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Effects of speed-schools in Niger☆

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A B S T R A C T

We evaluate a two-year accelerated education program in Niger. We use a two- phase experimental design where we first randomize the accelerated schools at the community level, and second, within treated communities, we randomize the available slots among interested participants. The program affected education and learning: Almost three times more treated children start lower secondary education (our main educational outcome), and they are more likely to be literate and numerate. Yet, most children still do not have basic reading and math skills after the program. We find no indications of spillover effects of the program. In particular, control children in treated communities are very similar to children in control communities at endline. Despite the effects on educational and learning outcomes, we find no effects on any of our other five main pre-registered outcomes: Beliefs about the appropriate marriage age and gender equality, well-being, support for violence, or engaging in hazardous work.

1. Introduction

While school attendance has dramatically improved in many of the world's poorest countries over recent decades, unfortunately, this has not translated into large learning gains. Many children, despite years of schooling, remain illiterate and innumerate (Glewwe and Muralidharan, 2016). The causes of this "learning crisis" are multifaceted, ranging from a low demand for education and insufficient educational resources, to a shortage of competent teachers (World Bank, 2018; Kremer et al., 2013; Pritchett , 2013; Fazzio et al., 2021). The situation is particularly grim in Niger, where less than half of primary school-aged children are enrolled in schools. Moreover, the quality of learning is poor, with less than 10 percent of sixth graders possessing reading and counting skills (World Bank, 2018). This predicament has left millions of older children behind, a situation that needs urgent rectification even if attendance and learning rates for younger children were to improve.

One approach to integrating older children, who have either never enrolled, dropped out, or did not learn enough in primary school to qualify for secondary school, involves admitting them into'speed-schools' or accelerated education programs. These programs vary in form but universally feature condensed schooling, with the aim of preparing learners for advanced educational stages in less time. We study a two-year speed-school program in Niger that caters to rural out-of-school children aged between 12 and 14, who have never enrolled or prematurely left primary education without qualifying for further education. The program covers the first six years of primary school curriculum within two years, intending to provide these children with a second chance to catch up and integrate into formal schools.

We closely collaborate with the Strømme Foundation, an NGO spearheading speed- schools in the Sahel region, to study the impacts of their speed-schools using a two- stage randomization procedure. We administer treatment randomization at both the community (across 85 communities) and individual levels (across 1,786 children in treated communities). This process creates 'treatment' and 'control' communities, as well as 'treatment', 'spillover', and 'pure control' children within these communities.

We find effects on both education and learning outcomes around 2.5 years after the intervention started. Whereas only 12 percent of the control group children in the treatment communities advance to lower secondary education, this number increases to 35 percent for the treated children. The offer to attend speed-school led to a 129 percent increase in literacy rates, rising from 14 to 32 percentage points. Furthermore, numeracy skills were enhanced. We did not observe any negative or positive spillover effects on either schooling or learning within the

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treated communities. We find clear treatment effects for all levels of previous education of the children and no statistically significant heterogeneity. On the one hand, the impact on learning was so pronounced that children who were offered two years of speed-school education on average demonstrated superior skills in mathematics, reading, and writing than those in the control group with six years of primary education. On the other hand, the large relative gains of the program are driven by the exceptionally poor outcomes for the control children, and most children in the treatment group can still not read, write, or count after the program ended.

Our primary contribution is to the literature on enhancing educational outcomes for children in impoverished settings. Specifically, speed-schools provide an avenue to catch up and reintegrate into the formal education system for children who would otherwise have few other opportunities. While speed-schools are becoming increasingly prevalent in Sub-Saharan Africa, there are very few rigorous evaluations of these initiatives. Two notable exceptions are Akyeampong et al. (2018) and Diazgranados-Ferráns et al. (2022). Akyeampong et al. (2018) evaluate a 10 month speed school program in Ethiopia that targets children aged 9–14. They have longitudinal data over 6 years and compare children in the speed school with children in other schools. They find that attendance and learning were higher for the speed school children. There are, however, also other concerning differences over time for the different groups of children. For instance, the relative household assets and livestock improvements for the speed school children households were very large during this period. Diazgranados-Ferráns et al. (2022) investigate the effects of a nine month speed-school on 1700 children aged between 9 and 14 in Nigeria. The study design was an individual-level RCT, but the low adherence to the assigned treatment conditions resulted in an imbalance in the samples based on e.g., prior education levels. Controlling for baseline imbalances, in particular in a difference-in-differences analysis, revealed positive program effects on both numeracy and literacy, albeit smaller than the effects we observe. We contribute to this small literature by investigating a longer program as well as by investigating spillover effects and effects on other important outcomes. Measuring spillover effects is vital for ensuring the accuracy and validity of the findings, understanding the full impact of the intervention, and for ethical reasons as some interventions might inadvertently harm non-target individuals. There may be particularly strong spillovers across siblings if parents diversify human capital investments within the household or if they are only able to allow some of their children to attend school (Lilleør, 2008).

We also contribute to the literature on remedial education in developing countries. Banerjee et al. (2007) evaluated the Pratham model, which, similar to the speed-school program we study, employs educated individuals from the local community and ensures a low student–teacher ratio to provide teaching at an appropriate level. A notable distinction, however, is that this model targets children who are lagging behind in their current studies, as opposed to our focus on out-of-school children. Another related remedial education program is the one in Niger studied by Aker and Ksoll (2019), who focused on adult education with participants having a mean age of 35 years old. In a cluster RCT at the community level, they found that the program increased math and reading test scores by around 0.2 standard deviations.

Previous literature demonstrates that targeted instruction and structured lesson plans, both integral components of our program, are effective in low-income contexts (Angrist and Meager, 2023; Angrist et al., 2020), albeit expensive. Our intervention costs the NGO roughly 350 USD per child per year, a figure that can be compared to the average annual per pupil expenditure of 279 USD in Sub-Saharan Africa at large. Two other recent successful interventions in Guinea Bissau (Fazzio et al., 2021) and Gambia (Eble et al., 2021), which report even larger learning improvements, had costs ranging from 240 USD in Gambia (supplementary to regular school costs as it was an after-school program) to over 400 USD in Guinea Bissau. Quality education indeed comes with a price tag, but these and, to some extent our, findings highlight that it is possible to accomplish significant advancements.

In addition to the results on educational outcomes we also test whether the program had any effects on other variables that are often hypothesized to be affected by education (such as outcomes related to gender equality, early marriage, and well-being) or that are particularly salient in our context (such as support for violence or hazardous child work). We find very limited effects on these outcomes.

2. Context and intervention

Located in West Africa, Niger is a Francophone country with school enrollment and completion rates among the lowest worldwide. Although households in Niger can formally enroll their children in primary school without charge, the reality often includes some ancillary school-related expenditures, in addition to the opportunity costs of children's time (Bagby et al., 2016). While education is officially mandatory for children aged 7 to 12, this law has not been consistently enforced, especially in rural areas. Niger faces challenges with education due to a rapidly increasing population growth (3.7 %) combined with erratic and slow economic growth, making it difficult to train more teachers and build more schools (World Bank, 2023).

Niger has long held the unfortunate last position among the 189 countries in the UNDP Human Development Index, with education being a significant contributing factor to its low ranking. Dosso and Tillaberi, the regions targeted by our intervention study, suffer from extreme deprivation, characterized by a child mortality rate of 123 per 1000, stunting rates of 47 percent, and acute malnutrition affecting almost 11 percent of children under five (DHS, 2012; INS, 2022, 2023).

Formal schools' learning outcomes are disheartening, with very few students exiting primary school having achieved minimum proficiency in reading and mathematics (UNESCO, 2021). In such a context, encouraging early teenagers to return to school is challenging.

The intervention offers "speed-schools" or accelerated learning programs, specifically catering to out-of-school children who surpass the usual primary school age. These speed- schools aim to bridge the education gap for these children by providing a condensed form of the primary curriculum. The particular program we study, termed Stratégie de Scolarisation Accélérée 2 (SSA2), distills the content of grades 1–6 into an intensive two-year course. The focus of the pedagogy is to have small enough classes to enable individually adapted teaching at the correct level. Classes are held 6 days a week. The total teaching load amounts to 34 h per week, out of which 24 h focus explicitly on Math and French and the rest on Social Science, Science, and Arts. The Social Science curriculum also covers human rights, including support for democracy, gender equality, and religious freedom. As opposed to in the regular schools, where teaching is in French only, teaching in other subjects in the speed schools starts in the mother tongue for the first two months before moving over to French only.

The SSA2 model of accelerated learning is designed to be sustainable and scalable within the context of a low-income economy. The program has been designed to avoid creating a parallel structure to the existing formal school system, which could potentially result in children opting out of public schooling. To prevent children from dropping out of formal school to join the speed-school, the program is not run consecutively in the same community.

The program employs an "animator" instead of a licensed teacher, deliberately avoiding draining valuable teacher resources from public schools. These animators are ideally local individuals who have reached the tenth grade of the formal education system. This approach taps into under-utilized local intellectual resources. The animators first receive one month of initial training before the first year and a one week refresher training the following year. There are also three shorter pedagogical meetings per year focusing on capacity building. The animators are paid around 50 percent more than contractual teachers in regular schools, but the work is temporary in nature as the school moves after two years. There are few problems of teacher absenteeism as teachers are monitored by community members and by the Strømme foundation conducting random visits.

The program tries to minimize alternative costs to society. The classrooms are makeshift structures constructed from local materials like branches, built by the local population. These classrooms can be dismantled during the rainy season and reconstructed for the second year of the program, with the materials being stored and recycled, thereby avoiding the need for permanent, solid structures that could be otherwise utilized. This helps in keeping capital investments minimal.

The main costs of SSA2 are in monitoring and supervision structures, as travel is time and gas-intensive in rural areas. The Global Education Monitoring Report estimates that the per-pupil and year cost for primary school children in Sub-Saharan Africa is estimated to be 279 USD. Given that SSA2 targets older and particularly disadvantaged children in rural areas the costs were expected to be higher and are estimated to be 350 USD per year. It is, however, difficult to know how to compare an intervention that is supposed to cover six years with numbers for single years of non-quality education.

The SSA2 project model has several objectives, each designed to address specific social and cultural challenges in the region:

1. **Boosting Education and Learning:** The primary goal of SSA2 is to enhance educational outcomes and increase enrollment in lower secondary education, particularly emphasizing on girls' education.
2. **Reducing Early Marriages and Pregnancy:** The program aims to address the prevalent issue of early marriages and pregnancies among girls. According to DHS (2012), nearly 80 percent of girls in Niger are married before the age of 18, and 24 percent before the age of 15. Secondary school attendance has been shown to reduce female fertility in Ghana and Kenya (Duflo et al., 2021, 2015) and it is possible that this primary school intervention affects perceptions of the appropriate marriage age.
3. **Reducing Hazardous Child Labor:** SSA2 seeks to curb child labor, which, as per our data, implicates approximately 20 percent of the children in the control group.
4. **Preventing Youth Recruitment into Violent Groups:** SSA2 intends to provide an alternative path for youth and reduce their vulnerability to recruitment by violent groups prevalent in the region. Niger, along with neighboring countries, has witnessed a surge in armed attacks often initiated by misguided individuals invoking religious motives, frequently labelled as jihadists.

In addition to these explicit goals, we also add the following goals:

5. **Improving Gender Attitudes:** The program aims to foster more balanced gender attitudes, as over 60 percent of children in control communities concur with the idea that it is more important to send boys to school than girls.
6. **Enhancing General Wellbeing:** By creating access to quality education, the program seeks to improve the overall wellbeing and self-esteem of children.

3. Randomization and sampling

It is important to rigorously evaluate educational interventions, particularly in the context of the ongoing learning crisis. We posit that randomization is ethically justified, as it does not reduce the available spots in schools, but rather ensures children of comparable need are granted an equal opportunity for program participation. Ex-ante, it was furthermore not certain that SSA2 would be successful in teaching valuable skills to the children and it was uncertain whether the benefits exceeded the opportunity costs.

The timeline of the experiment, the different randomizations, and the flow of children in the different groups are presented in Fig. 1. We conducted a listing exercise to map the need in different communities in the two regions. Families in 104 communities were listed based on the following criteria: household within what is locally described as the "academic distance" from the community center/prospective Speed-school location (5 km), age (around 12–14),[1] out of school, willingness to go to school, parental support and consent. We did not list children that were not willing to go to school or that were from families not wanting the children to attend. The listing criteria did not explicitly include the number of years of previous school attendance. As a result we have around 10 percent of the sample that state that they have more than six years of education (see e.g. Appendix Fig. A.2). The most likely reason for these children being out of school is that they did not qualify for secondary school and that the years also include grade repetition and non-formal schools. In total, 8507 individuals were listed and this includes some parents and siblings of the eligible children.

Based on the lists we selected 85 communities that had sufficiently many children and carried out the randomization. Among the 3,216 eligible individuals we randomly selected a subset that we interviewed based on power calculations and resources. The experiment is randomized at two different levels:

Randomization across communities: Our experiment takes place in 85 communities in two regions. 30 communities were randomized to treatment and 55 to control. It was decided that there would be 15 treated communities in each of two regions covered (Dosso and Tillaberi) and therefore randomization to treatment and control communities were done within regions. We show the location of the communities in Appendix Fig. A.1. In the control communities we surveyed 1,159 children at baseline.

Randomization across individuals: In the treatment areas we had larger samples to allow for comparisons of individual treatment and control groups. We surveyed 1,489 children at baseline. The schools want around 28 individuals per school and since treatment compliance was expected to be imperfect we randomized around 35 individuals to treatment. Randomization to the program at the individual level was stratified by gender.

We collected baseline data in August to November 2018. Baseline interviews in the treatment communities were blinded and treatment status was revealed after data collection in the community was finished. Treatment started shortly after baseline data were collected. The intervention was completed in June 2020 and the second round of data collection took place during December 2020-June 2021.

4. Main outcomes

A goal of the evaluation is to provide a comprehensive picture of the impacts of speed- schools. We therefore collect data on a large number of outcomes and we report the full breadth of the evidence. That being said, power considerations forced us to limit the number of main hypotheses tested. We group the outcomes into 6 groups and for each group we have either an index or a main variable to be tested. For each topic we also registered a number of secondary analyses that can be seen in the pre-analysis plan (added to the Appendix). We only present the results of these secondary analyses in the main paper if there are effects on the main outcomes.[2] We chose the following main groups with the corresponding main variables in italics:

1. Schooling and learning outcomes. *Starting lower-secondary education.*
2. Marriage and fertility. Belief about *Appropriate marriage age*.
3. Support for violence. An index of *Support violence*.
4. Self-esteem and wellbeing. A *Rosenberg index* of self-esteem.

[1] Out of the 3,216 listed eligible individuals 108 were outside of this age category.

[2] We follow the suggestions of Banerjee et al. (2020) and present the full set of results in a populated pre-analysis plan, available at https://andreaskotsadam.wordpress.com/wp- content/uploads/2024/06/populated-pap-ssa-niger.pdf.

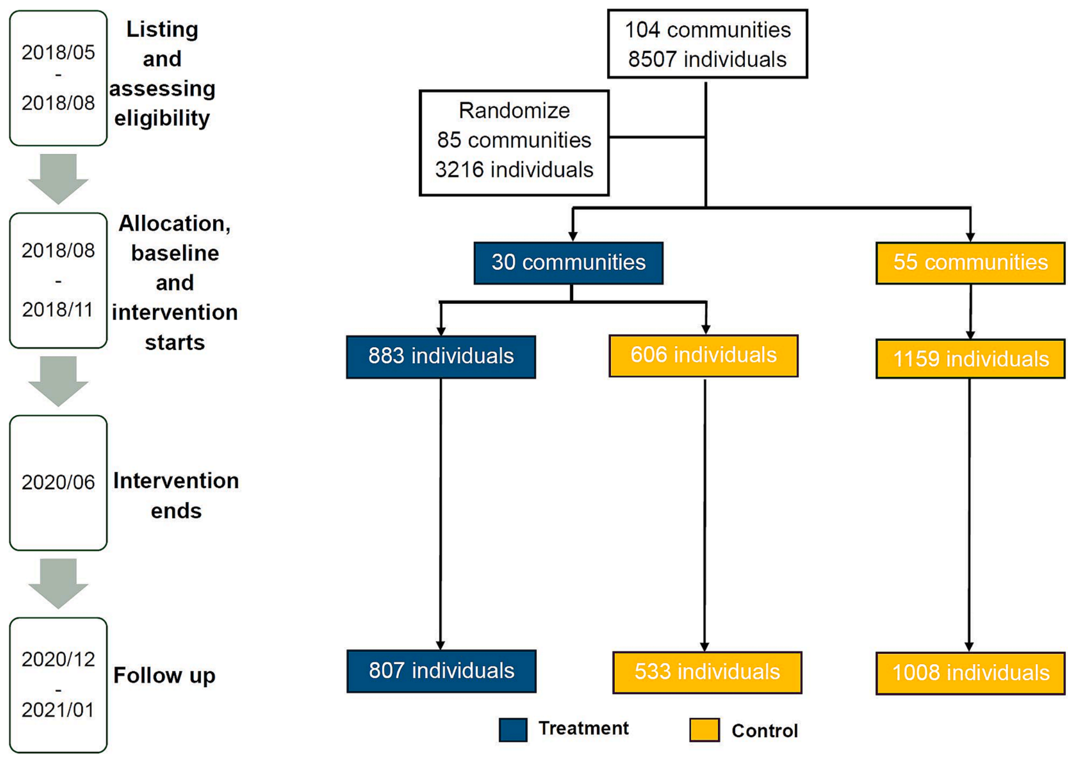

Fig. 1. Timeline and flow diagram of control and treated children.

5. Hazardous child labor and time use. Engagement in *Hazardous child labor*.
6. Gender attitudes. A *Gender equality index*.

We describe the coding of the variables in detail in Appendix Section A.1 but we here give an overview of the main variables. The overarching program goal of SSA2 is for out-of-school children to transfer back into lower secondary level of the traditional schooling system in Niger. Our main measure for the educational outcomes is *Starting lower-secondary education*. We also analyze a number of secondary variables for education and learning such as *Qualified for secondary*, *Years of schooling*, and *Highest grade level*.

We have several variables intended to measure learning. *Illiterate* is a measure based on self reported illiteracy and, if the child answers that they can read and write, we still code them as illiterate if they cannot write their own name on a piece of paper we provide them or if they cannot read the sentence "Fatima likes going to school, but she also likes to help her father". The sentence was invented by the research team and did not feature in any education material at the schools. *Numeracy index* ranges from 0 to 4 where one point is given for being able to solve each of the problems we give them on addition (77 + 11=?), subtraction (49–12=?), division (28/4=?), and multiplication (4x5=?). As with the literacy question, the teachers did not know what we asked the children.

There are several weaknesses with our measures of learning outcomes. First of all, they are not standard and therefore difficult to compare to other studies. Secondly, they are not measuring adequate learning after level 6 in school. In fact, our measures are much more lenient. For instance, in papers using EGRA and EGMA to determine literacy, the threshold is usually 45 words read per minute and 80 percent of reading comprehension questions answered correctly. Our literacy variable should therefore rather be seen as basic reading ability. Using the Global Proficiency Framework (GPF) for math and literacy (UNESCO, 2019), our measures correspond more to grade 2 and 3 than to grade 6. Thirdly, measuring numeracy skills as an index is somewhat dissatisfying as the measure is not continuous. Going from being able to add and subtract to doing multiplication is not the same as going from only being able to add to also being able to subtract. We therefore also show the individual level effects on the individual components of the numeracy index.

5. Empirical strategy and descriptive statistics

Our main empirical strategy is intended to capture reduced form (intention to treat) individual treatment effects, spillover effects, and effects at the community level. In Section A.4 we also present treatment effects on the treated by means of instrumental variables estimation.

5.1. Individual level effects

For the analysis of individual level treatment effects we restrict the sample to the treated communities and estimate the following regression:

$$Y_{2i} = \beta \cdot Treated_i + \lambda \cdot X_{1i} + \varepsilon_i \tag{1}$$

where 2 indicates follow up and 1 indicates baseline, i indexes individuals. The vector X always includes fixed effects for the strata variables gender and community. In addition we investigate if we can improve precision in the estimates by including the controls listed in Table 1 and by picking optimal controls from the total list of controls using LASSO (Belloni et al., 2014).[3] If we have missing values on explanatory variables we code the variables as zero and include dummy variables controlling for missing status so that we do not lose observations. We use robust standard errors in all estimations unless otherwise

[3] See Appendix Section 0A.1 for coding of the variables. As there is no limitation on how many variables that can meaningfully be included in the LASSO regressions we also include baseline variables and household variables for the variables listed in Appendix Section 0A.1 when they exist.In addition we explore whether the variables on building material for the roof, floor, and walls help us in precision beyond the asset index. In total we include 43 covariates and their interactions.

Table 1
Descriptive statistics.

	All (1) Mean	(2) SD	Individual Treatment (3) Mean	(4) SD	Individual Control (5) Mean	(6) SD	Community Treatment (7) Mean	(8) SD	Community Control (9) Mean	(10) SD
Treatment and strata variables										
Treatment individual	0.34	(0.48)	1.00	(0.00)	0.00	(0.00)	0.60	(0.49)	0.00	(0.00)
Treatment cluster	0.57	(0.50)	1.00	(0.00)	1.00	(0.00)	1.00	(0.00)	0.00	(0.00)
Female	0.47	(0.50)	0.42	(0.49)	0.46	(0.50)	0.43	(0.50)	0.52	(0.50)
Main outcome variables										
Starting lower secondary	0.21	(0.40)	0.35	(0.48)	0.12	(0.32)	0.26	(0.44)	0.14	(0.34)
Appropriate marriage age	21.48	(4.04)	21.89	(3.78)	21.60	(4.28)	21.78	(3.98)	21.07	(4.08)
Support violence	0.45	(0.50)	0.46	(0.50)	0.47	(0.50)	0.47	(0.50)	0.44	(0.50)
Rosenberg index	19.96	(3.63)	20.02	(3.71)	20.10	(3.58)	20.05	(3.66)	19.84	(3.59)
Gender equality index	0.03	(1.01)	0.03	(1.02)	−0.00	(1.00)	0.02	(1.01)	0.04	(1.01)
Hazardous work	0.19	(0.40)	0.20	(0.40)	0.18	(0.39)	0.19	(0.40)	0.20	(0.40)
Other outcome variables										
Qualified for secondary	0.16	(0.37)	0.36	(0.48)	0.06	(0.24)	0.25	(0.44)	0.03	(0.18)
Numeracy index	1.08	(1.57)	1.56	(1.70)	0.87	(1.48)	1.28	(1.65)	0.82	(1.41)
Illiterate	0.80	(0.40)	0.68	(0.47)	0.86	(0.35)	0.75	(0.43)	0.86	(0.35)
Highest grade	4.46	(2.06)	4.64	(2.12)	4.32	(2.02)	4.53	(2.09)	4.36	(2.01)
Years of schooling	3.50	(2.80)	3.82	(2.80)	3.07	(2.76)	3.52	(2.81)	3.48	(2.80)
Social desirability	6.11	(1.60)	6.09	(1.61)	6.05	(1.62)	6.07	(1.62)	6.17	(1.59)
Baseline control variables										
Years of schooling	3.06	(2.65)	3.01	(2.66)	2.78	(2.63)	2.92	(2.65)	3.24	(2.64)
Desired fertility	7.71	(3.46)	7.73	(3.39)	7.71	(3.54)	7.72	(3.45)	7.69	(3.47)
Rosenberg index	20.05	(3.41)	20.10	(3.44)	19.84	(3.43)	20.00	(3.44)	20.12	(3.37)
Religious intolerance	2.58	(0.90)	2.59	(0.92)	2.55	(0.94)	2.57	(0.93)	2.60	(0.86)
Accept religious violence	1.93	(0.76)	1.98	(0.81)	1.90	(0.77)	1.95	(0.79)	1.91	(0.72)
Gender eq. work	0.39	(0.49)	0.35	(0.48)	0.40	(0.49)	0.37	(0.48)	0.41	(0.49)
Gender eq. abuse	0.34	(0.47)	0.33	(0.47)	0.34	(0.47)	0.34	(0.47)	0.34	(0.47)
Gender eq. school	0.45	(0.50)	0.44	(0.50)	0.44	(0.50)	0.44	(0.50)	0.47	(0.50)
Male hh head	0.85	(0.36)	0.87	(0.34)	0.86	(0.35)	0.87	(0.34)	0.82	(0.38)
HH asset index	−0.04	(1.05)	−0.06	(1.08)	−0.07	(1.11)	−0.06	(1.09)	−0.01	(0.99)
Age of hh head	49.14	(11.61)	49.20	(11.56)	48.81	(10.94)	49.05	(11.31)	49.26	(11.98)
N	2348		807		533		1340		1008	

Notes: The table shows means and standard deviations for our main pre-registered variables for the different samples. The samples for the individual treatment and control individuals are restricted to treated communities.

stated. As there is no general imbalance across treatment and control (see below), the estimation with only the strata variables is our main specification as pre-specified.

5.2. Spillovers and community level effects

The specification in (1) is a biased estimate of the overall effect in a community if there are spillover effects within communities. The question of spillovers is also interesting in itself. To estimate spillover effects we restrict the sample to control individuals and estimate the following regression:

$$Y_{2ic} = \beta \cdot TreatedCommunity_{ic} + \lambda \cdot X_{1i} + \varepsilon_{ic} \tag{2}$$

where the treatment community is randomly assigned at the community level (c). As treatment at the community level is randomly assigned within regions we always include Region as a strata variable in these regressions. We still estimate this regression using individual level data as power may be increased by including individual level controls. We do, however, cluster the standard errors at the community level (c) in these regressions. As the sample includes only non-treatment individuals, β identifies within-community spillover effects by comparing control individuals in treatment communities to control individuals in pure control communities. Note that we have not randomly assigned the treatment share in different communities so we can not investigate how the spillovers change with different intensities of the treatment without further assumptions (Baird et al., 2018). We also use specification (2) but without excluding any individuals to estimate the total effect of having a speed-school in the area.[4]

We also use a specification to jointly estimate the treatment and spillover effects as in Crépon et al. (2013). We estimate the following model:

$$Y_{2ic} = \beta \cdot TreatedCommunity_{c} + \gamma \cdot Treated_{i} + \lambda \cdot X_{1i} + \varepsilon_{ic} \tag{3}$$

where Treated Community$_c$ is a dummy variable indicating a treated community, and Treated$_i$ is a dummy variable indicating whether the participant i is treated, the vector of control variables includes the stratification variables Region and Female, and ε_{ic} is the error term clustered at the community level.

The parameter γ then gives the treatment effect of treated participants compared to control participants in treated communities, and β gives the spillover effect. The combination $\beta+\gamma$ gives a comparison between treated participants and pure control participants in control communities.

5.3. Descriptive statistics and balance tests

In Table 1 we show descriptive statistics for the whole sample at endline and the different treatment and control groups. We see that one

[4] In the pre-plan we also wrote that we would also add a dummy variable for being in the control group and interact Treated community with being in the control group. As there are no treated children in the control communities, however, the interaction terms drop out of the regression and no additional information is gained.

third of all the children are treated and that the share of treated in the treatment communities is 60 percent. While there are 30 treated communities and 55 control communities, 57 percent of the surveyed children live in treatment communities as we interview more children there to allow for individual level comparisons. The share of girls in the sample is 47 percent.

We have six main outcome variables. As these are measured at endline we expect there to be differences across samples if there are treatment (and spillover) effects. We see that 14 percent of the children in control communities are starting lower secondary education. This number increases to 26 percent in the treated communities. The other main outcomes are more similar across samples and we see that children think the appropriate marriage rate is around 21 years, that over 40 percent of the children are coded as being supportive of violence, that children on average score above the low self- esteem cutoff (which is a Rosenberg index score of 15), and that around 20 percent of the children are involved in hazardous work. As the gender index is standardized, the numbers are close to zero, but we know that over 60 percent of the children in the control communities do not disagree that sending boys to secondary school is more important than sending girls, less than 15 percent think that girls should be allowed to study in secondary school if it is far away, and around 70 percent agree that men should have priority to remunerated work when it is rare.

With respect to the other outcome variables we see that 3 percent in the control communities did the Government test and say that they qualified for secondary education. Hence, there are fewer that report being qualified than have started secondary education. Most children in the control communities are both innumerate and illiterate despite having more than three years of schooling on average.

Regarding the control variables, we see that at baseline the children want large families, with almost 8 children on average. We also see that a large majority of the children live with male household heads and these household heads are almost 50 years old on average.

To test for baseline balance between treatment and control groups, we estimate equation (3) for all control variables and present the results in Table 2. γ shows the main treatment effect and β shows the spillover effects. We see that most variables are uncorrelated with treatment status. The only exceptions are that treated individuals seem more likely to accept religious violence. In Appendix Table A.4 we follow the pre-analysis plan and regress $Treatment_i$ on the baseline control variables, both individually and jointly, while controlling for the strata variables. When we include all variables at the same time the F-tests indicate that the variables do not jointly predict treatment status. As per our pre-plan, we therefore view the randomization as successful in creating balance and our main estimations are the ones with only the strata variables as controls. We also show that the results are similar when we add the full vector of controls and when choosing optimal controls. In Appendix Tables A.5 and A.6 we show descriptive statistics and balance tests for the full baseline samples as well, noting that also these samples are well balanced.

6. Effects on main outcomes

In Table 3 we present the main results for all our main outcome variables, measured more than two years after the start of the intervention, using equation (3). Estimates using equations 1 and 2 are provided in the Appendix Section A.2.

We start with describing our the result for our main outcome for schooling and learning outcomes: *Starting lower secondary education*. These results are seen in column 1. The mean for the control group in the treatment communities is 0.12, that is, 12 percent of the control kids in the treatment communities start lower secondary education. The coefficient for *treatment individual* corresponds to γ in equation (3) and gives the individual level treatment effect of treated participants compared to control participants in treated communities. We see that there is an effect of individual treatment of 0.23. Hence, 35 percent of the treated kids start lower secondary and the effect is an 192 percent increase.[5] As we pre-specified six main outcome variables it is also comforting to see that our p-value, which is lower than 0.0001, is lower than the critical p-value from a Bonferroni correction (0.008).[6] This result thereby also survives a correction for multiple hypotheses.

The coefficient for *Treatment cluster* corresponds to β in equation (3) and gives the spillover effect in the treated communities. There is no evidence of spillover effects.[7]

Turning to our other main outcomes, we see in columns 2 to 6 of Table 3 that there seem to be very limited effects of the program.[8] The effects are not only mostly statistically insignificant, they are also small as compared to the means and standard deviations of the outcomes. In Appendix Table A.1 we present the results of the pre- specified estimations and we there also present Two One-Sided Tests (TOST) intervals. These intervals show what effect sizes can be rejected in two one-sided tests, as pre- specified. We note that we can reject small effects for the individual level estimates for all other main outcomes. We conclude that being assigned to SSA improved educational attainment but that it did not improve any of our other main outcomes.[9]

7. Additional effects on schooling and learning

As we find effects on *Starting lower secondary education* we move on to also present the results of some secondary analyses on schooling and learning. A fuller set of secondary analyses are available in the populated analysis plan. In Appendix Section A.4 we also discuss the effects of attending the speed schools but we note that this analysis is hampered by weak measures of program participation.

In Table 4 we turn to the treatment effects on learning outcomes. We see that there are clear effects on numeracy in column 1. The control group children manage to solve 0.84 of the four tasks (addition, subtraction, multiplication, and division) on average. This increases to 1.52 for the treated children. There are also effects on literacy as we see in column 2. In this context, 86 percent of the control children cannot read and write and this is reduced by 18 percentage points (21 percent) for the treated children. Another way to interpret these numbers is that the literacy rate increased by 129 percent from 14 to 32 percentage points by being offered a place in the speed-school.

Measuring effects on learning is important as attainment in general is sadly not very highly correlated with learning in most developing countries (Angrist et al., 2020). This is true also in our setting. Fig. 2 shows the relationship between baseline schooling and learning

[5] In Appendix Table A.7, we also run separate regressions by gender. This is seen in the "Gender" row at the bottom of the tables. We see that the effect is smaller for girls in absolute terms, but since the mean is also much lower the relative effect magnitude (238 percent) is much larger.

[6] We pre-specified to use the Benjamini-Hochberg procedure for the multiple hypothesis testing, which is equivalent to a Bonferroni correction with only one statistically significant coefficient.

[7] 7In Appendix Table A.9, we show the effects on three other measures of educational attainment and we see there are effects at both the individual and community level on being qualified for secondary school (panel a). There are also individual level effects on the highest grade attained and years of schooling but the effects at the community level are not statistically significant for these outcomes (panels b and c).

[8] In Appendix Table A.8 we also show the effects on the Rosenberg Index when we standardize it by the control group mean.

[9] There were no statistically significant effects for any variable when restricting the samples to either boys or girls, with the exception of there being a treatment effect on the gender equality index for girls that is statistically significant at the 10 percent level (see Appendix Table A.10). In Appendix section 0A.6 we see that there are no spillover effects on the siblings in terms of them being more likely to start or quit school, move, marry, or conduct hazardous work. Furthermore, there were no detectable heterogeneity in treatment effects on any of the main variables when using machine learning methods.

Table 2
Balance tests.

Variable	Years of school	Desired fertility	Ros. index	Rel. intol.	Accept violence	Gender work	Gender abuse	Gender school	Male head	HH asset index	Age of hh head
Treatment ind. (γ)	0.069	−0.022	0.096	0.0066	0.078***	−0.035	−0.018	−0.0077	−0.0074	0.0089	0.44
	(0.13)	(0.17)	(0.12)	(0.048)	(0.027)	(0.024)	(0.020)	(0.022)	(0.011)	(0.048)	(0.41)
Treatment cl. (β)	−0.33	−0.025	−0.069	−0.035	−0.024	0.013	0.0080	0.0016	0.029	−0.056	−0.71
	(0.25)	(0.19)	(0.23)	(0.061)	(0.048)	(0.031)	(0.037)	(0.032)	(0.024)	(0.081)	(0.60)
Contr. mean	3.15	7.70	19.98	2.59	1.90	0.40	0.34	0.46	0.84	−0.02	49.42

Notes: The table reports regression results from equation (3). All dependent variables are measured at baseline. γ measures the individual level treatment effect and β measures the spillover effect. The control mean is for all control individuals in both treated and untreated communities. All regressions include the strata variables, which are gender and Region. Robust SE in parentheses clustered at the community level.

Table 3
Joint estimation of treatment effects on the main outcomes.

	(1) Starting lower Secondary education	(2) Appropriate Marriage age	(3) Support Violence	(4) Rosenberg Index	(5) Gender Equality	(6) Hazardous Work
Treatment individual (γ)	0.23***	0.14	−0.015	−0.10	0.052	0.013
	(0.033)	(0.30)	(0.026)	(0.17)	(0.045)	(0.019)
Treatment cluster (β)	−0.022	0.31	0.037	0.27	−0.019	−0.022
	(0.026)	(0.33)	(0.029)	(0.33)	(0.056)	(0.023)
Female	−0.057***	−3.69***	0.00010	−0.14	0.31***	−0.14***
	(0.020)	(0.19)	(0.025)	(0.16)	(0.044)	(0.018)
Control mean	0.13	21.26	0.45	19.93	0.02	0.19
Control mean in treated areas	0.12	21.60	0.47	20.10	−0.00	0.18
N	2348	2134	2320	2348	2316	2348
R-squared	0.07	0.21	0.01	0.01	0.03	0.04
Controls	Strata	Strata	Strata	Strata	Strata	Strata
Gender	Both	Both	Both	Both	Both	Both
Sample	All	All	All	All	All	All
T ind. vs Pure C	0.00	0.07	0.48	0.59	0.56	0.68

Notes: The table reports treatment and spillover effects estimates on starting lower secondary education. All regressions control for strata fixed effects, which are gender and Region. The regressions follow equation (3) and the sample contains all individuals. P-values are $\leq 0.01^{***}$, $\leq 0.05^{**}$, and $\leq 0.1^{*}$. Robust SE in parentheses clustered at the community level.

Table 4
Joint estimation of treatment effects on learning outcomes.

	(1) Numeracy index	(2) Illiterate
Treatment individual (γ)	0.68***	−0.18***
	(0.12)	(0.033)
Treatment cluster (β)	0.025	0.011
	(0.12)	(0.028)
Female	−0.37***	0.073***
	(0.074)	(0.016)
Control mean	0.84	0.86
Control mean in treated areas	0.87	0.86
N	2348	2348
R-squared	0.07	0.06
Controls	Strata	Strata
Gender	Both	Both
Sample	All	All
P-values:		
T ind. vs Pure C	0.00	0.00
Treatment individual	(0.00000008)	(0.0000001)
FDR critical p-value	0.008	0.017

Notes: The table reports treatment and spillover effects estimates on starting lower secondary education. All regressions control for strata fixed effects, which are gender and Region. The regressions follow equation (3) and the sample contains all individuals. P-values are $\leq 0.01^{***}$, $\leq 0.05^{**}$, and $\leq 0.1^{*}$. Robust SE in parentheses clustered at the community level.

outcomes for control and treated children in the treatment communities.[10] Focusing first on the control children, we see that individuals without schooling are almost all illiterate which is not that surprising. More surprising is the fact that less than 30 percent of control individuals that have been to school for 6 years, and less than half of the control individuals who have attended more than 6 years, have basic reading and writing skills. With respect to numeracy, we see that control children having attended school for 6 y ears can still on average only complete around one of the four numeracy tasks. The solid horizontal lines in the figures show the levels of literacy and numeracy for treated children and we note that being offered to attend speed school for two years causes the children to be better educated than children that have attended primary school for six years in the control group. The dashed horizontal lines show the average levels in the control group. We show in Appendix Fig. A.4 that the results for starting secondary education follow the same pattern. We also see that there are clear treatment effects for all levels of previous education of the children and testing for heterogenous treatment effects by baseline years of schooling we find no statistically significant differences in the effects. Furthermore, there were no detectable heterogeneity in treatment effects when using the omnibus test proposed by Chernozhukov et al. (2018) in an honest random forest framework (Athey et al., 2019 Wager and Athey, 2018;).

In Fig. 3 we show the distribution of the numeracy index separately for treated and control children in treatment communities. We see that

[10] Appendix Figs. A.2 and A.3 show the distribution of baseline years of education as well as in the three categories for the treated and control children in the treated communities. We can not reject that the distributions are equal (p-values 0.45 and 0.97 respectively).

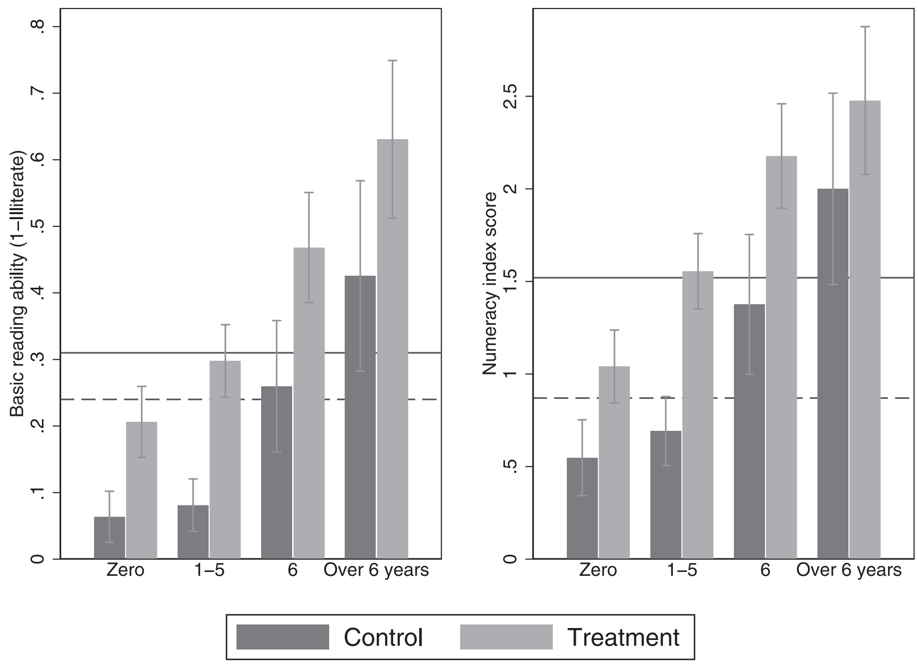

Fig. 2. Relationship between years of schooling and learning outcomes for control and treated children. *Notes:* The bars show the average levels of basic reading ability and numeracy for control and treated children in the treatment communities by years of previous education with 95 percent confidence intervals. The solid horizontal lines show the average levels for children that were offered to attend speed school and the dashed lines the corresponding average for the control group.

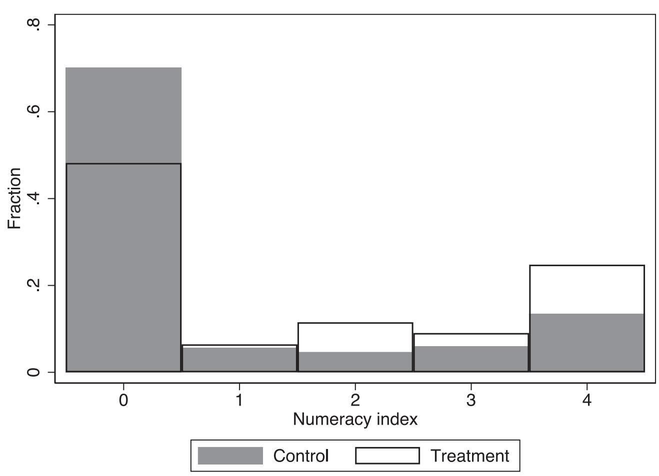

Fig. 3. Distribution of the numeracy index for control and treated children.

treated children are more likely to be able to solve 1, 2, 3, and especially all 4 tasks. In Appendix Fig. A.5, we show the corresponding, and very similar, results when we compare the treated children with children in the control communities.

Since there are problems with measuring the numeracy skills with our index (as discussed above), we also show the individual level effects on the individual components of the numeracy index in Table 5. We note that there are effects on all of them and that the effects on subtraction and multiplication are of over 100 percent. In Appendix Tables A.11 to A.14 we show the full tables for each of the numeracy variables and we note that the effects are often larger for boys than for girls, despite the means in the control groups already being higher for boys.

As the results in this section are based on secondary analyses we also correct for multiple hypothesis testing within this family of tests. In the tables we therefore present p-values and the critical thresholds accounting for false discovery rates when testing six different hypotheses. We see that the p-values are so small that the conclusions are the same also with these adjusted p-values.

Table 5

Joint estimation of treatment effects on different components of numeracy.

	(1) Addition	(2) Subtraction	(3) Multiplication	(4) Division
Treatment individual (γ)	0.21***	0.20***	0.15***	0.12***
	(0.035)	(0.034)	(0.034)	(0.028)
Treatment cluster (β)	−0.011	0.014	0.016	0.0062
	(0.035)	(0.032)	(0.034)	(0.028)
Female	−0.11***	−0.096***	−0.099***	−0.063***
	(0.022)	(0.021)	(0.021)	(0.017)
Control mean	0.26	0.21	0.22	0.15
N	2348	2348	2348	2348
R-squared	0.06	0.07	0.05	0.04
Controls	Strata	Strata	Strata	Strata
Gender	Both	Both	Both	Both
Sample	All	All	All	All
P-values:				
T ind. vs Pure C	0.00	0.00	0.00	0.00
Treatment individual	(0.00003)	(0.00005)	(0.0000003)	(0.0000004)
FDR critical p-value	0.05	0.042	0.025	0.033

Notes: The table reports individual level treatment effects estimates on different components of numeracy. All regressions control for strata fixed effects, which are gender and community. The regressions follow equation (3). P-values are ≤ 0.01*** , ≤ 0.05** , and ≤ 0.1* . Robust SE in parentheses.

7.1. *Additional results and robustness*

The results are similar both when adding the pre-specified vector of control variables and when using a double debiased LASSO approach to select optimal control variables. We show this in Appendix Tables A.15 and A.16 for starting lower secondary education but this conclusion holds for all variables investigated (see the populated pre-analysis plan).

In investigating attrition, we see in Table A.17 that there is a no statistically significant imbalance in attrition rates for treated and control individuals. We see that girls are less likely to attrit. The only variable that correlates with attrition when we add the full list of controls is years of schooling but the coefficient is very small. Taking the coefficient at face value it implies that children with one more year of schooling are 0.77 percent more likely to attrit.

In order to investigate social desirability bias, and more importantly, experimental demand effects we use a social desirability index (see Appendix Section A.5). The idea of the index is to see whether individuals are more likely to give saint-like answers if they are treated, which would then likely lead to self-reported measures being positively biased. Such worries are greater with reported attitudes than with outcomes from tests, but some of the educational outcomes are also self reported. As seen in Appendix Table A.22, there is no treatment effect on social desirability. Following Dhar et al. (2022) we also test for and reject that there are heterogeneous treatment effects based on the social desirability score (see Table A.24). The worrisome pattern would be if the treatment effects were driven by children with a high propensity to disingenuously give socially desirable answers and vanished for those with a low such tendency.

8. Conclusion

In the face of an ongoing learning crisis in low-income countries—-where numerous students fail to master basic literacy and numeracy skills even after six years of schooling—we have evaluated the impact of an ambitious program, designed to prepare children for secondary school in just two years.

Our findings reveal improvements in both schooling and learning. Where only 12 percent of control group children in the treatment communities progressed to lower secondary education, this figure rose to 35 percent for those in the treatment group. The program also increased literacy and numeracy. Notably, no negative or positive spillover effects on schooling or learning were identified in the treated communities. One interpretation of our results is that the relative effects are very large and impressive.

Considering that many of the treated children still cannot read and write, however, the program does not go very far in mitigating the learning crisis.

Beyond educational outcomes, we also examined whether the program influenced other factors often presumed to be affected by education—such as gender equality, early marriage, and well-being—as well as those specifically pertinent to our context, including support for violence and hazardous child labor. However, the impact on these outcomes was at most marginal. Most likely, effects on other aspects than learning need time to materialize and the hopes that education will be a short run silver bullet is likely unwarranted.

Given the low attendance rates and pervasive underperformance in formal schools, the need for catch-up programs targeting slightly older children is profound. The SSA2 program, costing the NGO 350 USD per child-year, is more expensive than the estimated average cost of 279 USD per child-year for primary education in Sub-Saharan Africa.

We strongly encourage further research into the effects of speed schools in different contexts, as well as the exploration of cost-saving strategies. With further validation and optimization, programs like SSA2 could be scaled up to address the significant educational gap in low-income countries more effectively.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jpubeco.2025.105307.

Data availability

Data will be made available on request.

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