13-16-year-olds. Columns 2-4 successively add control variables and household fixed effects to these regressions. Similar to the pattern found for the highest grade attended, this substantially reduces the size of the estimates for Tanzania, where the impact for the oldest age group is now estimated at 0.08 SD. In contrast, the coefficients for Kenya are relatively stable across specifications, suggesting that there is little selection into preschool based on academic ability.

Column 5 presents results for a specification with the dummy for possessing second-grade literacy and numeracy skills as dependent variable. In both countries, the effect for the youngest age group is close to zero. This should come as no surprise because preschools are unlikely to teach

[18]Unfortunately, the Uwezo data do not contain information on school starting age and grade repetition, which prevents us from fully disentangling the mechanisms behind this catch-up. In column 5 of Table 3, note that for the younger two age groups, enrollment rates are very high and the predicted probabilities from the linear probability model sometimes exceed 100 percent. Addressing this issue, we confirmed that probit models and a simple comparison of means also suggest that children who attended preschool are more likely to be currently enrolled in school.

children such advanced skills, and because attendance actually leads to a reduction in grades completed at these ages (see Table 3). Note that this finding implies that the positive impact on test scores for young children in Tanzania in column 4 must be due to pre-primary education boosting their very basic literacy and numeracy skills. Mirroring the long-term gains on the composite test score, the estimates for 13-16-year-olds show a 3.2 (4.7) percentage point increase in the likelihood to achieve second-grade skills in Kenya (Tanzania), which corresponds to a sizable 4 (7) percent over the mean. Finally, Figure 2 shows age-by-age impacts on test scores and the dummy for basic skills which confirm the patterns observed in Table 4.

*5.3. Heterogeneity*

In Table 5, we explore the heterogeneity of the preschool impacts along several dimensions. Columns 1-3 report estimates from specifications that allow the effects to differ by length of attendance. The results reveal that children who went to preschool for two or three years tend to have attended fewer school grades than those who went for only one year, likely because they entered primary school later. This difference shrinks over time, however, suggesting that children with more years of preschool progress through grades at a faster pace. The impacts on the composite test score similarly tend to be less positive for children with several years of pre-primary education, at least in Kenya. This could be due to the fact that they are further behind in school, or due to negative selection at the intensive margin, with parents keeping children with lower academic ability in preschool for longer. Moreover, the fact that children of different ages are often taught together in preschool implies that attending for more years does not necessarily mean learning higher-level skills, a reality that might also contribute to the lack of a positive effect at the intensive margin.

Columns 4-5 show that in both countries, the effects of preschool attendance are consistently more positive for girls, even though the differences are not always statistically significant at conventional levels. Furthermore, columns 6-7 reveal that the improvements in literacy and numeracy skills among 13- to 16-year-olds tend to be larger for those with uneducated mothers: in Tanzania, for example, preschool attendance raises the composite test score for these children by 0.14 SD, compared to 0.06 SD for

children with mothers who have at least some formal education. Finally, columns 8-9 show that in Tanzania only, longer-term gains in the number of grades attended and test scores are substantially larger in more urban, high-economic-activity areas, as proxied by living in a district with night light density above the 85$^{th}$ percentile of the national distribution.

*5.4. "Head start" as a potential mechanism*

An interesting open question is how exactly preschools improve learning outcomes for attending children. One obvious explanation is that with their focus on teaching basic literacy and numeracy, preschools give children a head start that makes it easier for them to follow the primary school curriculum. An implication of this explanation is that these students should have higher skills already at school start. To what extent this is indeed the case is not immediately obvious from our results above, which are based on same-age comparisons that blend the potential effect of a skill boost from preschool attendance with any effect on learning due to later school entry and lower grade attainment. In order to disentangle these two channels, one would ideally want to compare children who differ in terms of their preschool attendance but who started school at the same time. Unfortunately, such a comparison is not feasible here because school starting age is not observed in the Uwezo data.

As an alternative way to separate the test score impacts due to learning in preschool from the impacts due to later school entry, Table 6 reports results from specifications that control for the highest grade attended. If children do indeed get a head start from attending preschool, we would expect the coefficients in these regressions to increase compared to our main results, especially for the youngest children who are furthest behind in school. This turns out to be the case: for example, the effect for 7- to 9-year-olds in Kenya is now estimated to be 0.16 SD, compared to only 0.04 SD when their lower grade attainment is not taken into account. Thus, these results suggest that preschools do in fact give children a head start in the learning of literacy and numeracy skills, and that this might drive the long-term improvements in learning outcomes.

*5.5. Comparison with results from previous studies*

We now compare our results to the previous literature on preschool effectiveness in developing countries. Focusing first on schooling, our estimates are qualitatively similar to those by Berlinski, Galiani, and Manacorda (2008) for Uruguay. Like us, the authors find that children who go to preschool initially fall behind in terms of grades attended but that they progress through school faster later on, leading to an increase in educational attainment by 0.8 years of schooling at age 15. For Egypt, Krafft (2015) similarly shows that children with pre-primary education accumulate 0.4 more years of schooling by ages 18-29. Unlike her, we do not observe final educational attainment, which may explain our smaller estimates at ages 13-16. However, the monotonic positive trend in the impact on grades attended in Figure 2 suggests that children who attended preschool might eventually acquire more years of schooling also in Kenya and Tanzania.

Turning to cognitive skills, Berlinski, Galiani, and Gertler (2009) find that in Argentina, preschool attendance increases third-grade students' math and language test scores by 0.23 SD. For Indonesia, Brinkman et al. (2017) similarly show that three years after the establishment of early childhood services, children from poor households improved by 0.20 SD on an index of language and cognitive development.[19] One potential reason why our estimates are smaller is the presence of ceiling effects: as discussed in Section 3, the Uwezo assessments measure second-grade skills, and although most children in our sample do not reach the highest competency levels on the tests, a non-negligible fraction of the older children in particular does (see Appendix Table 1). As ceiling effects lead to an attenuation of regression estimates, this could explain our lower point estimates compared to the previous literature, as well as the slight fade-out of the effects on cognitive skills visible for the older age groups in Figure 2.

Finally, a recent study by Bouguen et al. (2018) shows that in Cambodia, 6-year-old children scored 0.19 SD lower on an index of cognitive development one year after preschools were constructed in their villages.

[19] Behrman, Cheng, and Todd (2004) and Martinez, Naudeau, and Pereira (2013) also document positive effects on measures of child development that include cognitive skills, but these are difficult to compare directly to our outcomes.

Investigating potential channels, the authors find that preschool construction led to a shift from early enrollment in primary school to enrollment in preschool, suggesting that the decrease in cognitive skills was partly due to the lower emphasis on literacy and numeracy skills in the preschool curriculum.[20] Interpreted in the light of our results, this shift in enrollment led to a decrease in grades attended early on. While Bouguen et al. (2018) cannot investigate the longer term consequences of this change, our estimates reveal that despite low or negative initial returns, children who attend preschool can catch up in terms of grades attended and strongly benefit in terms of learning later on. These longer-run estimates on cognitive skills in particular are a key contribution of our paper over the previous literature, which has only been able to study short-run effects.

## 6. Robustness

### *6.1. Addressing selection concerns*

In Section 3 above, we argue that the between-sibling variation in preschool attendance is likely due to changes in availability, which came about because of the expansion of the pre-primary sector during our study period. One might worry, however, that this variation instead reflects child-specific investments that are correlated with unobservables. As an example, households with limited resources may choose to invest only in children with the "highest potential." If this is indeed the case, one would expect that families differentially spend on children who are sent to preschool also in other ways. We test this hypothesis in a falsification exercise by examining whether children who attended preschool are more likely to benefit from two other costly educational inputs observed in our data: private after-school tutoring and enrollment in private school.[21] Table 7 shows that

[20] Also in our setting, early enrollment in primary school might be part of the counterfactual. However, whereas Bouguen et al. (2018) report that 60 percent of children in control villages attended primary school in the year before they reached the official school starting age, only 26 percent of 5-year-olds in the 1999 Kenyan census and 8 percent of 6-year-olds in the 2002 Tanzanian census did so. Note also that respondents in the Uwezo surveys answer separate questions about current and past preschool attendance and current and past school attendance, allowing us to separate the impacts of preschool attendance from those of early enrollment in primary school.

[21] See Wamalwa and Burns (2017) for an analysis of private school effectiveness in Kenya using the Uwezo survey data.

in regressions of indicators for receiving these inputs, the coefficients on preschool attendance are close to zero and precisely estimated. Although these are just two out of many ways in which parents invest in their children, these estimates thus suggest that differential investments based on child unobservables are not driving our main results.

To judge the importance of selection bias more generally, we next ask how large such bias would need to be in order to explain away our main effects. Our analysis builds on the approach presented in Oster (2017), which relies on comparing the coefficients of interest and the $R$-squared between regressions with and without control variables to gain insights into the influence of omitted variables. Here, we focus on the calculation of $\delta$, which is the ratio of the impact of unobservables to the impact of observable controls that would drive the coefficient on the treatment variable to zero. As a point of reference, Oster (2017) suggests that effects for which $\delta > 1$ can be considered robust. Applying this method to our case, we contrast estimates of the impact of preschool attendance from a specification with only basic controls (as in column 1 of Tables 3 and 4) with those from our preferred specification with household fixed effects. We restrict our sample to households with variation in preschool attendance for this analysis because for all other households, the fixed effects fully explain preschool attendance, leaving no role for selection on unobservables.[22]

Table 8 reports the results from this exercise. Columns 1 and 3 show estimates from specifications with only basic controls, with regressions underlying columns 2 and 4 adding further controls and household fixed effects. As would be expected, the estimates from our preferred specification are generally very similar in the restricted sample compared to the full sample used in the main analysis (we test the equality of these coefficients more formally below). Moving from the basic specification to our preferred specification substantially increases the $R$-squared and tends to decrease the coefficient estimates, in line with what is observed in Tables 3 and 4. Based on these differences, we report the implied $\delta$ for preschool impacts for 13- to 16-year-olds, for whom we find the most positive impacts and where selection on unobservables is thus the most relevant concern. In three

[22]We thank Emily Oster for this suggestion.

out of four specifications, $\delta$ is greater than one, implying that selection on unobservables would have to be greater than selection on the observed control variables to drive the preschool impacts to zero. For the case of the composite test score, this value is even above four for both countries, which strongly suggests that omitted variable bias is not driving these results.[23]

*6.2. Further robustness checks*

We now address a number of further potential concerns regarding our empirical analysis. First, as discussed in Section 3, our results may be affected by recall error in the retrospectively reported preschool variable. Note that such recall error can lead to upward bias only if it systematically varies between siblings who did and did not attend preschool; any general, idiosyncratic recall error will simply drive our estimates towards zero. We investigate this issue by taking advantage of the repeated cross-sectional nature of our data, which lets us follow cohorts over time. Appendix Table 2 shows the fraction of children in each cohort reporting to have attended preschool separately for each Uwezo survey wave.[24] If our data were contaminated by recall error, we would expect these fractions to change over time. This is not the case for the majority of cohorts, though, which suggests that recall error does not bias our results.

Second, our estimates might be attenuated by measurement error in the preschool attendance variable. Such downward bias is particularly relevant in the context of sibling fixed effects models, in which the signal to noise ratio of the measurement may be greatly reduced (Ashenfelter and Krueger, 1994). Because of the absence of repeated individual-level information on preschool attendance in our data, we are unable to establish the

[23]There is a strong mechanical relationship between highest grade attended and age, as reflected by the high values of $R$-squared in column 1 of Table 8. This limits the additional explanatory power any observed controls can have in these regressions, which partly explains the lower values for $\delta$ in column 2 compared to column 4 of Table 8.

[24]For this exercise, we focus on a comparable sample of districts that were visited in all waves of the Uwezo surveys. We disregard the 2014 wave in Tanzania because only a small subsample of districts were included in that year's survey. In the raw data, we observe level shifts in preschool attendance rates *for all cohorts* between some of the waves, likely because the question asking about preschool attendance changed. In Appendix Table 2, we therefore report regression-adjusted attendance rates after taking out wave fixed effects. Note that level shifts in preschool attendance do not influence our within-household results, which use variation within survey waves.

extent of this problem conclusively. However, the strong similarity of the aggregate, cohort-level measure over time in Appendix Table 2 suggests that measurement error is not a major issue in our context.

Third, one might worry that our results are driven by siblings who are very different in age, and who thus grew up under very distinct circumstances. For example, the impacts on cognitive skills might vary between age groups only because different cohorts attended preschools of very different quality (although as discussed in Section 2, the available metrics do not show clear trends in preschool quality over time). To address this issue, Online Appendix Table B.1 presents estimates from regressions in which the sample is restricted to siblings who are born at most five years apart. As can be seen there, the results are qualitatively and quantitatively similar to our main findings.

Finally, we conduct additional analyses that address a range of further potential concerns. Thus, in Appendix Table 3, we compare our main results with those obtained from a sample which is restricted to households with within variation in preschool attendance. The sub-sample results are qualitatively and, for the most part, quantitatively similar to the main results, but, as expected, less precisely estimated. In Online Appendix Figure B.1, we further document that preschool impacts on the individual English, Swahili, and numeracy scores are very similar to the ones on the composite test scores used in the main analysis. Our main results are also robust to not using sampling weights, as indicated in Online Appendix Table B.1. Lastly, Online Appendix Figure B.2 presents Kaplan-Meier estimates as an alternative way to investigate the impacts of preschool attendance on grade progression. These estimates are in line with our main findings in Section 5.

## 7. Conclusion

Most children in Sub-Saharan Africa enroll in school nowadays, but they learn remarkably little there. One possible reason is that they enter school unprepared, which makes preschool programs that aim to get children ready for school a promising way to improve learning outcomes. While pre-primary education is becoming increasingly common within the region, to date very little is known about its effectiveness.

In this paper, we provide some of the very first evidence of preschool impacts on learning outcomes in Sub-Saharan African. We use data from large-scale surveys of children's educational attainment and cognitive skills from Kenya and Tanzania, which also collect retrospective information on preschool attendance. Our analysis compares the highest school grade attended as well as achievement on standardized literacy and numeracy tests of siblings who did and did not attend preschool. This strategy allows us to control for any determinants of pre-primary enrollment and outcomes that do not vary within households. We provide evidence that the leftover between-sibling variation in attendance is due to changes in availability, which came about because of a large expansion of preschool education during our study period.

Our results show that preschool education leads to important long-term learning benefits: at ages 13-16, children who went to preschool are three and five percentage points more likely to achieve basic, second-grade literacy and numeracy in Kenya and Tanzania, respectively. These gains materialize relatively late because children who attend preschool tend to enter primary school late and thus fall behind early on. However, the skills learned in preschool give them a head start in school, meaning that they can progress through grades faster and eventually catch up with their peers who did not attend preschool in terms of grades attended. Overall, our analysis shows that increasing access to pre-primary education can be an effective instrument to improve learning outcomes in Sub-Saharan Africa.

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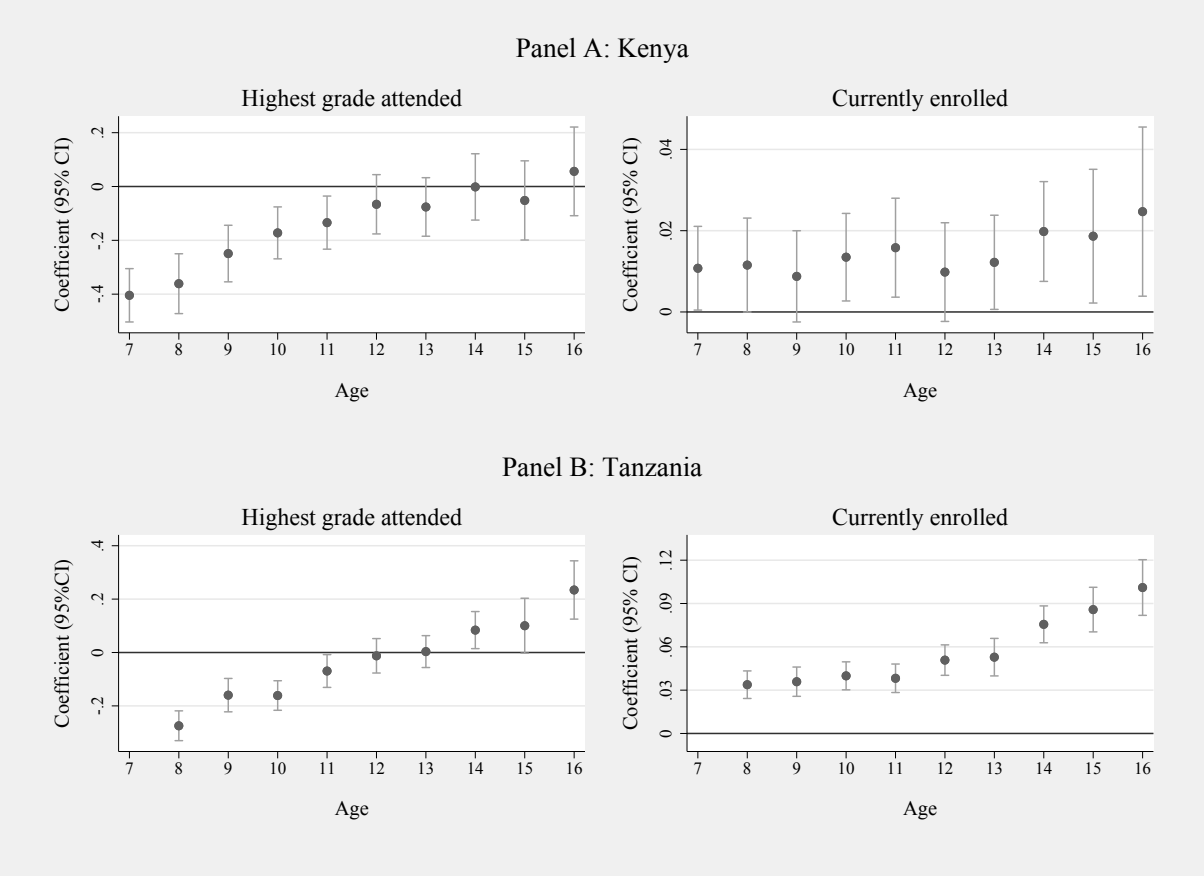
## Figure 1

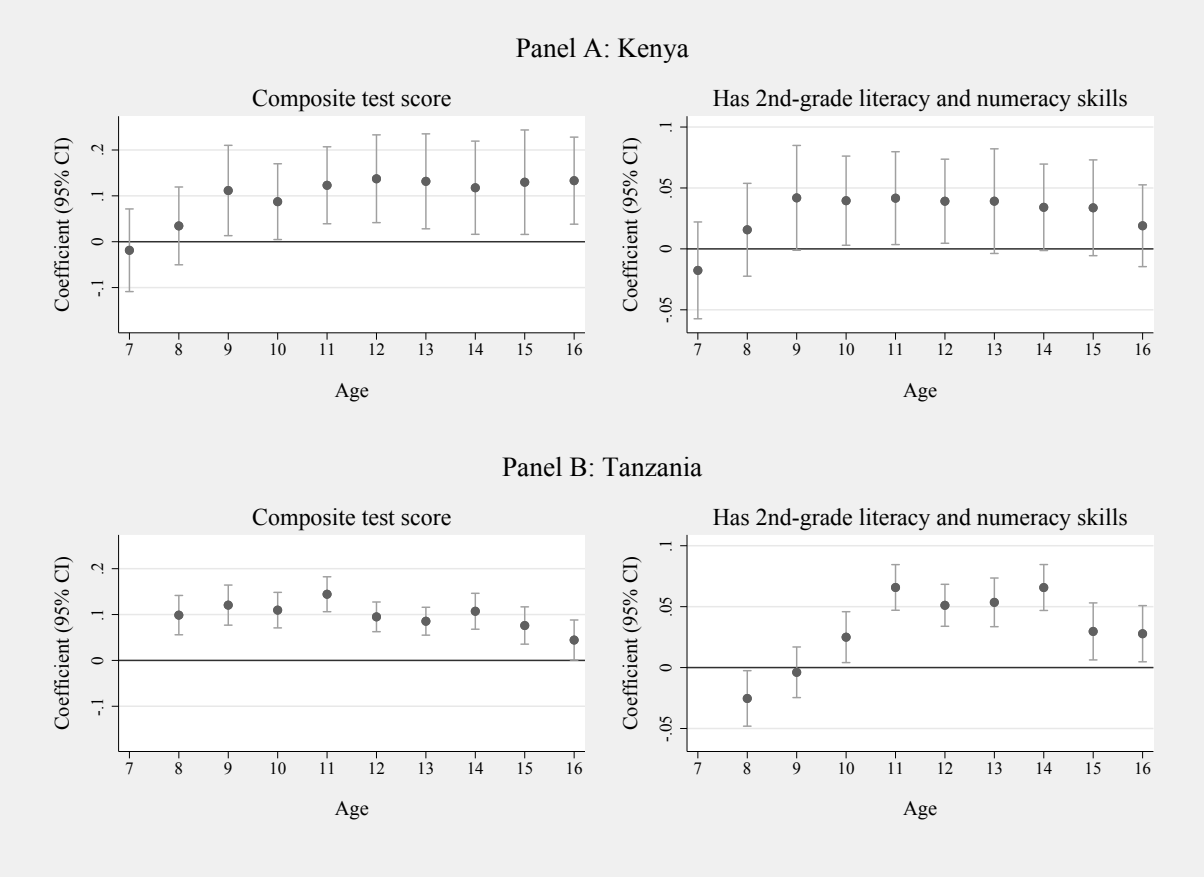
**Effects of preschool attendance on highest grade attended and school enrollment, by age**




*Notes:* The figure plots coefficient estimates and 95% confidence intervals from regressions of highest grade attended and an indicator for current enrollment on preschool attendance. The indicator for preschool attendance is interacted with a full set of age dummies, and the figure shows the main effect of preschool attendance at each age. Specifications are otherwise equal to the household fixed effects regressions reported in columns 4 and 5 of Table 3.

## Figure 2

**Effects of preschool attendance on literacy and numeracy skills, by age**




*Notes:* The figure plots coefficient estimates and 95% confidence intervals from regressions of the composite test score and an indicator for having second-grade literacy and numeracy skills on preschool attendance. The indicator for preschool attendance is interacted with a full set of age dummies, and the figure shows the main effect of preschool attendance at each age. Specifications are otherwise equal to the household fixed effects regressions reported in columns 4 and 5 of Table 4.

## Table 1
**Summary statistics**

| | Kenya | Tanzania |
|---|---|---|
| *Socio-demographic characteristics* | | |
| Age | 11.08 | 11.72 |
| | (2.77) | (2.46) |
| Female | 0.49 | 0.50 |
| | (0.50) | (0.50) |
| Mother's education: | | |
| None | 0.17 | 0.19 |
| | (0.37) | (0.39) |
| Some primary or more | 0.83 | 0.81 |
| | (0.37) | (0.39) |
| No. of children in household | 3.09 | 2.47 |
| | (1.55) | (1.26) |
| Current household wealth (index) | 0.00 | 0.00 |
| | (1.00) | (1.00) |
| Rural location | 0.67 | 0.78 |
| | (0.47) | (0.41) |
| *Early-life economic conditions* | | |
| No. of negative rainfall shocks | 1.44 | 1.87 |
| | (0.85) | (0.87) |
| No. of positive rainfall shocks | 1.10 | 1.27 |
| | (0.79) | (0.72) |
| Log night light density | -1.44 | -2.39 |
| | (2.32) | (2.29) |
| *Preschool attendance* | | |
| Attended preschool | 0.85 | 0.62 |
| | (0.36) | (0.48) |
| Years of preschool \|attended | 2.11 | 1.32 |
| | (1.08) | (0.75) |
| *Outcomes* | | |
| Highest grade attended | 4.62 | 4.42 |
| | (2.53) | (2.28) |
| Currently enrolled | 0.99 | 0.94 |
| | (0.10) | (0.24) |
| Composite test score | 0.00 | 0.00 |
| | (1.00) | (1.00) |
| Has 2nd-grade lit./num. skills | 0.58 | 0.43 |
| | (0.49) | (0.49) |
| Observations (children): | | |
| Total | 223,339 | 293,757 |
| With within-household variation | 7,532 | 31,153 |

*Notes:* The table reports means and standard deviations (in parentheses) of key variables separately for children in Kenya and Tanzania. In regressions, early-life economic conditions are proxied by district-level indicators for negative and positive rainfall shocks and district-level log night lights at each age between 0 and 5 in Kenya (0 and 6 in Tanzania); for conciseness, this table shows totals across all of these ages. Years of preschool are observed only in the 2013 and 2014 waves of the Uwezo survey ($N$=223,339 in Kenya and $N$=111,043 in Tanzania). Currently enrolled is an indicator for being currently enrolled in either preschool or school. Has 2nd-grad lit./num. skills is an indicator for achieving the highest competency level in the numeracy test and at least one of the two literacy tests. The final row reports the number of children living in households with within variation, that is, households in which at least one child went to preschool and at least one child did not. Further details on the construction of all variables are provided in Online Appendix A.

## Table 2
**Predicting preschool attendance**

| | Kenya | | Tanzania | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Female | 0.003 | -0.001 | 0.008** | 0.003 |
| | (0.003) | (0.002) | (0.003) | (0.003) |
| Firstborn | 0.005 | 0.000 | -0.002 | 0.002 |
| | (0.004) | (0.003) | (0.005) | (0.004) |
| Female × firstborn | -0.001 | 0.001 | -0.004 | 0.000 |
| | (0.004) | (0.003) | (0.005) | (0.005) |
| Mother ≥ some primary edu. | 0.016** | | 0.097*** | |
| | (0.007) | | (0.006) | |
| No. of children in household | -0.002 | | -0.012*** | |
| | (0.002) | | (0.002) | |
| Household wealth index | 0.006** | | 0.052*** | |
| | (0.003) | | (0.004) | |
| Rural location | -0.000* | | -0.016 | |
| | (0.000) | | (0.022) | |
| No. of negative rainfall shocks | -0.003** | -0.001 | -0.007* | -0.000 |
| | (0.001) | (0.001) | (0.004) | (0.004) |
| No. of positive rainfall shocks | -0.004 | -0.000 | 0.006 | 0.007* |
| | (0.002) | (0.001) | (0.004) | (0.004) |
| Log night light density | 0.009 | 0.002 | 0.016 | 0.011 |
| | (0.006) | (0.005) | (0.016) | (0.012) |
| Cohort | | | | |
| 1996 | | | 0.023*** | 0.021 |
| | | | (0.008) | (0.014) |
| 1997 | | | 0.030*** | 0.035*** |
| | | | (0.009) | (0.011) |
| 1998 | 0.005 | 0.002 | 0.047*** | 0.050*** |
| | (0.008) | (0.004) | (0.009) | (0.013) |
| 1999 | 0.012* | 0.003 | 0.062*** | 0.069*** |
| | (0.007) | (0.004) | (0.010) | (0.014) |
| 2000 | 0.011 | 0.006 | 0.088*** | 0.084*** |
| | (0.007) | (0.004) | (0.011) | (0.015) |
| 2001 | 0.019*** | 0.006 | 0.090*** | 0.086*** |
| | (0.007) | (0.005) | (0.011) | (0.016) |
| 2002 | 0.022*** | 0.008 | 0.098*** | 0.101*** |
| | (0.007) | (0.005) | (0.011) | (0.015) |
| 2003 | 0.026*** | 0.004 | 0.110*** | 0.106*** |
| | (0.007) | (0.005) | (0.011) | (0.016) |
| 2004 | 0.023** | 0.007 | 0.113*** | 0.109*** |
| | (0.009) | (0.005) | (0.012) | (0.017) |
| 2005 | 0.033*** | 0.007 | 0.099*** | 0.099*** |
| | (0.009) | (0.006) | (0.012) | (0.017) |
| 2006 | 0.040*** | 0.014** | 0.147*** | 0.149*** |
| | (0.007) | (0.006) | (0.016) | (0.023) |
| 2007 | 0.032*** | 0.013 | | |
| | (0.009) | (0.009) | | |
| Household fixed effects | No | Yes | No | Yes |
| Observations | 223,339 | 223,339 | 293,757 | 293,757 |

*Notes:* The table reports estimates from regressions of an indicator for preschool attendance on the variables listed in rows and Uwezo wave dummies. Standard errors in parentheses are clustered at the district level. * p<0.10, ** p<0.05, *** p<0.01.

## Table 3

**Effects of preschool attendance on highest grade attended and school enrollment**

| | Highest grade attended | | | | Currently enrolled |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | Panel A: Kenya | | | | |
| Effect of preschool | | | | | |
| at ages 7-9 | -0.234*** | -0.252*** | -0.242*** | -0.336*** | 0.011* |
| | (0.040) | (0.042) | (0.042) | (0.045) | (0.005) |
| at ages 10-12 | 0.017 | -0.006 | -0.006 | -0.123*** | 0.013** |
| | (0.024) | (0.023) | (0.023) | (0.044) | (0.006) |
| at ages 13-16 | 0.178*** | 0.161*** | 0.153*** | -0.023 | 0.018*** |
| | (0.034) | (0.034) | (0.033) | (0.055) | (0.006) |
| $p$ (equal effects) | 0.000 | 0.000 | 0.000 | 0.000 | 0.032 |
| Observations | 218,728 | 218,728 | 218,728 | 218,728 | 218,728 |
| | Panel B: Tanzania | | | | |
| Effect of preschool | | | | | |
| at ages 8-9 | -0.071*** | -0.127*** | -0.115*** | -0.212*** | 0.036*** |
| | (0.018) | (0.018) | (0.018) | (0.025) | (0.005) |
| at ages 10-12 | 0.081*** | 0.028 | 0.031* | -0.074*** | 0.044*** |
| | (0.020) | (0.018) | (0.019) | (0.025) | (0.004) |
| at ages 13-16 | 0.313*** | 0.258*** | 0.243*** | 0.096*** | 0.076*** |
| | (0.026) | (0.026) | (0.024) | (0.031) | (0.006) |
| $p$ (equal effects) | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Observations | 284,396 | 284,396 | 284,396 | 284,396 | 284,396 |
| | Controls included in panels A and B | | | | |
| Age × cohort effects | Yes | Yes | Yes | Yes | Yes |
| District fixed effects | Yes | Yes | Yes | No | No |
| Socio-demographics | No | Yes | Yes | Yes | Yes |
| Early-life conditions | No | No | Yes | Yes | Yes |
| Household fixed effects | No | No | No | Yes | Yes |

*Notes:* The table reports estimates from regressions of highest grade attended and enrollment status on an indicator for preschool attendance and control variables as indicated in the lower panel. The indicator for preschool attendance is interacted with an exhaustive set of three age-group dummies, such that the coefficients shown here reflect the main effects for each age group. Socio-demographic controls include the variables shown in Table 1, dummies for birth order and gender and their interactions, dummies for cohort and their interactions with individual age dummies, and sibling age span. Controls for early-life economic conditions include district-level indicators for negative and positive rainfall shocks and district-level log night lights at each age between 0 and 5 (0 and 6 in Tanzania), all of which are interacted with a dummy for rural location. Specifications in columns 1-3 additionally control for Uwezo wave dummies. The $p$-value reported below each set of coefficients is from an $F$-test of equal effects across age groups. See Appendix Table 1 for outcome means by age. Standard errors in parentheses are clustered at the district level. * p<0.10, ** p<0.05, *** p<0.01.

## Table 4

**Effects of preschool attendance on literacy and numeracy skills**

| | Composite test score | | | | $2^{nd}$-grade lit./num. |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | Panel A: Kenya | | | | |
| Effect of preschool | | | | | |
| at ages 7-9 | 0.036* | 0.015 | 0.012 | 0.042 | 0.013 |
| | (0.022) | (0.022) | (0.021) | (0.040) | (0.016) |
| at ages 10-12 | 0.097*** | 0.078*** | 0.077*** | 0.114*** | 0.040** |
| | (0.022) | (0.021) | (0.021) | (0.041) | (0.015) |
| at ages 13-16 | 0.104*** | 0.095*** | 0.098*** | 0.125*** | 0.032* |
| | (0.021) | (0.021) | (0.021) | (0.044) | (0.017) |
| $p$ (equal effects) | 0.010 | 0.005 | 0.003 | 0.001 | 0.093 |
| Observations | 218,134 | 218,134 | 218,134 | 218,134 | 218,134 |
| | Panel B: Tanzania | | | | |
| Effect of preschool | | | | | |
| at ages 8-9 | 0.256*** | 0.204*** | 0.195*** | 0.108*** | -0.016* |
| | (0.017) | (0.016) | (0.015) | (0.018) | (0.009) |
| at ages 10-12 | 0.263*** | 0.212*** | 0.209*** | 0.113*** | 0.046*** |
| | (0.014) | (0.013) | (0.013) | (0.014) | (0.007) |
| at ages 13-16 | 0.217*** | 0.163*** | 0.169*** | 0.081*** | 0.047*** |
| | (0.016) | (0.015) | (0.014) | (0.014) | (0.008) |
| $p$ (equal effects) | 0.021 | 0.008 | 0.032 | 0.152 | 0.000 |
| Observations | 288,084 | 288,084 | 288,084 | 288,084 | 288,084 |
| | Controls included in panels A and B | | | | |
| Age × cohort effects | Yes | Yes | Yes | Yes | Yes |
| District fixed effects | Yes | Yes | Yes | No | No |
| Socio-demographics | No | Yes | Yes | Yes | Yes |
| Early-life conditions | No | No | Yes | Yes | Yes |
| Household fixed effects | No | No | No | Yes | Yes |

*Notes:* The table reports estimates from regressions of the composite test score and the indicator for achieving second-grade literacy and numeracy skills on an indicator for preschool attendance and control variables as indicated in the lower panel. The indicator for preschool attendance is interacted with an exhaustive set of three age-group dummies, such that the coefficients shown here reflect the main effects for each age group. Socio-demographic controls include the variables shown in Table 1, dummies for birth order and gender and their interactions, dummies for cohort and their interactions with individual age dummies, and sibling age span. Controls for early-life economic conditions include district-level indicators for negative and positive rainfall shocks and district-level log night lights at each age between 0 and 5 (0 and 6 in Tanzania), all of which are interacted with a dummy for rural location. Specifications in columns 1-3 additionally control for Uwezo wave dummies. The $p$-value reported below each set of coefficients is from an $F$-test of equal effects across age groups. See Appendix Table 1 for outcome means by age. Standard errors in parentheses are clustered at the district level. * p<0.10, ** p<0.05, *** p<0.01.

# Table 5
## Heterogeneity

| | by number of years attended | | | by gender | | by mother's education | | by night lights | |
|---|---|---|---|---|---|---|---|---|---|
| | main effect | × 2 years | × 3 years | main effect | × female | main effect | × some edu. | main effect | × > 85% |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **Panel A: Kenya** | | | | | | | | | |
| *1. Highest grade attended* | | | | | | | | | |
| Effect of preschool | | | | | | | | | |
| at ages 7-9 | -0.211*** | -0.374*** | -0.503*** | -0.341*** | 0.013 | -0.270*** | -0.024 | -0.336*** | 0.053 |
| | (0.050) | (0.045) | (0.055) | (0.049) | (0.046) | (0.073) | (0.096) | (0.044) | (0.184) |
| at ages 10-12 | 0.168*** | 0.190*** | 0.268*** | -0.164*** | 0.083** | -0.094 | -0.033 | -0.127*** | 0.009 |
| | (0.035) | (0.035) | (0.033) | (0.047) | (0.036) | (0.070) | (0.086) | (0.037) | (0.211) |
| at ages 13-16 | 0.181*** | 0.293*** | 0.441*** | -0.071 | 0.100** | -0.024 | -0.046 | -0.054 | 0.093 |
| | (0.053) | (0.054) | (0.056) | (0.066) | (0.049) | (0.073) | (0.092) | (0.047) | (0.225) |
| Observations | 218,728 | | | 218,728 | | 218,728 | | 218,728 | |
| *2. Composite test score* | | | | | | | | | |
| Effect of preschool | | | | | | | | | |
| at ages 7-9 | 0.045 | -0.006 | 0.074 | 0.034 | 0.019 | 0.076 | -0.041 | 0.022 | 0.104 |
| | (0.041) | (0.043) | (0.047) | (0.046) | (0.032) | (0.072) | (0.091) | (0.043) | (0.098) |
| at ages 10-12 | 0.089*** | 0.091*** | 0.043* | 0.086** | 0.055** | 0.143* | -0.069 | 0.116** | -0.013 |
| | (0.024) | (0.024) | (0.023) | (0.041) | (0.024) | (0.079) | (0.094) | (0.045) | (0.104) |
| at ages 13-16 | 0.122*** | 0.109*** | 0.025 | 0.100** | 0.054 | 0.235** | -0.148 | 0.136*** | -0.037 |
| | (0.037) | (0.037) | (0.035) | (0.046) | (0.038) | (0.100) | (0.120) | (0.049) | (0.103) |
| Observations | 218,134 | | | 218,134 | | 218,134 | | 218,134 | |
| **Panel B: Tanzania** | | | | | | | | | |
| *1. Highest grade attended* | | | | | | | | | |
| Effect of preschool | | | | | | | | | |
| at ages 8-9 | -0.237*** | -0.510*** | | -0.256*** | 0.087*** | -0.255*** | 0.070 | -0.205*** | -0.000 |
| | (0.054) | (0.083) | | (0.028) | (0.029) | (0.043) | (0.049) | (0.027) | (0.068) |
| at ages 10-12 | 0.081** | 0.116** | | -0.080*** | 0.012 | -0.127*** | 0.058 | -0.095*** | 0.102 |
| | (0.038) | (0.056) | | (0.028) | (0.029) | (0.046) | (0.047) | (0.026) | (0.072) |
| at ages 13-16 | 0.185*** | 0.332*** | | 0.100*** | -0.010 | -0.047 | 0.167*** | 0.048* | 0.310*** |
| | (0.055) | (0.098) | | (0.032) | (0.039) | (0.055) | (0.062) | (0.027) | (0.104) |
| Observations | 107,825 | | | 284,396 | | 284,396 | | 284,396 | |
| *2. Composite test score* | | | | | | | | | |
| Effect of preschool | | | | | | | | | |
| at ages 8-9 | 0.080*** | 0.157*** | | 0.080*** | 0.054** | 0.080** | 0.031 | 0.096*** | 0.029 |
| | (0.034) | (0.039) | | (0.022) | (0.025) | (0.039) | (0.050) | (0.018) | (0.060) |
| at ages 10-12 | 0.047 | -0.013 | | 0.089*** | 0.048*** | 0.140*** | -0.039 | 0.097*** | 0.090*** |
| | (0.029) | (0.030) | | (0.016) | (0.018) | (0.037) | (0.044) | (0.015) | (0.032) |
| at ages 13-16 | 0.044 | -0.089** | | 0.069*** | 0.024 | 0.144*** | -0.082* | 0.075*** | 0.105** |
| | (0.037) | (0.042) | | (0.017) | (0.018) | (0.040) | (0.046) | (0.014) | (0.049) |
| Observations | 107,605 | | | 288,084 | | 288,084 | | 288,084 | |

*Notes:* The table reports estimates from specifications in which the dummy for preschool attendance is interacted with three age-group dummies as in Tables 3 and 4 and additionally with two dummies for attending preschool for two and three years (columns 1-3), a female dummy (columns 4-5), a dummy for having a mother with at least some formal education (columns 6-7), and a dummy for living in a district with night light density above the 85$^{th}$ national percentile in the year 2000 (columns 8-9). The table reports the main effects for each age group as well as the coefficients on the interactions with these characteristics. Base levels in columns 1/4/6/8: attended for 1 year/male/no formal education/district night lights below the 85$^{th}$ national percentile. Outcome variables are indicated in cursive in the rows above the respective regressions. All regressions include household fixed effects and controls as in column 4 of Tables 3 and 4. Standard errors in parentheses are clustered at the district level. * p<0.10, ** p<0.05, *** p<0.01.

## Table 6

**Effect of preschool attendance on composite test scores after controlling for highest grade attended**

| | Baseline | Controlling for highest grade attended |
|---|---|---|
| | (1) | (2) |
| | Panel A: Kenya | |
| Effect of preschool | | |
| at ages 7-9 | 0.042 | 0.157*** |
| | (0.040) | (0.036) |
| at ages 10-12 | 0.114*** | 0.148*** |
| | (0.041) | (0.035) |
| at ages 13-16 | 0.125*** | 0.177*** |
| | (0.044) | (0.039) |
| Observations | 218,134 | 218,134 |
| | Panel B: Tanzania | |
| Effect of preschool | | |
| at ages 8-9 | 0.108*** | 0.157*** |
| | (0.018) | (0.017) |
| at ages 10-12 | 0.113*** | 0.126*** |
| | (0.014) | (0.014) |
| at ages 13-16 | 0.081*** | 0.104*** |
| | (0.014) | (0.013) |
| Observations | 288,084 | 288,084 |

*Notes:* Column 1 replicates the estimates shown in column 4 of Table 4. Specifications in column 2 add separate dummies for the number of grades attended to these regressions as controls. See the notes to Table 4 for further details on included control variables. Standard errors in parentheses are clustered at the district level. * p<0.10, ** p<0.05, *** p<0.01.

## Table 7

**Effects of preschool attendance on other educational investments**

| | After-school tutoring | | Private school attendance | |
|---|---|---|---|---|
| | Estimates | Outcome means | Estimates | Outcome means |
| | (1) | (2) | (3) | (4) |
| | Panel A: Kenya | | | |
| Effect of preschool | | | | |
| at ages 7-9 | -0.002 | 0.268 | -0.004 | 0.239 |
| | (0.011) | | (0.010) | |
| at ages 10-12 | 0.005 | 0.312 | -0.014 | 0.153 |
| | (0.011) | | (0.011) | |
| at ages 13-16 | 0.020 | 0.371 | -0.015 | 0.092 |
| | (0.017) | | (0.011) | |
| Observations | 223,339 | | 208,424 | |
| | Panel B: Tanzania | | | |
| Effect of preschool | | | | |
| at ages 8-9 | -0.000 | 0.219 | -0.005 | 0.060 |
| | (0.025) | | (0.004) | |
| at ages 10-12 | 0.018 | 0.245 | -0.006* | 0.047 |
| | (0.024) | | (0.003) | |
| at ages 13-16 | 0.020 | 0.253 | -0.001 | 0.077 |
| | (0.019) | | (0.003) | |
| Observations | 25,346 | | 264,810 | |

*Notes:* Column 1 shows estimates from regressions in which the dependent variable is an indicator for receiving private after-school tutoring, and column 3 shows estimates from regressions in which the dependent variable is an indicator for currently attending private school. All specifications are otherwise identical to the ones in column 4 of Tables 3 and 4. Columns 2 and 4 show age-group-specific means of the dependent variables. Sample size is reduced in column 1 for Tanzania because the outcome is only observed in the 2014 wave there. Sample size is reduced for both countries in column 3 because private school attendance is only observed for students who are currently enrolled in school. Standard errors in parentheses are clustered at the district level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

## Table 8
### Judging the importance of selection on unobservables

| | Highest grade attended | | Composite test score | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | Panel A: Kenya | | | |
| Effect of preschool | | | | |
| at ages 7-9 | -0.703*** | -0.423*** | -0.104 | 0.023 |
| | (0.072) | (0.070) | (0.063) | (0.066) |
| at ages 10-12 | 0.110 | -0.090 | 0.251*** | 0.165** |
| | (0.070) | (0.078) | (0.064) | (0.066) |
| at ages 13-16 | 0.144* | -0.020 | 0.092 | 0.111 |
| | (0.086) | (0.085) | (0.069) | (0.075) |
| $R$-squared | 0.688 | 0.869 | 0.114 | 0.619 |
| $\delta$ (13-16 years) | | -0.119 | | 11.317 |
| Observations | 7,229 | 7,229 | 7,141 | 7,141 |
| | Panel B: Tanzania | | | |
| Effect of preschool | | | | |
| at ages 8-9 | -0.115*** | -0.147*** | 0.108*** | 0.093*** |
| | (0.030) | (0.041) | (0.028) | (0.032) |
| at ages 10-12 | -0.154*** | -0.088** | 0.073*** | 0.111*** |
| | (0.034) | (0.038) | (0.023) | (0.022) |
| at ages 13-16 | 0.110*** | 0.083** | 0.128*** | 0.120*** |
| | (0.042) | (0.038) | (0.022) | (0.024) |
| $R$-squared | 0.622 | 0.831 | 0.072 | 0.637 |
| $\delta$ (13-16 years) | | 1.150 | | 4.553 |
| Observations | 29,981 | 29,981 | 30,427 | 30,427 |
| | Controls included in panels A and B | | | |
| Age × cohort effects | Yes | Yes | Yes | Yes |
| District fixed effects | Yes | No | Yes | No |
| Socio-demographics | No | Yes | No | Yes |
| Early-life conditions | No | Yes | No | Yes |
| Household fixed effects | No | Yes | No | Yes |

*Notes:* The table reports regression estimates that provide the inputs into the computation of Oster's (2017) $\delta$, that is, the ratio of the impact of unobservables to the impact of observable controls that would drive the coefficient on preschool attendance for 13- to 16-year-olds to zero. For details on the underlying method, see text and Oster (2017). For this analysis, the sample is restricted to households with variation in preschool attendance. The regressions are otherwise identical to those in columns 1 and 4 in Table 3 (columns 1 and 2 in the current table) and columns 1 and 4 in Table 4 (columns 3 and 4 in the current table). To calculate $\delta$, we use the Stata command `-psacalc-`, setting the maximum achievable $R$-squared ('*Rmax*') to 1.3 times the $R$-squared in the regression with household fixed effects (and at most 1). * p<0.10, ** p<0.05, *** p<0.01.

## Appendix Table 1

**Grade progression, enrollment, and literacy and numeracy skills, by age**

| | Highest grade attended | Currently enrolled | Still in preschool | Raw numeracy score | Raw English lit. score | Raw Swahili lit. score | Has 2nd-grade lit./num. |
|---|---|---|---|---|---|---|---|
| | | | | Panel A: Kenya | | | |
| Age 7 | 1.49 | 1.00 | 0.13 | 3.33 | 2.00 | 2.01 | 0.14 |
| Age 8 | 2.27 | 1.00 | 0.06 | 4.12 | 2.43 | 2.46 | 0.26 |
| Age 9 | 3.00 | 1.00 | 0.03 | 4.70 | 2.82 | 2.87 | 0.40 |
| Age 10 | 3.80 | 1.00 | 0.02 | 5.05 | 3.10 | 3.15 | 0.51 |
| Age 11 | 4.66 | 0.99 | 0.00 | 5.40 | 3.36 | 3.41 | 0.65 |
| Age 12 | 5.39 | 0.99 | 0.00 | 5.56 | 3.53 | 3.57 | 0.73 |
| Age 13 | 6.17 | 0.99 | 0.00 | 5.70 | 3.66 | 3.70 | 0.80 |
| Age 14 | 6.83 | 0.98 | 0.00 | 5.75 | 3.75 | 3.78 | 0.84 |
| Age 15 | 7.42 | 0.97 | 0.00 | 5.78 | 3.80 | 3.83 | 0.88 |
| Age 16 | 8.15 | 0.95 | 0.00 | 5.81 | 3.84 | 3.85 | 0.90 |
| | | | | Panel B: Tanzania | | | |
| Age 8 | 1.73 | 0.98 | 0.04 | 3.09 | 1.01 | 1.64 | 0.10 |
| Age 9 | 2.43 | 0.98 | 0.02 | 3.71 | 1.29 | 2.06 | 0.18 |
| Age 10 | 3.19 | 0.98 | 0.01 | 4.17 | 1.58 | 2.41 | 0.27 |
| Age 11 | 3.96 | 0.97 | 0.01 | 4.62 | 1.87 | 2.75 | 0.39 |
| Age 12 | 4.73 | 0.96 | 0.00 | 4.88 | 2.11 | 2.97 | 0.46 |
| Age 13 | 5.55 | 0.94 | 0.00 | 5.12 | 2.38 | 3.20 | 0.56 |
| Age 14 | 6.19 | 0.91 | 0.00 | 5.30 | 2.65 | 3.35 | 0.64 |
| Age 15 | 6.71 | 0.86 | 0.00 | 5.39 | 2.81 | 3.44 | 0.69 |
| Age 16 | 7.28 | 0.82 | 0.00 | 5.47 | 2.99 | 3.50 | 0.73 |

*Notes:* The table reports means of the variables indicated in the column heads across children of the age indicated in rows. Raw numeracy scores range from 0 to 6. Raw English and Swahili literacy scores range from 0 to 4. For definitions of all other variables, see the notes to Table 1 and Online Appendix A. Note that the figures reported here can differ from the enrollment statistics reported in Section 2 of the paper because those statistics are based on the unrestricted sample of children in the Uwezo data.

## Appendix Table 2

**Reported preschool attendance by cohort and wave**

| Wave: | 2011 | 2012 | 2013 | 2014 |
|---|---|---|---|---|
| | Panel A: Kenya | | | |
| Cohort | | | | |
| 1997 | | | 0.83 | |
| 1998 | | | 0.84 | 0.83 |
| 1999 | | | 0.84 | 0.84 |
| 2000 | | | 0.83 | 0.84 |
| 2001 | | | 0.84 | 0.85 |
| 2002 | | | 0.85 | 0.84 |
| 2003 | | | 0.84 | 0.85 |
| 2004 | | | 0.85 | 0.84 |
| 2005 | | | 0.85 | 0.85 |
| 2006 | | | 0.87 | 0.85 |
| 2007 | | | | 0.85 |
| | Panel B: Tanzania | | | |
| Cohort | | | | |
| 1995 | 0.57 | | | |
| 1996 | 0.59 | 0.58 | | |
| 1997 | 0.59 | 0.58 | 0.60 | |
| 1998 | 0.61 | 0.60 | 0.61 | |
| 1999 | 0.63 | 0.61 | 0.62 | |
| 2000 | 0.65 | 0.64 | 0.64 | |
| 2001 | 0.66 | 0.65 | 0.63 | |
| 2002 | 0.69 | 0.66 | 0.65 | |
| 2003 | 0.69 | 0.67 | 0.64 | |
| 2004 | | 0.68 | 0.65 | |
| 2005 | | | 0.64 | |

*Notes:* The table shows the fractions of children reporting to have attended preschool by country, cohort, and Uwezo survey wave. The sample is restricted to districts that were visited in all waves of the Uwezo survey. We disregard the 2014 wave in Tanzania because only a subsample of districts were sampled (45 districts versus more than 120 districts in the three previous waves). In the raw data, we observe level shifts in preschool attendance rates for all cohorts between some of the waves, likely because the question asking about preschool attendance changed. The table therefore shows regression-adjusted attendance rates after taking out wave fixed effects.

## Appendix Table 3

**Results for the full sample vs. the sample of households with within variation in preschool attendance**

| | Highest grade attended | | | Composite test score | | |
|---|---|---|---|---|---|---|
| | Main sample | Sub-sample | $p$ (equal effects) | Main sample | Sub-sample | $p$ (equal effects) |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Panel A: Kenya | | | | | |
| Effect of preschool | | | | | | |
| at ages 7-9 | -0.336*** | -0.423*** | 0.121 | 0.042*** | 0.023 | 0.670 |
| | (0.021) | (0.051) | | (0.015) | (0.041) | |
| at ages 10-12 | -0.123*** | -0.090* | 0.547 | 0.114*** | 0.165*** | 0.283 |
| | (0.021) | (0.049) | | (0.015) | (0.041) | |
| at ages 13-16 | -0.023 | -0.020 | 0.967 | 0.125*** | 0.111*** | 0.785 |
| | (0.021) | (0.047) | | (0.015) | (0.039) | |
| Observations | 218,728 | 7,229 | | 218,134 | 7,141 | |
| | Panel B: Tanzania | | | | | |
| Effect of preschool | | | | | | |
| at ages 8-9 | -0.212*** | -0.147*** | 0.069 | 0.108*** | 0.093*** | 0.589 |
| | (0.013) | (0.033) | | (0.008) | (0.021) | |
| at ages 10-12 | -0.074*** | -0.088*** | 0.625 | 0.113*** | 0.111*** | 0.889 |
| | (0.011) | (0.023) | | (0.006) | (0.015) | |
| at ages 13-16 | 0.096*** | 0.083*** | 0.602 | 0.081*** | 0.120*** | 0.034 |
| | (0.010) | (0.022) | | (0.006) | (0.014) | |
| Observations | 284,396 | 29,981 | | 288,084 | 30,427 | |
| | Controls included in panels A and B | | | | | |
| Age × cohort effects | Yes | Yes | | Yes | Yes | |
| Socio-demographics | Yes | Yes | | Yes | Yes | |
| Early-life conditions | Yes | Yes | | Yes | Yes | |
| Household fixed effects | Yes | Yes | | Yes | Yes | |

*Notes:* The table shows estimates of the impact of preschool attendance on highest grade attended and the composite test score, separately for the full sample used in the main analysis and for the restricted sample of households with within variation in preschool attendance. Columns 1 and 4 re-estimate the main specifications of Tables 3 and 4. Columns 2 and 5 present the corresponding estimates for the restricted sample. Columns 3 and 6 present $p$ values from Wald tests of the hypothesis that the age-group specific effects in the two columns to the left are equal. These cross-specification tests are conducted using seemingly unrelated regression using Stata's `-suest-` command. Due to computational constraints, rather than estimating the household fixed effects models directly, we first demean the data using `-xtdata-`, and then estimate regressions that exclude the household fixed effects on these data. This results in the same coefficient estimates, but lower standard errors. This in turn increases the chance of rejecting the null of equal coefficients in the Wald tests, which means that we err on the conservative side in our interpretation of the $p$ values.