Assessing the most effective ways to reduce early secondary school leaving: evidence from a field experiment in Burkina Faso[1]

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Abstract:

While major progress has been made in terms of primary school enrollment in most low- and middle-income countries in the past two decades, secondary schooling completion rates remain low in many settings. We conducted a small randomized controlled trial in Burkina Faso to assess the relative effectiveness of three potential interventions: i) information on returns to schooling; ii) tutoring and iii) cash incentives. We find that all three interventions substantially increased school enrollment and attendance, but only tutoring improved the proportion of students advancing to the next grade. Our results suggest that academic constraints are most binding in the studied setting, and that increasing school enrollment alone is not sufficient to ensure student progression. Further research will be needed to assess the most effective and cost-effective combination of the strategies explored in this study.

Keywords: secondary schooling, incentives, tutoring, dropout rates, Burkina Faso.

JEL codes: I25, D80, O1

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I. Introduction

Although access to primary schooling has improved dramatically over the past two decades in Sub-Saharan Africa, the transition to, and completion of, secondary education remains a challenge in many settings. According to the latest World Bank estimates, gross primary school enrollment was 102% in low- and middle-income countries overall in 2023, and 98% in Sub-Saharan Africa (World Bank, 2025). In the same year, gross secondary school enrollment was only 45% in Sub-Saharan Africa, and the proportion completing upper secondary school among individuals ages 20-24 remained below 30% (UNESCO Institute of Statistics, 2025).

A substantial body of literature has highlighted the high dropout rates in secondary school and linked them to financial constraints at the individual or household level (e.g., Behrman et al. 2005; Baird et al. 2011), weak academic preparation (e.g., Banerjee et al. 2017), and limited information about the economic returns to schooling (e.g., Jensen 2010; Nguyen 2008).

To assess the relative importance of these three mechanisms in rural Sub-Saharan Africa, we conducted a small randomized controlled experiment in Northern Burkina Faso, where previous research had documented a large majority of children dropping out of school during secondary education. We designed the experiment to evaluate the impacts of three interventions addressing the three main constraints identified in the literature on secondary school dropouts: (i) *an information intervention*, which provided students with locally validated data on the labor market and health returns to secondary school; (ii) *a conditional cash transfer (CCT)* program, offering USD 50 conditional on regular attendance and academic promotion; and (iii) *a personalized academic tutoring* program, delivered weekly by local tutors, and roughly equivalent to the cash

transfer in terms of the implicit transfer provided to students. Our primary outcomes include school attendance, dropout, and grade promotion over a one-year period. We find positive enrollment effects for all three interventions. The tutoring intervention increased the probability of not having missed more than two weeks of school in the 3-months period leading up to the final exam by 10.6 percentage points (s.e.= 4.9, $p = 0.043$). The cash intervention increased attendance by by 9.2 percentage points (s.e. = 4.2, $p = 0.045$), and the information intervention increased attendance by (statistically insignificant) 7.9 (s.e. =5.9) percentage points. Compared to the 7.2% of adolescents in the control group attending school in the same period, all three interventions resulted in more than a doubling of school enrollment rates. The relative magnitude of these effects is large and suggests that even low-cost informational nudges can lead to substantial behavioral adjustments in this population (Jensen 2010).

While all three interventions resulted in large increases in general school attendance, this increased attendance did not result in a higher proportion of students advancing to the next grade in the information only arm. In the control group and information arms, roughly 157% of students either passed the exam or were allowed to repeat the grade. In the cash arm, this proportion increased by 6.7%, but this difference was not significant. With tutoring, we see a 13.7%pt increase in the probability of being allowed to continue school, which roughly corresponds to doubling school continuation rates relative to the control. The relatively high rates of positive academic outcomes among the tutoring group are particularly striking given that less than half of the eligible students were able to receive tutoring. Due to continued local violence, coordinating tutoring sessions turned out to be a major challenge for our local study team. Although some of the tutoring sessions not delivered were likely due to lacking efforts made by students, we believe that larger impacts are likely in less complicated circumstances.

Nevertheless, the differences in the treatment effects on enrollment and school progression rates across arms are important, and clearly suggest that students at the margin may not easily be able to catch up with the academic requirements once they dropped out. Nudges and financial incentives may be sufficient to motivate them to come to school again, but additional academic support may be needed to reach the skill levels required for advancing to the next grade.

This study contributes to several strands of literature. First, it adds to a growing body of work on behavioral frictions in educational decision-making and the role of beliefs (Nguyen 2008; Jensen 2010; Dizon-Ross 2019). Given the large body of literature highlighting the strong causal effect of education on health and life expectancy (e.g. Fletcher et al, 2022, De Neve et al 2015), we provided recent dropouts both with information regarding long term health and on labor market returns in our study, which could potentially be more powerful than the promise of better jobs in rural settings where non-agricultural jobs are scarce. Second, it offers new causal evidence on the effects of CCTs in the under-studied context of secondary schooling in fragile states (Baird et al. 2011; Filmer and Schady 2009). Throughout the study period, the entire study area was heavily affected by violent attacks and insecurity, leading in quite a few cases to temporary school closures and high levels of insecurity. Third, it extends the literature on personalized instruction and educational support, demonstrating that even in highly resource-constrained settings, targeted tutoring can yield substantial gains (Banerjee et al. 2017; Evans and Popova 2016).

The rest of the paper is organized as follows. **Section 2** outlines the study context. **Section 3** describes the experiment, data and empirical approach and **Section 4** summarizes the main results. **Section 5** shows some additional supplementary results and **Section 6** concludes.

II. Study Context

Study Area

This field experiment took place in the Health and Demographic Surveillance System (HDSS) site of Nouna in the Kossi province in the north-west of Burkina Faso (Boucle du Mouhoun region). The Health and Demographic Surveillance System has existed since 1992 and currently covers a population of 105,000 habitants (Sié, et al., 2010). The HDSS site includes the semi-urban village of Nouna (29% of the population) as well as 58 villages (71% of the population) with a total of about 30 secondary schools. The mostly rural population of the study area consists predominantly of subsistence farmers and cattle keepers. The Dioula language serves as a lingua franca, permitting communication across ethnic groups. The Nouna Health and Demographic Surveillance System is operated by the Nouna Health Research Center (Centre de Recherche en Santé de Nouna - CRSN), which provided the necessary infrastructure to conduct the current study and implemented the interventions described below. Schools in the study area are considered potential targets and we therefore do not provide a map of the study area with the exact location of schools.

We chose the Nouna site for this project because the Health and Demographic Surveillance System allows for long-term follow up of our participants by leveraging their routine data collection of community members and has a long-standing collaboration with one of the project partners (University of Heidelberg, Germany).

Education Context

The public education system in Burkina Faso is based on the "6-4-3" system, including six years of primary education, four years of junior secondary school ("Collège"), and three years of

senior secondary school ("Lycée") (UNESCO Institute of Statistics, 2025). The school year in Burkina Faso typically starts in October and ends in May, followed by an exam period in June and July depending on the school year (Government of Burkina Faso, 2022). Mean years of schooling completed is one of the lowest in the world (2.3 total years of schooling completed among adults aged 25+; 1.6 among women and 3.1 among men in 2021). Access to school varies substantially by geographical region and is particularly low in rural areas. The junior secondary school completion rate, for instance, was 56% in the Centre region compared to 13% in the rural Sahel region in 2021 (Institut National de la Statistique et de la Démographie & The DHS Program, 2023). In principle, primary education and junior secondary education is free and compulsory until ages 16 years in Burkina Faso. In practice, however, parents face considerable school costs. In our experiment's baseline survey, for example, the average self-reported cost of attending secondary school was 38,000 CFA per year (about 65 USD) and ranged from 2,000 to 300,000 CFA per year depending on type of school (public vs. private secondary school) (**Figure S1** in the Appendix). Average class size in our sample, defined as the "number of children in your last class", was 62 children. The average distance between home and most recent school was 3.0 km and most common modes of transport to/from school were biking (59.6%) and walking (40.1%). The primary language of instruction at the secondary level is French. Political instability and insecurity have caused considerable disruption in educational processes. About one fifth of all deaths from terrorism globally have been suggested to occur in Burkina Faso. In 2021, about 3,000 schools in Burkina Faso were forced to close (Bene, 2022). In 2022, two coups d'état took place, the latest inflection points in Burkina Faso's political instability, causing further heightened uncertainty and school closures. A large portion of the country has been suggested to be outside of central government control (up to 40% in 2024 by some estimates)

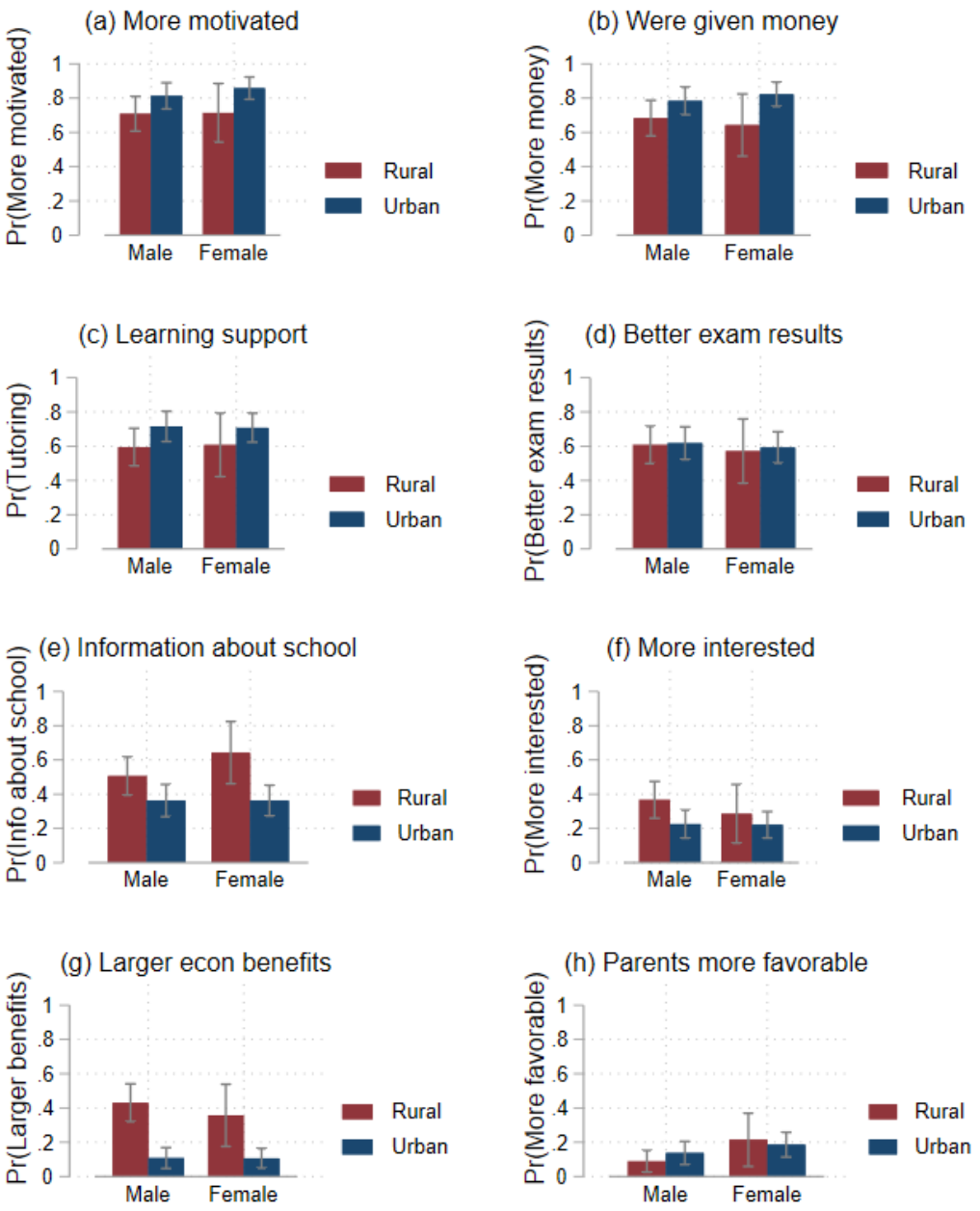
(Bertelsmann Foundation - Bertelsmann Transformation Index, 2024) and large areas are often cut off for extended periods or can only be reached with military escorts.

Reasons for Early School Leaving

To inform the design of our field experiment, we conducted formative qualitative research in the study area to elicit the perceived barriers to secondary schooling among adolescents, parents, teachers, school administrators, and other local stakeholders (Jabbarian, et al., 2022). Major reported barriers included school-related expenses, the lack of school infrastructure and resources, and insufficient French language skills. Among young women, forced marriages, adolescent pregnancies, and the low perceived economic benefits of secondary schooling were also reported as barriers. While respondents reported nearly universally short-term health benefits of schooling, responses with regard to economic benefits and indirect and long-term health benefits of secondary schooling were more ambiguous (Werner, et al., 2019). Overall, adolescents had only limited awareness of the long term benefits of secondary schooling, suggesting that interventions providing information on returns to schooling could effectively increase the perceived benefits of school attendance.

In **Figure 1**, we show factors related to schooling decisions using data from our quantitative survey at baseline, separately by gender and area of residence. Factors that would make children go back to school - with multiple responses allowed – were related to motivation (79.0%), financial reasons (75.6%), lack of learning support (67.0%), and academic performance (59.9%). In urban areas, factors were more commonly related to motivation, financial means, and learning support compared to rural areas. Conversely, in rural areas, factors were more commonly related to lack of information about secondary school and the low perceived economic benefits of staying in secondary school compared to urban areas.

Figure 1. Would you go back to secondary school if… (*N*=324)



Notes: Responses to the question "Would you go back to school if…" Outcomes were defined as probabilities and their 95% CIs for (a) being more motivated, (b) receiving money, (c) receiving learning support, (d) had better exam results, (e) obtaining more information about school, (f) being more interested in going back to school, (g) if there were larger economic benefits to going to school, and/or (h) if parents were more favorable of going to school. Respondents said yes/no to each category (reasons for going back were not mutually exclusive). All samples were stratified by gender and area of residence (urban and rural). Outcomes were reported by respondents (households). Complete case analysis using data from baseline survey. *N*=324.

The perceived benefits of education have also been suggested to be of importance for school attendance and performance in other settings. In a field experiment in Madagascar, for example, the provision of information on the benefits of schooling improved school attendance and increased investment in children's human capital (Nguyen, 2008). Similar results were found in Kenya (Buchmann, 2000) and Malawi (Grant, 2011). In a randomized controlled trial in Mexico, providing information on the economic benefits of schooling improved standardized test scores (Avitabile & de Hoyos, 2018).

III. Intervention and Study Design

Description of Interventions

The content and format of the interventions used in our experiment was based on our review of the published literature and formative research conducted in the study area. To further fine-tune our interventions and mobilize additional community support, we presented the study to a group of local and internatoinal, including Burkinabé government officials, different Health and Demographic Surveillance System sites, academic experts, as well as Burkinabé youth with contextual knowledge but who were not directly related to the study and/or residing in the study area.[2] After incorporating feedback, we finalized three locally relevant and culturally appropriate strategies that were employed to reduce early secondary school leaving (shown in **Box 1**): (i) information on the long-run benefits economic and health benefits of secondary schooling using data on the locally measured long-term benefits of secondary schooling; (ii) in-person tailored tutoring support continuously for up to five months preceding final exams (e.g., tutoring sessions in physics, mathematics, or languages); and (iii) economic support in the form of a cash payment

[2] Included the Minister of Education of Burkina Faso at the time of the study, the President of the Université Ouaga 1 Pr Joseph Ki-Zerbo, leadership of the Nouna HDSS and Ouaga HDSS, and several students.

of approximately 50 USD at baseline and an economic incentive of approximately 50 US at endline as a cash payment conditional on regular school attendance (100 USD in total). We provide additional details on each of these three strategies below.

Box 1. Strategies to reduce early secondary school leaving used in experiment

Strategy	**Description**
Information	Video on the long-term economic and health benefits of secondary schooling, as measured locally, supplemented with a one-page information sheet
Tutoring	Regular in-person one-on-one tailored tutoring support for up to 5 months provided by teachers or peers at school or in community context
Cash	About 50 USD at start of intervention and 50 USD conditional on not missing > 2 weeks of school unexcused between March 1st and May 31st, 2023

Notes: Box 1 shows strategies used in pilot field experiment. In addition to a control arm, we assessed providing information on the benefits of school, tailored tutoring, and economic support and incentives.

In a previous study, we had measured long-run economic and health benefits of secondary schooling in the study area (Werner, et al., 2022). To communicate this information in a simple and clear way, we developed and designed a new wordless animated video (2:19 min)[3]. Different versions were developed, and a final version was piloted and fine-tuned iteratively based on feedback. The video was delivered at the participant's home using the tablet of the field worker and shown typically once although participants could review the video and/or parts of the video if explicitly requested by the participant. At the end of the video, the participant received a one-page information sheet summarizing the video's main message in French [4]. Our hypothesis was

[3] In collaboration with Maya Adam (Stanford University) and animated by David Makadi Fwamba and Astride Animation. Music was provided by Florent Belemgnegré - an Ivorian-Burkinabé singer and musician known by his stage name as Floby. He sings in both French and Mòoré languages. The video can now be watched on YouTube at the following link. The video was delivered using the tablet of the field worker as well as a 1-page information sheet which summarized the economic and health benefits of staying in school in French.
[4] All participants indicated they could read and write in French in the baseline survey.

that information should remove any potential underestimation of schooling benefits. A recent review suggests this type of intervention to be highly cost-effective (Global Education Evidence Advisory Panel, 2021). Relatively little remains known, however, on the effect of providing information on the long-term economic and health benefits of education, particularly in settings of extreme poverty where the long-term benefits of schooling may be less well known and/or not prominent in the minds of adolescents and parents (Werner, et al., 2019).

Our second intervention was personalized tutoring. For this intervention, we provided regular in-person one-on-one tailored tutoring support to eligible participants for up to five months between January 1st, 2023, and May 31st, 2023. Tutoring sessions were provided at school or in the community by local teachers or older peers who were selected by the study team (**Figure S2**). Tutors were generally adult men who were members of the local community. Specific tutoring subjects, location, and duration of sessions were determined jointly by the participant and field worker. Tutors were subject-specific, and field workers retained tutors with a record of demonstrated expertise in each subject. Participants were offered up to 10 sessions in total for a total value of up to about 70,000 CFA (100 USD) per student over the intervention period. [5] We chose this amount to match the value of the cash transfer described in further detail below. Subjects included mathematics, sciences (e.g., physics, chemistry), languages (e.g., French, English, German), and life and earth sciences ("Sciences Vie et Terre"). Tutoring sessions were documented by field workers, including date of tutoring session, participant identifier, tutoring

[5] Tutors were paid up to about 100 USD divided by the number of hours and subjects preferred by the student. The minimum cost for a tutor per subject and hour was determined by the study team in Nouna and varied by subject. For example, one hour of tutoring in mathematics, physics, or chemistry was more expensive compared to one hour of tutoring in philosophy or French.

subject, as well as identifiers for the tutor and field worker. Participants did not receive information about the benefits of going to school or economic support (cash).

Third, we offered financial transfers, which were partially conditional and partially unconditional. To overcome initial credit constraints, all selected students obtained 35,000 CFA (about 50USD) at the start of the intervention. In addition, they were offered 35,000 CFA at the end of the intervention, conditional on regular school attendance. The USD 50 paid upfront was chosen such that they could cover average school costs for up to one year in public secondary schools in the study area (**Figure S1**). To be eligible to receive the second portion of the cash transfer, participants had to continuously attend school during the second half of the school year leading up to the final exams (between March 1st, 2023, and May 31st, 2023). "Continuous attendance" was defined as missing no more than 2 weeks of schooling in this 3-month window. Excused absences of school included a major health condition, social (family) problem (e.g., death in family), or inability to travel to school due to security situations. School attendance was confirmed by school administrators and cash transfers were provided in person at the Nouna Health Research Center, either alone or in the presence of a parent (if minor). All participants received a one-page confirmation letter with details on the intervention which was signed in-person by the participant and field workers upon delivery of the cash transfer.

Students in the control group did not receive any interventions. Information treatments were only provided to participants in the intervention arm.

Sample Size

We chose a sample size of about 300 out-of-school adolescents pragmatically to balance resource and logistical constraints with the objective of providing a first approximate estimate of the increases in secondary school attendance that can be achieved with the interventions. Given

the relatively large size of the implicit transfer - USD 100 corresponds to roughly 2 months of personal income in this setting for poorer households - we were expecting relatively large effect sizes. Our study was powered to detect a 20-percentage point increase in secondary school re-enrollment in the three treatment arms relative to a 10% anticipated re-enrollment rate among dropouts recruited for the control group with probability 0.8, assuming alpha = 0.05 and a two-sided test. For power 0.9, a minimum detectable effect size (MDE) of 0.22 percentage points was required.

Target Population

Our target population were students who had recently dropped out of secondary school (also known as "baseline dropouts" as opposed to e.g., "baseline school-going children"). We included participants who last attended grades "6e" to "3e" – also known as "post-primaire" – and who are typically ages 12 years or higher (junior secondary school). We also included participants who last attended grades "2e" to "terminal" – also known as "secondaire" – and who are typically ages 16 years or higher (senior secondary school). Specifically, we considered out-of-school adolescents who were living in the Nouna Health and Demographic Surveillance System study area and were enrolled in a secondary school during the past 12 months. Individuals who had never attended formal schooling, were currently in school, or had already completed all their formal secondary schooling were not eligible to participate. We sought to leverage the strong ties of the Nouna Health Research Center with local schools and community leaders to identify participants using student rosters obtained from each school in the study area. Eligible participants (recent dropouts) were approached at home by field workers and written informed consent was obtained from at least one parent or guardian if the respondent was minor (defined as age < 18 years). **Table S1** in the Appendix provides a school-level recruitment tracking sheet,

including the total number of students, number of students who recently left school, and number of students contacted by field workers, separately for each school. **Figure S3** shows a study participant flow diagram. Field workers contacted a total of 403 students, and, out of those, 327 (81.1%) were reached and consented.

Study Timeline

In **Figure 2**, we show a timeline for the field experiment during the 2022-23 school year in Burkina Faso. The Burkinabé school year starts in October and ends in May with a final exam period in June or July depending on the school level and grade (Government of Burkina Faso, 2022). Data collection started in October at the beginning of the school year and outside of the rainy season and main agricultural season [6]. Training of field workers lasted 3 days, between October 25th and October 27th, 2022. Start of data collection for the baseline survey was October 31st, 2022 and the last day of data collection for the baseline survey was December 29th, 2022. In terms of interventions, tutoring support took place between January 1, 2023, and May 31st, 2023. The last day of data collection for the endline survey took place on December 22nd, 2023. Given the considerable security concerns and phone network interruptions, field workers made several attempts to disburse the second cash transfer to eligible participants (i.e., participants in the intervention arm with economic support who had stayed in school) for up to one year after completion of the endline survey. The last day of the cash intervention was therefore December 20th, 2024 when field workers had reached and disbursed the second portion of the cash to most eligible participants in-person. Our primary outcome variable focused on the three-month period leading up to the final exam period highlighted in light green in **Figure 2** below.

[6] The rainy season and main agricultural season in Burkina Faso typically run from June to September.

Figure 2. Timeline of pilot field experiment

<table>
<tr><th rowspan="2">Activities</th><th colspan="10">School year 2022-2023</th><th></th></tr>
<tr><th>Oct ’22</th><th>Nov ’22</th><th>Dec ’22</th><th>Jan ’23</th><th>Feb ’23</th><th>Mar ’23</th><th>Apr ’23</th><th>May ‘23</th><th>Jun ’23</th><th>Jul ’23</th><th>Aug ‘23</th><th>Sep ‘23</th></tr>
<tr><td>Recruitment and baseline survey</td><td>x</td><td>x</td><td>x</td><td></td><td></td><td></td><td></td><td></td><td rowspan="3">Final exam period</td><td></td><td></td><td></td></tr>
<tr><td>Randomization and roll-out interventions</td><td>x</td><td>x</td><td>x</td><td>x</td><td>x</td><td>x</td><td>x</td><td>x</td><td></td><td></td><td></td></tr>
<tr><td>Follow-up survey</td><td></td><td></td><td></td><td></td><td></td><td></td><td></td><td></td><td></td><td>x</td><td>x</td></tr>
<tr><td>Target Enrollment window</td><td></td><td></td><td></td><td></td><td></td><td>x</td><td>x</td><td>x</td><td></td><td></td><td></td><td></td></tr>
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Survey Implementation

Surveys were conducted by trained staff of the Nouna Health Research Center with experience in quantitative data collection either in French or in a mix of the country’s regional language (Djoula) and French. Participants in the study were contacted on average 3.0 times by phone and were visited on average 2.3 times at home by the time the endline survey was completed. Our final sample at baseline included a total of 324 respondents with complete data on demographic characteristics. Mean age at baseline was 18.4 and 19.5 among young men and women, respectively, and average last grade (school year) attended was 9.0 and 9.4 among young men **Figure S4** shows the distribution in age and last grade. Our endline sample included a total of 284 participants (response rate: 87.6%). The response rate was highest in the control arm (90.5%), followed by the cash (89.9%), tutoring (88.0%), and information arms (82.6%).

Randomization, Trial Registration and Ethical Clearance

Randomization was done at the individual (child) level using a simple random number draw generated by the tablet used for the baseline survey. At the end of the baseline survey, each study participant was immediately assigned to one of the three interventions or the control group using a simple random number draw generated by the data capture software.

The experiment and analytical plan were pre-registered on October 22nd, 2022, at socialscienceregistry.org (RCT ID: AEARCTR-0010289). Ethical clearance for the experiment was obtained in 2022 from the National Ethics Committee for Health Research, Ministry of Health in Burkina Faso (2022-09-203) and the Heidelberg University Hospital Ethics Committee in Germany (S-362/2022). Ethical clearance for formative research was obtained in 2018 in Burkina Faso (2018-03-/CIE-CRSN) and Germany (S-193/2018).

Baseline characteristics and Balance

Table 1 shows selected baseline characteristics of participants, distinguishing between control group (top panel) and each of the treatment groups. We show results for gender, age, household characteristics (access to electricity), total years of schooling completed, and measured height (cm) and weight (kg) of participants. We report the mean of each variable, its standard deviation (SD), the *p*-value on the difference between treated (T) and control (C) and the number of observations. The *p*-value on the test of equality is based on an ordinary least squares (OLS) regression of the outcome of interest regressed on dummies for each treatment group with control as reference.

Table 1. Baseline characteristics of participants by treatment status (*N*=324)

Variable	(1) Mean	(2) SD	(3) T = C p-value	(4) N
Control arm				
Female (=1)	0.39	0.49	-	84
Age (years)	18.3	3.19	-	84
Has electricity (=1)	0.46	0.50	-	83
Years of schooling	10.2	2.42	-	80
Measured height (cm)	163.5	9.26	-	82
Measured weight (kg)	58.4	16.4	-	83
Treatment arm: information				
Female (=1)	0.52	0.50	0.110	69
Age (years)	19.1	3.92	0.240	69
Has electricity (=1)	0.43	0.50	0.777	69
Years of schooling	10.9	2.98	0.119	68
Measured height (cm)	161.8	10.8	0.319	67
Measured weight (kg)	57.2	10.6	0.586	68
Treatment arm: tutoring				
Female (=1)	0.49	0.50	0.199	92
Age (years)	18.8	3.96	0.356	92
Has electricity (=1)	0.48	0.50	0.735	91
Years of schooling	10.7	3.01	0.716	89
Measured height (cm)	162.9	8.63	0.898	90
Measured weight (kg)	57.3	16.4	0.593	91
Treatment arm: cash				
Female (=1)	0.37	0.49	0.740	79
Age (years)	19.3	3.70	0.095	79
Has electricity (=1)	0.42	0.50	0.610	79
Years of schooling	10.8	2.81	0.167	78
Measured height (cm)	165.0	11.1	0.324	78
Measured weight (kg)	57.7	11.5	0.757	77

Notes: Table reports the mean of each variable, its standard deviation (SD), the p-value on the difference between treated (T) and control (C) and the number of observations. The p-value on the test of equality is based on an ordinary least squares (OLS) regression of the outcome of interest regressed on dummies for each treatment group with control as reference. *N*=324.

Overall, characteristics of the control group and treatment groups seem balanced at conventional benchmarks of statistical significance (*p*-value=0.05) in line with the randomized design of the evaluation. In the information intervention, there are slightly more female participants, and in the cash intervention participants are slightly older compared to the control group (*p*-values of 0.110 and 0.095, respectively). We address these imbalances by adding controls for gender and age in robustness checks (**Section 5**).

Outcome Measures

Our primary outcome of interest was student's re-enrollment in secondary schooling. We considered a student re-enrolled if she missed less than two weeks of school continuously in the second half of the academic year, defined as the period between March 1st and May 31st, 2023 (unexcused absences). The cutoff of two weeks was determined jointly by local study team members to be most appropriate for the study context. Specifically, in the education context in the study area, if an adolescent misses school for one week (five days consecutively), school staff must contact the student's parent or guardian immediately the following week to understand the reasons for being out-of-school (with two weeks missed maximum in total). Excused absences included a major health problem, social (family) problem (e.g., death in family), or inability to travel to school due to security concerns in the study area. Our primary outcome was obtained from two different sources. First, we asked households during the endline survey. Second, we used administrative records. To do so, we contacted school administrators of the last secondary school attended by the participant following completion of the endline survey. In the pooled sample, 45 out of 285 participants (15.7%) did not miss more than two weeks in the endline survey as reported by households. These results were largely consistent with reports obtained from school administrators (14.7% did not miss more than two weeks). We also

explored several secondary outcomes. Specifically, we assessed school re-enrollment during the school year 2022-23 (binary), school attendance (binary) and the total number of school days missed during the school year 2022-23. We also assessed the academic performance of participants as reported by school administrators, including whether the participant succeeded in that academic year (defined as a binary indicator where 1=passed and zero otherwise). In Burkina Faso, adolescents are allowed to not enroll in school but just register for final school exams as an "independent candidate" (who is not regularly attending a school). As a result, the proportion that is formally enrolled in that school year may, in principle, be similar or lower than the proportion that ultimately succeeds in that school year. We also assessed whether the interventions led to changes in time-use and perceptions of secondary schooling.

Econometric Framework

To estimate causal impacts of our interventions, we estimate the following equation:

$$Y_i = \beta_i + \beta_1 \, 1[T]_i + \gamma X_i + u_i \qquad (1)$$

where Y_i is the outcome of student *i* recorded in the endline data. T_i are separate indicators that take the value one if student i is assigned to treatment group 1, 2, or 3 with control as reference group. Since not all respondents in the study took up the offer of the interventions, β_1 reflects the intention-to-treat (ITT) effect of receiving the intervention. In robustness checks, we also add a vector of control variables (X_i), including age (years), gender, and interviewer fixed effects which is not expected to substantially affect the point estimates but may reduce the variance. We also show results when clustering standard errors at the school level (n=19). We also assessed heterogeneity by gender, school performance (ever repeated a grade vs. never repeated a grade), and area of residence (urban vs. rural). Because many adolescents in our sample are out-of-school during the 2022-23 school year, we do not disaggregate our analyses by current school

performance (e.g., test scores in 2022-23) or school-level characteristics that are conditional on current school attendance (e.g., current type of school or class size). Following our pre-registered analysis plan, we used a significance level of $p<0.05$ for all analyses.

IV. Main Results

We first provide a descriptive analysis of the uptake of the interventions and how these interventions were perceived by study participants. We then show our main results for the impact of the interventions on our pre-defined primary outcome and secondary outcomes. We also show impacts on time-use during the school year, perceptions about schooling and plans, and heterogeneity by gender, area of residence, and school performance. Lastly, we provide cost-effectiveness estimates for each of the interventions.

Intervention Compliance

Compliance was defined as the proportion of participants who took up the offer at any point during the intervention period among those who were randomized into one of the treatment groups at baseline. The numerator included participants who accepted *any* study resources (i.e., regardless of the number to tutoring sessions scheduled or amount of financial transfers received during the intervention period). The denominator across the three interventions included a total of 240 participants who were eligible to receive any intervention **(Table 1**). Out of those 240 participants, at least 150 participants accepted an offer so that overall intervention compliance was 62.5%. To assess compliance with treatments, we used tracking sheets (study records) during implementation supplemented with self-reported data from the endline survey. In **Section 5** on costs and cost-effectiveness, we further differentiate between intervention compliance (uptake of *any* resources) and amount of resources (USD) taken up by compliers.

For the information intervention, 50/69 (72%) reported having viewed the video. Among those 50 participants, 24 reported having watched the video once and 26 reported having watched the video twice. Participants reporting having seen the video liked the video with information on the benefits of secondary schooling "very much" (n=26), "a little" (n=23), or "not" (n=1). Over 90% reported that they would recommend or share the video with other people and that it "changed their way of thinking about school". To determine how information about the benefits of schooling may spread through participants' social networks, we also asked about common communication platforms used in the study area. Participants reported communicating mostly with friends in-person at home (98%), in-person in the community (71%), in-person at school (66%), SMS message (46%), WhatsApp (21%), and Facebook (19%). At baseline, 65% reported having a phone and 22% reported having a smartphone with internet.

For the tutoring intervention, 34/92 (37%) participants took up our tutoring offer based on tracking sheets maintained during the implementation of the tutoring sessions. The rest of the participants in the tutoring offer did not participate in the tutoring sessions because they could not be reached on time by the study team or were unable to return safely to the village of Nouna due to security reasons. No participants refused to participate because of a lack of interest in the tutoring sessions, suggesting that those who "complied" were not necessarily more motivated that those not receiving the intervention. Among those participants who reported receiving tutoring sessions during the endline survey[7], participants reported completing on average 4.1 tutoring sessions (ranging between 1 and 11 sessions) and thus used on average only 41% of the tutoring resources that were made available. Nearly all participants who took up the offer of

[7] At least 25 participants reported receiving tutoring sessions during the endline survey.

tutoring sessions interviewed at endline reported that the level of academic difficulty of the tutoring sessions was "appropriate" for their competency level (n=21/25).

For the cash intervention, 66/79 (84%) participants reported receiving *any* cash during the past 6 months in the context of the experiment at endline. Among those 66 participants, the project's cash offer was received by either the adolescent (n=21), legal guardian (n=20), mother (n=9), father (n=8), both parents together (n=4), or another person (n=4). Participants reported using the cash from the project to pay for food (n=54), school supplies –such as a calculator (n=33) or light/batteries to study at night (n=11) –, medication for their health (n=20), and helping a parent (n=12).

Impacts on Attendance and Performance

Figure 3 shows the unadjusted relationship between treatment status and our primary and secondary outcomes for school attendance and school performance at endline. We show results for our primary pre-specified outcome, i.e. for whether the child did not miss more than two weeks unexcused between March 1st and May 31st, 2023 in panel (a). We also show results for whether the child enrolled (registered) during the school year 2022-23 (panel (b)), attended during the school year 2022-23 (panel (c)), the total number of school days missed during that school year (panel (d)), whether the child succeeded the academic year 2022-23 or not (panel (e)) and whether the child was allowed to pass or repeat that school year (panel (f)).

Panels (a), (e), and (f) were obtained using academic records and reported directly by school administrators to field workers. Panels (b)-(d) were reported by adolescents and households. Note that in Burkina Faso, individuals can opt to not enroll or attend school but still register for

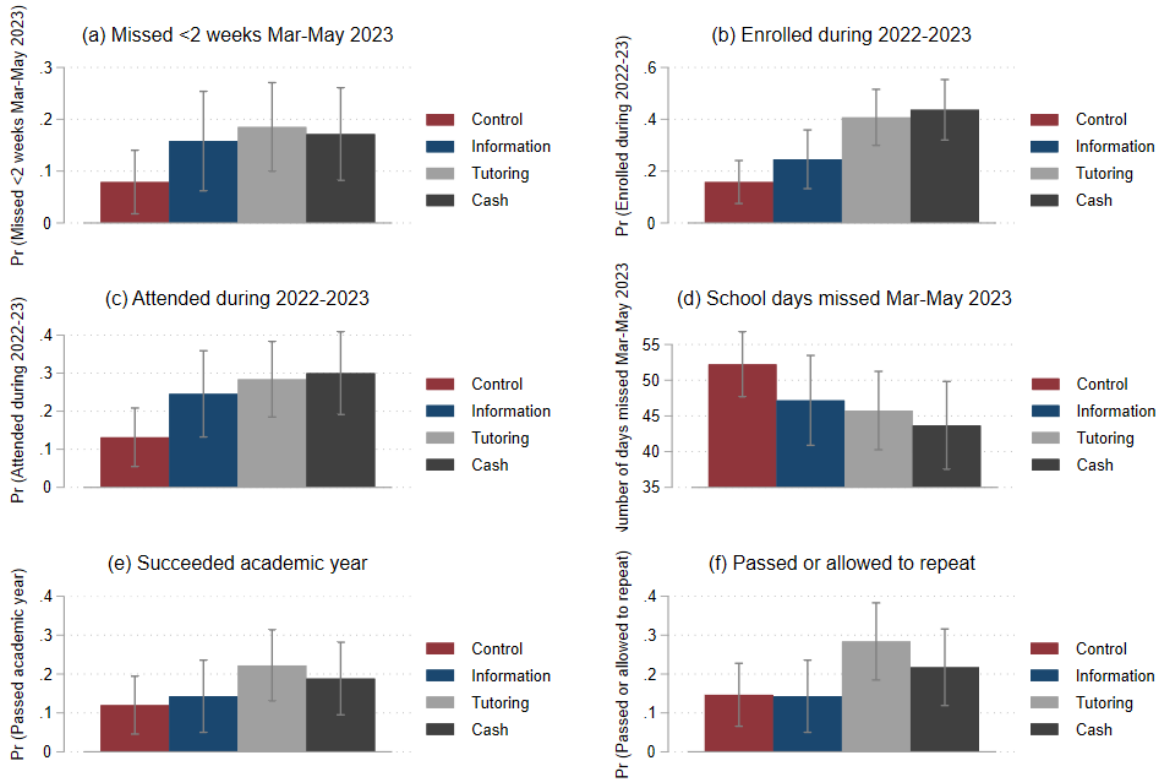
final exams at the end of the year as an independent candidate. [8] The proportion of participants that can pass (or repeat) a given school year can thus be higher than the proportion formally enrolled or attending school. Overall, we find that all three interventions boosted school attendance compared to no intervention (Panels (a)-(d). However, results for school performance at endline were more mixed.

In **Table 2**, we show our main ITT regression results for the impacts on secondary school attendance at endline (columns 1-4). In our baseline specification, our results are consistent with those shown in Panels (a)-(d) in **Figure 3**. We find that all three interventions improved our primary outcome as well as other measures of school attendance. The tutoring intervention increased the probability of not having missed more than two weeks of school by 10.6 percentage points (s.e.= 4.9, $p = 0.043$). The cash intervention increased the probability of not having missed more than two weeks by 9.2 percentage points (s.e. = 4.2, $p = 0.045$). Relative to a baseline of 7.2% in the control group, these findings suggest that the tutoring and cash interventions increased school attendance by over 100%. The information intervention increased the probability of not having missed more than two weeks by 7.9 percentage points (s.e. = 0.196) but these results did not reach our pre-registered benchmark for statistical significance (*p*-value=0.05). Columns 5-6 in **Table 2** show results for school performance at endline. Consistent

[8] Note that in the control group, 16% enrolled, and 13% attending school, but then 15% allowed to continue schooling (pass or repeat). In the treated groups, we have much higher enrollment, but only about 8% higher continuation, which seems much lower. There could be several reasons for this observation. First, quite a few respondents did not enroll/attend school during the 2022-23 school year but did register for the final exams that year as a "candidat libre" (i.e., as independent candidate not formally enrolled but just registering for the final exams). Second, enrollment/attendance in the table (columns 1-3) is reported by households whereas pass/repeat is reported by school administrators (columns 4-6) so there may be differences in measurement - e.g., if households tend to over-report actual school enrollment/attendance. Third, many of the "activated" students may not have the skills to make it. These observations may explain why the proportion pass/repeat may be similar or higher than the proportion enrolled/attending.

with **Figure 3**, we find that results for school performance were more muted.[9] One explanation may be that improving school attendance through financial support and incentives may have limited impact on participants' learning outcomes if the existing curriculum content, material, and/or teachers available are not of sufficient quality (Snilstveit, et al., 2016).

Figure 3. School attendance and performance by treatment status at endline (N=281)



Notes: Outcomes were reported by school administrators in figures (a), (e), and (f) and by households in figures (b) - (d). Succeeded academic year was defined as a binary indicator where 1=passed and 0=allowed to repeat, failed, expelled, or dropped out during the previous or current school year. Passed or allowed to repeat was defined as a binary indicator where 1=passed or allowed to repeat and 0=failed, expelled, or dropped out during the current or previous school year. In Burkina Faso, individuals can opt to not enroll/attend school during the

[9] Extending school attendance during adolescence may have profound consequences even in the absence of impacts on school performance. Extending schooling during adolescence may, for example, keep students occupied and supervised, thereby reducing opportunities to engage in criminal or risky health behaviors (such as early childbearing) (Berthelon & Kruger 2011; Adamecz-Völgyi & Scharle 2020). Conversely, keeping children in school longer may have opportunity costs for households if it keeps adolescents from engaging in economic or domestic activities (such as selling peanuts, herding animals, or taking care of younger siblings). We explore impacts on time-use categories in Section 5. We also collected data on health outcomes (such as early childbearing) but are likely underpowered.

2022-23 school year but register for final exams as a "candidat libre" (an independent candidate not formally enrolled in school but registering for final exams). The proportion pass/repeat may therefore be similar or higher than the proportion enrolled/attending school. Complete case analysis using data from endline survey. *N*=281.

*T*able 2. ITT regression results: impacts on secondary school attendance and performance at endline

Outcome:	**Missed < 2 weeks during Mar-May 2023 (=1)**	**Enrolled during 2022-23 school year (=1)**	**Attended during 2022-23 school year (=1)**	**Number of school days missed during Mar-May 2023**	**Succeeded academic year 2022-23 (=1)**	**Passed or allowed to repeat (=1)**
Treatment status						
Control	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Information (=1)	0.079	0.088	0.114**	-5.048	0.023	-0.004
	(0.059)	(0.054)	(0.049)	(3.086)	(0.040)	(0.045)
Tutoring (=1)	0.106**	0.250***	0.152*	-6.471	0.102*	0.137*
	(0.049)	(0.075)	(0.078)	(4.452)	(0.056)	(0.067)
Cash (=1)	0.092**	0.279***	0.168**	-8.548**	0.068	0.071
	(0.042)	(0.061)	(0.061)	(3.444)	(0.051)	(0.054)
Mean DV, control group	0.079	0.158	0.132	52.2	0.120	0.147
Mean DV, treated groups	0.173	0.373	0.279	45.4	0.189	0.223
Observations	284	285	284	285	281	281
R-squared	0.014	0.063	0.025	0.018	0.012	0.022

Notes: Results from unadjusted ordinary least squares (OLS) regression models. Treatment status was defined as a categorical variable with control (standard of care) as the reference group. Succeeded academic year was defined as a binary indicator where 1=passed and 0=allowed to repeat, failed, expelled, or dropped out during the previous or current school year. Passed or allowed to repeat was defined as a binary indicator where 1=passed or allowed to repeat and 0=failed, expelled, or dropped out during the current or previous school year. Outcomes were reported by school administrators in columns (1), (5), and (6) and by households in columns (2) - (4). Complete case analysis using data from endline survey. In robustness checks presented in the Appendix, we show results when controlling for covariates. Standard errors clustered at school level (n=19) in parentheses. *** p<0.01, ** p<0.05, * p<0.01

Treatment effects on the treated

As with most experiments, compliance was not perfect and ranged from 37% for the tutoring intervention to 84% for the cash intervention. Among participants who reported that they had received tutoring sessions offered by the experiment, 10/25 (40%) advanced to the next grade the following school year and 15/25 (60%) were allowed to pass or repeat. In **Table 3** (columns 1 and 2), we show OLS regression results among compliers by excluding participants who were eligible at baseline but reported during the endline survey that they had not received an intervention. We also show local average treatment effects (LATE) using two-stage least squares (2SLS) regression models where treatment assignment at baseline was used as an instrumental variable to predict exposure to each intervention at endline (columns 3 and 4, **Table 3**). In 2SLS models, tutoring increased regular school attendance by 31.9 percentage points (s.e. = 0.121), compared to a baseline of 7.9%, and the probability of being allowed to pass or repeat by 41.2 percentage points (s.e. = 0.173), off a baseline of 14.7%.[10] The cash intervention increased regular school attendance by 10.4 percentage points (s.e. = 0.047). Results for the impacts of cash and information on school performance were smaller and non-significant.

Robustness checks

Our main results were generally consistent across several robustness checks. In **Table S2**, we present robustness checks for our ITT regression results when adding additional control variables, including age (years), gender, and interviewer fixed effects.

[10] Contextual factors, such as the security situation in the study area, likely moderate the impact of offering tutoring on school performance as opposed to mostly e.g., self-selection by more "motivated" students.

Table 3. Complier average causal and local average treatment effects

Model	**(1)**	**(2)**	**(3)**	**(4)**
	OLS: compliers only		**2SLS results (LATE)**	
Dependent variable (DV)	**Missed < 2 weeks during Mar-May 2023 (=1)**	**Passed or allowed to repeat (=1)**	**Missed < 2 weeks during Mar-May 2023 (=1)**	**Passed or allowed to repeat (=1)**
Treatment status				
Control	Ref.	Ref.	Ref.	Ref.
Information (=1)	0.077*	-0.024	0.090	-0.004
	(0.042)	(0.059)	(0.068)	(0.050)
Tutoring (=1)	0.217*	0.372***	0.319***	0.412**
	(0.106)	(0.114)	(0.121)	(0.173)
Cash (=1)	0.041	0.060	0.104**	0.077
	(0.066)	(0.055)	(0.047)	(0.058)
Mean DV, control group	0.079	0.147	0.079	0.147
Mean DV, treated groups	0.170	0.237	0.174	0.223
Observations	217	214	281	281

Notes: Columns 1 and 2 show results from unadjusted ordinary least squares (OLS) regression models where the sample excludes participants who were eligible to receive an intervention at baseline but reported during the endline survey that they had not received an intervention. Columns 3 and 4 show local average treatment effects (LATE) using two-stage-least squares (2SLS) models where the experiment's randomization is an instrumental variable for the intervention (treatment status). Treatment status was defined as a categorical variable with control (standard of care) as the reference group. Passed or allowed to repeat was defined as a binary indicator where 1=passed or allowed to repeat and 0=failed, expelled, or dropped out during the current or previous school year. Both outcomes were reported by school administrators. Complete case analysis using data from the endline suvey. Standard errors clustered at school level (n=19) in parentheses. *** p<0.01, ** p<0.05, * p<0.01

V. Supplementary Analyses

Impacts on Time-Use

To explore the impact of the interventions on activities among adolescents during the school year, we collected detailed time-use data. Time-use was defined as average time spent per week between March 1st, 2023, and May 31st, 2023, during a typical school week. Economic activities included any income-generating activities (e.g., selling goods) and/or non-paid economic activities. School activities included studying for courses or exams and participating in student societies. Domestic activities included washing clothes, cleaning or cooking at home as well as taking care of other older or younger household members at home. We also show results for other common time-use categories among study participants, including socializing with friends, collecting water or firewood for home, as well as herding animals. All time-use outcomes, including school activities, were reported by adolescents. In **Table 4**, we show ITT regression results for the impact on time-use categories. The cash intervention reduced time spent on socializing with friends (-2.2 hours per week) and herding animals (-2.3 hours per week). Consistent with our main findings for school attendance, reported in **Section 4**, the interventions also led to large qualitative increases in time spent on school activities (such as studying for final exams) but estimates are more noisy here and not generally statistically significant.

Table 4. ITT regression results: impacts on time-use categories during school year

Intention-to-treat model	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Dependent variable (DV)	**Economic activities (hours)**	**School activities (hours)**	**Domestic activities (hours)**	**Socializing with friends (hours)**	**Collecting water or firewood (hours)**	**Herding animals (hours)**
Treatment status						
Control	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Information (=1)	-4.214	2.156	-0.823	-0.520	-1.394*	-1.364
	(3.384)	(2.629)	(1.079)	(0.958)	(0.724)	(0.967)
Tutoring (=1)	-0.656	1.953	1.639	-0.457	-0.728	-1.338
	(4.208)	(2.864)	(2.153)	(0.914)	(0.737)	(1.102)
Cash (=1)	-3.258	4.568	-2.107	-2.157***	0.628	-2.278**
	(2.911)	(3.018)	(1.456)	(0.704)	(0.897)	(1.037)
Controls						
Age (years)	✓	✓	✓	✓	✓	✓
Gender	✓	✓	✓	✓	✓	✓
Interviewer fixed effects	✓	✓	✓	✓	✓	✓
Mean DV, control group	28.5	15.8	10.3	5.5	5.1	3.6
Mean DV, treated groups	29.8	19.0	12.2	4.3	4.7	1.9
Observations	280	283	283	282	282	283
R-squared	0.007	0.011	0.020	0.029	0.015	0.021

Notes: Results from adjusted ordinary least squares (OLS) regression models. Treatment status was defined as a categorical variable with control (standard of care) as the reference group. Time-use was defined as average time spent per week between March 1, 2023, and May 31, 2023, during a typical week. Economic activities in column (1) included income-generating activities (e.g., selling goods) and/or non-paid economic activities. School activities in column (2) included studying for courses or exams and participating in student societies. Domestic activities in column (3) included washing clothes, cleaning or cooking at home as well as taking care of other older or younger household members at home. All time-use outcomes, including school activities, were reported by adolescents. Complete case analysis using data from endline survey. Standard errors were clustered at school level (n=19) in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Impacts on Perceptions and Plans

In **Table 5**, we show impacts on perceptions about schooling and plans. The interventions generally led to more positive impressions and experiences with secondary school attendance and activities (columns 1 and 2). Respondents in the intervention groups more commonly enjoyed "studying for courses" and reported that they found the course material less "difficult or too much work" compared to respondents in the control group. Most participants believed that secondary schooling had economic benefits (>90%) but only about 60% believed that secondary school had health benefits. In our experiment, providing information on the health benefits of schooling had limited impact on changing these beliefs about schooling. One explanation is that the intervention mostly served as a nudge/commitment device which encouraged participants to attend school but without affecting fundamental beliefs about the health benefits of schooling. Another potential explanation is that the uptake of the information sheet (which accompanied the video component of the intervention) on the benefits of school was relatively low among participants. [11] In terms of prospects, all three interventions considerably increased the probability of planning school re-entry the next school year (2023-24). Effect sizes ranged from 15.8 percentage points (s.e. = 0.061) in the information arm to 28.4 percentage points (s.e. = 0.059) in the cash intervention, off a baseline of 14.5% in the control group (column 6).

[11] In the endline survey, only 37/69 (53%) of participants eligible to receive the information intervention reported having received an information sheet on the benefits of schooling. Among all participants who reported at endline that they had received the information sheet, 72% believed that secondary schooling had health benefits. Among all participants who reported at endline that did *not* receive the information sheet, only 59% believed that secondary schooling had health benefits.

Table 5. ITT regression results: impacts on perceptions and plans

Outcome:	**Enjoys studying for courses (score out 100)**	**School material to difficult or to much work (score out 100)**	**Believes secondary schooling has economic benefits (=1)**	**Believes secondary schooling has health benefits (=1)**	**In-school or intends to go back to school (=1)**	**Plans to continue next school year 2023-24 (=1)**
Treatment status						
Control	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Information (=1)	7.756**	2.595	-0.044	0.010	0.125*	0.158**
	(3.417)	(4.580)	(0.028)	(0.074)	(0.069)	(0.061)
Tutoring (=1)	5.979	-4.209*	-0.062	0.010	0.190**	0.179***
	(4.434)	(2.328)	(0.037)	(0.074)	(0.074)	(0.062)
Cash (=1)	8.237*	-7.526***	-0.001	0.090	0.302***	0.284***
	(4.574)	(2.539)	(0.036)	(0.086)	(0.055)	(0.059)
Controls						
Age (years)	✓	✓	✓	✓	✓	✓
Gender	✓	✓	✓	✓	✓	✓
Interviewer fixed effects	✓	✓	✓	✓	✓	✓
Mean DV, treated groups	60.6	52.2	0.937	0.628	0.630	0.377
Mean DV, control group	51.8	55.3	0.974	0.579	0.395	0.145
Observations	283	283	283	283	284	283
R-squared	0.158	0.133	0.016	0.185	0.183	0.153

Notes: Results from adjusted ordinary least squares (OLS) regression models. All models controlled for age (continuously in years), indicators for gender, and indicators for interviewer ID (n=3). Treatment status was defined as a binary indicator for any intervention with control (standard of care) as zero. Complete case analysis using data from endline survey. Standard errors clustered at school level (n=19) in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Subgroup Analyses

The interventions may be most beneficial to those at the bottom of the distribution in school performance if, for example, the marginal benefit of additional resources is larger among those who perform worse at baseline. Conversely, however, better performing students may be in a better position to take advantage of additional resources through dynamic complementarities (Cunha and Heckman 2007). A student skilled in French, for example, may acquire new knowledge and skills in other subjects faster if these are taught in French (the primary language of instruction). We therefore assessed heterogeneity in the impact of the interventions by gender, whether the participant ever repeated a grade or not, and area of residence. To increase our statistical power with multiple subgroups, we first compared outcomes between adolescents who received any intervention (i.e., either information, tutoring, or cash) compared to control. In **Tables 6 - 8**, we show results stratified by gender. Without interventions, young men performed considerably worse on all outcomes for school attendance and performance compared to young women (**Table 6**). In the control group, the probability of regular school attendance was 4.1% among young men compared to 14.8% among young women. Having received any intervention, however, increased regular school attendance by 11.2 percentage points (s.e. = 0.037) among young men whereas effects among young women were smaller and non-significant (column 1, **Table 6**). When analyzing results separately for each intervention, we find that tutoring had large and significant effects among young men. Tutoring increased regular school attendance by 14.9 percentage points (s.e. = 0.057), off the baseline of 4.1%, and academic progression by 14.1 percentage points (s.e. =0.062), off a baseline of 8.2% (columns 1 and 5, **Table 8**). While re-enrollment effects were slightly larger among women - this re-enrollment does not translate into higher shares passing the grade. Our results suggest that tutoring may help mitigate gender gaps

in education in this setting. In **Tables S3-S4**, we show results when disaggregating our sample by school performance (ever repeated a grade or not) and area of residence.

Table 6. ITT regression results: impacts of receiving any intervention by gender

Dependent variable (DV)	Missed < 2 weeks during Mar-May 2023 (=1)	Enrolled during 2022-23 school year (=1)	Attended during 2022-23 school year (=1)	Number of school days missed during Mar-May 2023	Succeeded academic year 2022-23 (=1)	Passed or allowed to repeat (=1)
			Panel A: Female subsample			
Treatment status						
Any intervention (=1)	0.042	0.268**	0.196*	-9.349	0.077	0.152
	(0.077)	(0.096)	(0.096)	(5.716)	(0.094)	(0.104)
Mean DV, control group	0.148	0.185	0.185	40.9	0.192	0.192
Mean DV, treated groups	0.191	0.467	0.367	49.0	0.250	0.318
Observations	116	117	117	117	114	114
R-squared	0.075	0.127	0.104	0.113	0.149	0.131
			Panel B: Male subsample			
Any intervention (=1)	0.112***	0.151**	0.103	-4.879	0.064	0.033
	(0.037)	(0.059)	(0.064)	(3.682)	(0.051)	(0.055)
Mean DV, control group	0.041	0.305	0.102	48.8	0.082	0.122
Mean DV, treated groups	0.161	0.143	0.212	54.0	0.144	0.153
Observations	167	167	167	167	167	167
R-squared	0.128	0.151	0.094	0.079	0.048	0.047

Notes: Results from adjusted ordinary least squares (OLS) regression models, separately for young women (Panel (a)) and young men (Panel (b)). All models controlled for age (continuously in years) and indicators for interviewer ID (n=3). Treatment status was defined as a binary indicator for any intervention with control (standard of care) as zero. Succeeded academic year was defined as a binary indicator where 1=passed and 0=allowed to repeat, failed, expelled, or dropped out during the previous or current school year. Passed or allowed to repeat was defined as a binary indicator where 1=passed or allowed to repeat and 0=failed, expelled, or dropped out during the current or previous school year. Outcomes were reported by school administrators in columns (1), (5), and (6) and by households in columns (2) - (4). Complete case analysis using data from endline survey. Standard errors clustered at school level (n=19) in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7. ITT regression results: impacts of interventions among young women

Outcome:	Missed < 2 weeks during Mar-May 2023 (=1)	Enrolled during 2022-23 school year (=1)	Attended during 2022-23 school year (=1)	Number of school days missed during Mar-May 2023	Succeeded academic year 2022-23 (=1)	Passed or allowed to repeat (=1)
Treatment status						
Control	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Information (=1)	0.063	0.172*	0.169*	-8.715*	0.068	0.072
	(0.096)	(0.088)	(0.086)	(4.973)	(0.099)	(0.098)
Tutoring (=1)	0.028	0.316**	0.201	-7.171	0.064	0.215
	(0.083)	(0.138)	(0.134)	(7.775)	(0.101)	(0.130)
Cash (=1)	0.036	0.314***	0.220*	-13.185*	0.108	0.155
	(0.099)	(0.102)	(0.122)	(7.231)	(0.125)	(0.120)
Controls						
Age (years)	✓	✓	✓	✓	✓	✓
Interviewer fixed effects	✓	✓	✓	✓	✓	✓
Mean DV, control group	0.148	0.185	0.185	49.0	0.192	0.192
Mean DV, treated groups	0.191	0.467	0.367	40.9	0.250	0.318
Observations	116	117	117	117	114	114
R-squared	0.076	0.140	0.105	0.120	0.150	0.143

Notes: Results from adjusted ordinary least squares (OLS) regression models among young women. All models controlled for age (continuously in years) and indicators for interviewer ID (n=3). Treatment status was defined as a categorical variable with control (standard of care) as the reference group. Succeeded academic year was defined as a binary indicator where 1=passed and 0=allowed to repeat, failed, expelled, or dropped out during the previous or current school year. Passed or allowed to repeat was defined as a binary indicator where 1=passed or allowed to repeat and 0=failed, expelled, or dropped out during the current or previous school year. Outcomes were reported by school administrators in columns (1), (5), and (6) and by households in columns (2) - (4). Complete case analysis using data from endline survey. Standard errors clustered at school level (n=19) in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8. ITT regression results: impacts of interventions among young men

Dependent variable (DV)	Missed < 2 weeks during Mar-May 2023 (=1)	Enrolled during 2022-23 school year (=1)	Attended during 2022-23 school year (=1)	Number of school days missed during Mar-May 2023	Succeeded academic year 2022-23 (=1)	Passed or allowed to repeat (=1)
Treatment status						
Control	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Information (=1)	0.077	-0.012	0.043	-0.502	-0.025	-0.066
	(0.055)	(0.088)	(0.079)	(4.594)	(0.056)	(0.061)
Tutoring (=1)	0.149**	0.198**	0.128	-7.199	0.141**	0.105
	(0.057)	(0.071)	(0.077)	(4.451)	(0.062)	(0.069)
Cash (=1)	0.099*	0.217**	0.120	-5.540	0.046	0.029
	(0.056)	(0.097)	(0.082)	(4.513)	(0.061)	(0.063)
Controls						
Age (years)	✓	✓	✓	✓	✓	✓
Interviewer fixed effects	✓	✓	✓	✓	✓	✓
Mean DV, control group	0.041	0.143	0.102	54.0	0.082	0.122
Mean DV, treated groups	0.161	0.305	0.212	48.8	0.144	0.153
Observations	167	167	167	167	167	167
R-squared	0.134	0.185	0.100	0.089	0.075	0.072

Notes: Results from adjusted ordinary least squares (OLS) regression models among young men. All models controlled for age (continuously in years) and indicators for interviewer ID (n=3). Treatment status was defined as a categorical variable with control (standard of care) as the reference group. Succeeded academic year was defined as a binary indicator where 1=passed and 0=allowed to repeat, failed, expelled, or dropped out during the previous or current school year. Passed or allowed to repeat was defined as a binary indicator where 1=passed or allowed to repeat and 0=failed, expelled, or dropped out during the current or previous school year. Outcomes were reported by school administrators in columns (1), (5), and (6) and by households in columns (2) - (4). Complete case analysis using data from endline survey. Standard errors clustered at school level (n=19) in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Costs and Cost-effectiveness

Although our main point estimates are imprecise, we can provide some preliminary back-of-the envelope estimates of costs and potential cost-effectiveness as a comparison with other known interventions to increase schooling in the region.[12] We provide an overview of intervention costs and cost-effectiveness in **Table 9**. Panel (a) summarizes the estimated costs per school year separately for each intervention. For the information intervention, we conservatively assumed a cost of about 5 USD per student (e.g., to support printing material for an information sheet). We did not include the one-time costs of developing the video to deliver the information intervention under the assumption that this cost (per student) would likely be minimal if the intervention is scaled up. In the tutoring intervention, only 34/92 students took up our offer at an average cost of about USD 41 per student (4.1 out of 10 allowable sessions). The total cost of the tutoring intervention was therefore USD 1515 per 100 students targeted. In the financial transfer intervention, 66 students took up the unconditional cash offer at baseline. At endline, only 12 students were eligible for the conditional portion of the financial transfer (50 USD) - this implies a total transfer of USD 4937 per 100 students. We also asked participants about potential transport costs that were associated with travelling in person to/from the tutoring sessions or picking up financial transfers in Nouna village. However, among participants who reported at endline receiving tutoring sessions or financial transfers, about 90% reported no additional costs. Panel (b) provides the estimates' gains due to the intervention in terms of the number of

[12] The BRIGHT program, for example, was a "girl-friendly" primary school program in Burkina Faso which increased enrollment by 13 percentage points (Kazianga, Levy, Linden, & Sloan, 2013). For each additional student per year, the BRIGHT program costs between $42.9 and $63 (US$ 2007). Nevertheless, these comparisons require caution. Studies assess different types of school programs (such as building new schools entirely vs. more light-touch interventions), different school levels (such as primary and secondary school) and use different measures of school enrollment. Interventions, such as school-based health programs (e.g., focused on de-worming, malaria or micronutrients), may also affect multiple outcomes and the overall effectiveness of these interventions may be understated when calculating a cost effectiveness estimate for school attendance alone.

additional students per year that attended regularly, enrolled, or continued schooling based on impact estimates from our ITT regressions shown in **Table 2**. Panel (c) provides the estimated cost-effectiveness. The cost of enrolling one additional student per year was USD 41, 61 and 177 for information, tutoring, and cash interventions, respectively. The ranking for school continuation was rather different, with estimated average cost per additional student of USD 9058, 111 and 695, respectively.

Table 9. Costs and cost-effectiveness of strategies as implemented

Panel A: Intervention costs

	Intervention compliance	Uptake of resources	Cost per 100 students (USD)
Information	72%	72%	362
Tutoring	37%	15%	1515
Cash	84%	49%	4937

Panel B: Impact (additional number of students)

Outcome	Continous attendance (=1)	Any school enrollment (=1)	School continuation (=1)
Information	8	9	0
Tutoring	11	25	14
Cash	9	28	7

Panel C: Cost-effectiveness

Information	46	41	9058
Tutoring	143	61	111
Cash	549	177	695

Notes: Table presents estimates of costs and potential cost-effectiveness for each of the strategies as implemented. Panel (a) summarizes the estimated costs per 100 students per school year separately for each intervention. To do so, we multiplied 100 by intervention compliance and the cost per student for that intervention (unit cost). Panel (b) provides the estimates' gains due to the intervention in terms of the number of additional students per year that attended regularly, had any school enrollment, or continued school based on impact estimates from our ITT regressions shown in Table 2. Panel (c) provides the estimated cost-effectiveness. To do so, we calculate the cost per unit of benefit achieved for regular school attendance, secondary school enrollment, and continuation at the end of the school year.

VII. Conclusion

Secondary schooling completion rates remain low in many LMICs. The results presented in this paper suggest that students dropping out of secondary school in Burkina Faso can relatively easily be motivated to re-enroll in school. While enrollment responses to relatively cheap information interventions seem comparable to the response to more costly tutoring and cash incentives, the same is not true for academic progression. Our results suggest that passing secondary schooling exams is relatively challenging for students at the margins of dropping out, and that providing cash or information alone is likely not a very effective way to achieve better academic outcomes. Despite a relatively small share of students taking up the tutoring offer, we find tutoring to be most effective in increasing secondary school continuation and grade progression. This suggests that academic constraints are likely more binding than financial or knowledge constraints in this setting.

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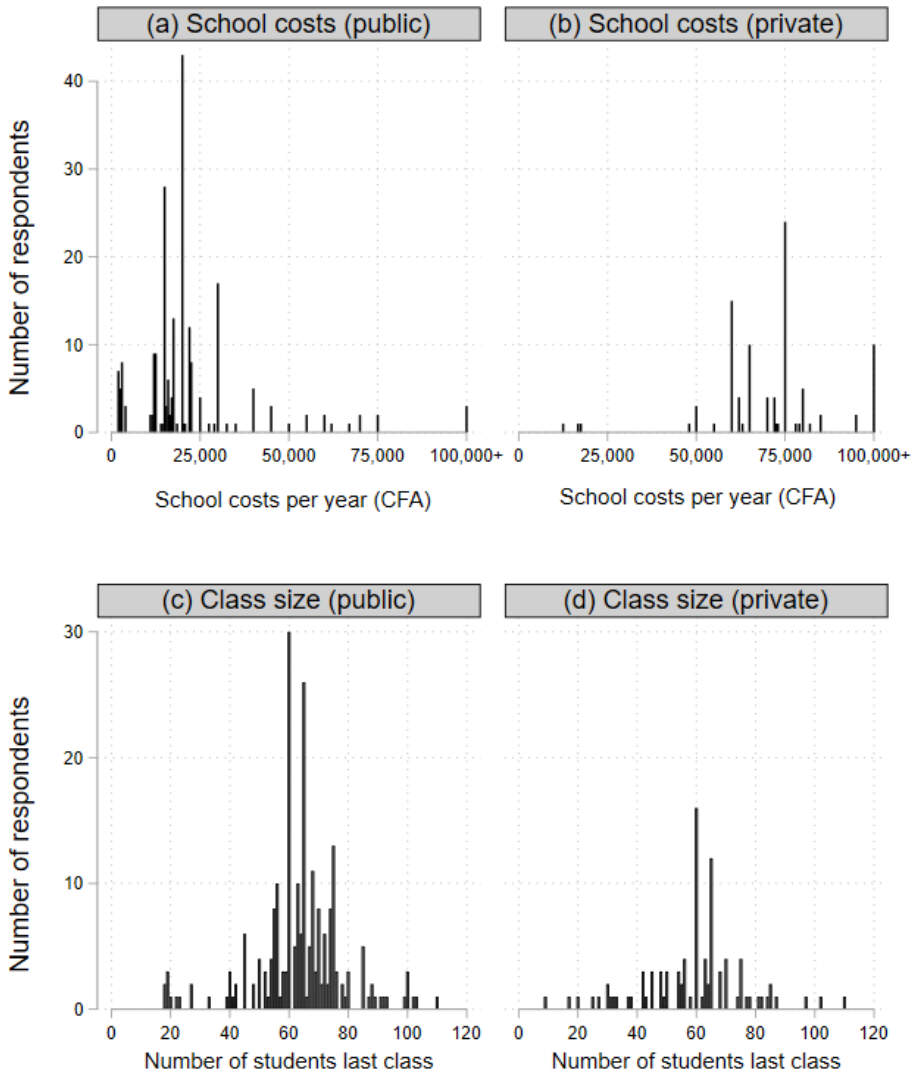
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Appendix

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VIII. Figure S1. School characteristics in the study area



Notes: Figure shows total annual school costs of the last school attended in CFA reported by households, separately for children in public (a) and private schools (b). Figures (c) and (d) show the total number of students in children's last class separately for public and private schools. Households in both public and private schools reported school costs even though basic education is, in principle, free of charge in Burkina Faso. Additionally, despite private school being more expensive, class sizes were generally similar across public and private schools in the study area. Data from baseline survey of field experiment (N=322).

IX. Figure S2. Tutoring sessions offered during field experiment

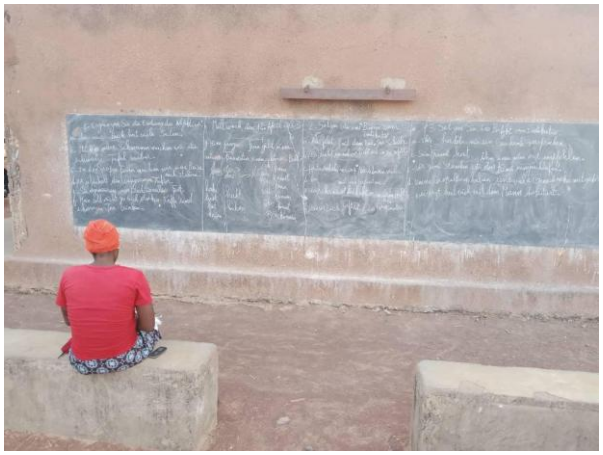
(a) Tutoring session: French



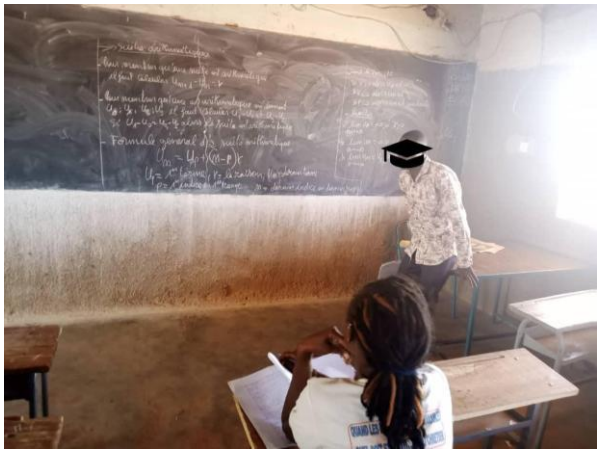
(b) Tutoring session: French



(c) Tutoring session: German

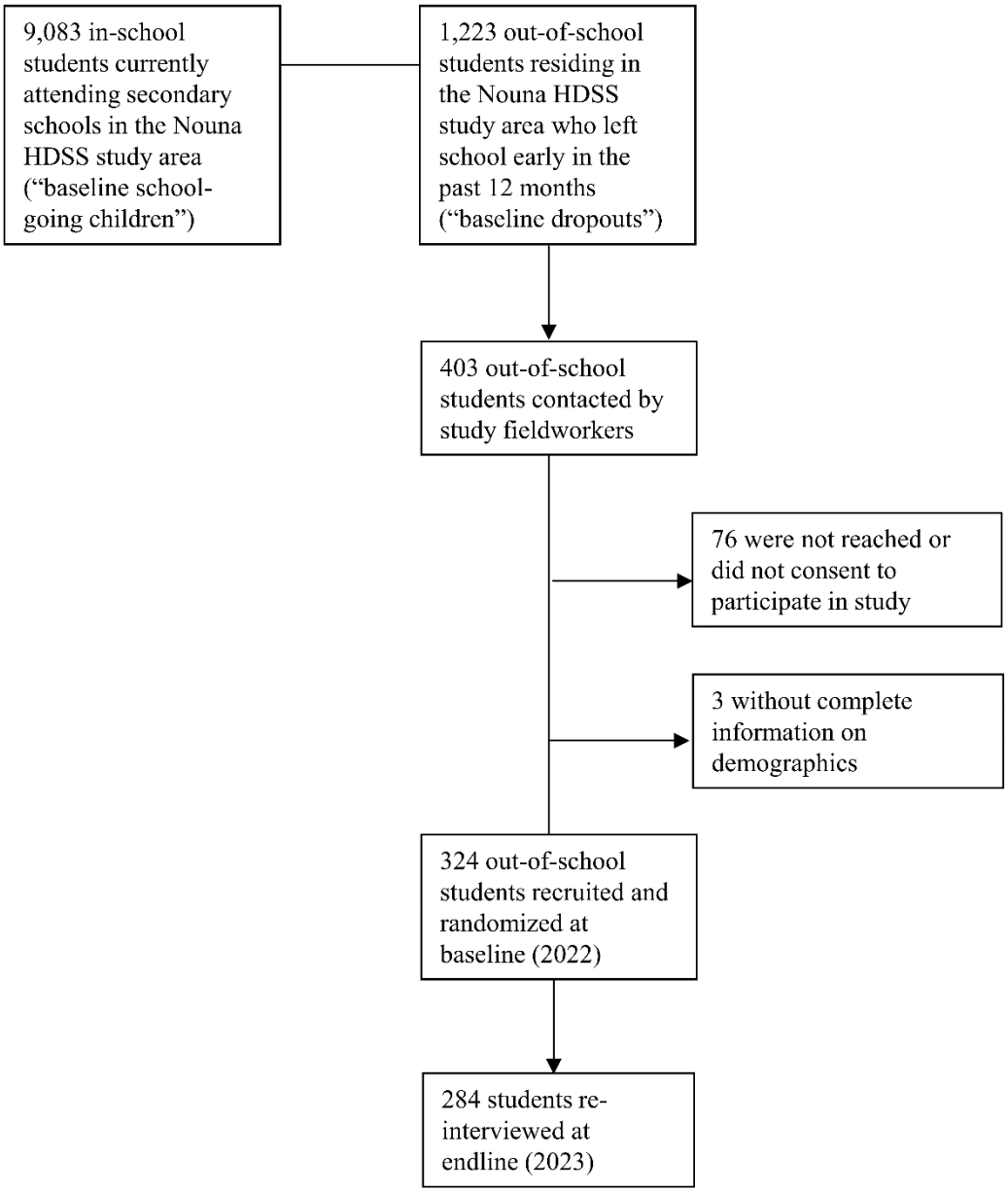


(d) Tutoring session: mathematics



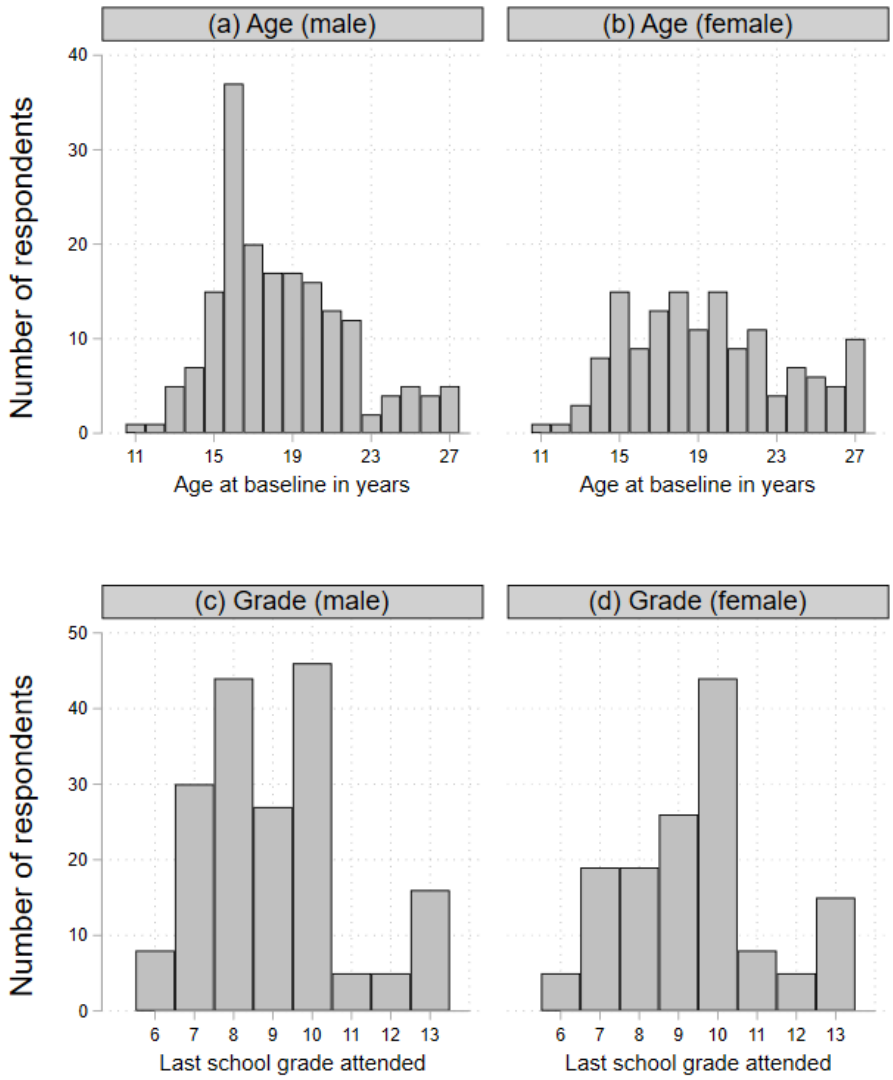
Notes: Figure shows selected tutoring sessions for the study, including in French, German, and mathematics. Instructors included schoolteachers and older peers and received a stipend for each tutoring session.

X. Figure S3. Study participant flow diagram



Notes: Figure shows the flow of participants through the study from initial recruitment until the endline survey of the study was completed (December 2023). Table S1 shows recruitment separately by secondary school in the study area.

XI. Figure S4. Study sample distribution in age and grade



Notes: Figure shows age and last school grade attended reported by adolescents and households, separately for young men (a) and young women (b). Average age was 18.4 and 19.5 among young men and women, respectively, and average last grade (school year) attended was 9.0 and 9.4 among young men and women, respectively. The public education system in Burkina Faso is based on the "6-4-3" system, including six years of primary education, four years of junior secondary school ("Collège"), and three years of senior secondary school ("Lycée"). Data from baseline survey of field experiment (N=322).

XII. Table S1. Recruitment separately by secondary school

Name of commmunity	Name of secondary school located in the study area (Nouna HDSS)	Number of students in school	Number of students who left school early as provided by school	Number of students contacted by field workers
Nouna	CEG de Nouna 1	370	22	8
Nouna	CFP de Nouna	365	20	5
Nouna	Collège Charles Lwanga	556	12	7
Nouna	Collège Privé ADEODAT de Nouna	31	19	10
Nouna	Collège Privé Horoya	960	144	46
Nouna	Collège Privé la Clé du Monde	60	7	6
Nouna	Collège Privé l'Avenir de Nouna	123	19	12
Nouna	Collège Privé Saint M'Baga Tuzindé	414	21	9
Nouna	Lycée Communal de Nouna	846	53	34
Nouna	Lycée Privé Confessionnel Islamique Al-Ilm	31	21	8
Nouna	Lycée Privé Evangélique la Grâce	280	35	17
Nouna	Lycée Privé Franco-Arabe Kadidiatou de Nouna	154	0	0
Nouna	Lycée Privé Koundèle Coulibaly	144	13	7
Nouna	Lycée Privé la Fraternité de Nouna	230	12	5
Nouna	Lycée Privé Olympia de Nouna	213	57	19
Nouna	Lycée Provincial de Nouna	2,209	255	75
Nouna	Lycée Privé Marie Reine de Nouna	278	0	0
Dokuy	CEG de Bonikuy	87	0	0
Dokuy	CEG de Goni	199	99	46
Dokuy	CEG de Kamadéna	43	49	34
Dokuy	LD Dokuy	86	0	0
Doumbala	Lycée Konkui-Koro	135	0	0
Nouna	CEG de Bagala	116	73	15
Nouna	CEG de Dara	317	74	32
Nouna	CEG de Dembo	500	113	5
Nouna	CEG de Konankoira	142	105	3
Nouna	Lycée Municipal de Nouna	194	0	0
All schools	-	9,083	1,223	403

Notes: CEG = Collège d'Enseignement Général; CFP = Centre de Formation Professionnelle.

XIII. Table S2. Robustness checks: impacts on schooling outcomes controlling for covariates

Intention-to-treat model	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Dependent variable (DV)	**Missed < 2 weeks during Mar-May 2023 (=1)**	**Enrolled during 2022-23 school year (=1)**	**Attended during 2022-23 school year (=1)**	**Number of school days missed during Mar-May 2023**	**Succeeded academic year 2022-23 (=1)**	**Passed or allowed to repeat (=1)**
Treatment status						
Control	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Information (=1)	0.084	0.071	0.104*	-4.621	0.022	-0.005
	(0.061)	(0.065)	(0.058)	(3.615)	(0.049)	(0.054)
Tutoring (=1)	0.085*	0.226****	0.135	-5.688	0.095	0.126*
	(0.048)	(0.076)	(0.081)	(4.692)	(0.058)	(0.070)
Cash (=1)	0.089**	0.266****	0.166**	-8.899**	0.078	0.083
	(0.042)	(0.066)	(0.066)	(3.718)	(0.058)	(0.059)
Controls						
Age (years)	✓	✓	✓	✓	✓	✓
Gender	✓	✓	✓	✓	✓	✓
Indicator for interviewer ID (n=3)	✓	✓	✓	✓	✓	✓
Mean DV, control group	0.079	0.158	0.132	52.2	0.120	0.147
Mean DV, treated groups	0.173	0.373	0.279	45.4	0.189	0.223
Observations	283	284	284	284	281	281
R-squared	0.08	0.158	0.090	0.078	0.086	0.103

Notes: Results from ordinary least squares (OLS) regression models. Treatment status was defined as a categorical variable with control (standard of care) as the reference group. Succeeded academic year was defined as a binary indicator where 1=passed and 0=allowed to repeat, failed, expelled, or dropped out during the previous or current school year. Passed or allowed to repeat was defined as a binary indicator where 1=passed or allowed to repeat and 0=failed, expelled, or dropped out during the current or previous school year. Outcomes were reported by school administrators in columns (1), (5), and (6) and by households in columns (2) - (4). Complete case analysis using data from endline survey. Standard errors clustered at school level (n=19) in parentheses. *** p<0.01, ** p<0.05, * p<0.1

XIV. Table S3. ITT regression results: impacts of interventions by school performance

Intention-to-treat model	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable (DV)	Missed < 2 weeks during Mar-May 2023 (=1)	Enrolled during 2022-23 school year (=1)	Attended during 2022-23 school year (=1)	Number of school days missed during Mar-May 2023	Succeeded academic year 2022-23 (=1)	Passed or allowed to repeat (=1)
			Panel A: Never-repeated subsample			
Treatment status						
Any intervention (=1)	0.055	0.145*	0.122	-6.111	0.137*	0.109**
	(0.073)	(0.081)	(0.074)	(4.268)	(0.069)	(0.046)
Mean DV, control group	0.107	0.179	0.143	51.5	0.143	0.000
Mean DV, treated groups	0.162	0.324	0.265	45.4	0.279	0.109
Observations	96	96	96	96	96	89
R-squared	0.005	0.021	0.017	0.013	0.021	0.033
			Panel B: Ever-repeated subsample			
Treatment status						
Any intervention (=1)	0.117**	0.254***	0.161**	-7.295*	0.039	0.061
	(0.053)	(0.060)	(0.067)	(3.833)	(0.057)	(0.057)
Mean DV, control group	0.063	0.146	0.125	52.7	0.106	0.128
Mean DV, treated groups	0.180	0.400	0.286	45.4	0.145	0.188
Observations	187	188	188	188	185	185
R-squared	0.021	0.055	0.027	0.018	0.002	0.005

Notes: Results from unadjusted ordinary least squares (OLS) regression models, separately for participants who never repeated a grade (Panel (a)) and ever repeated a grade (Panel (b)). Treatment status was defined as a binary indicator for any intervention with control (standard of care) as zero. Succeeded academic year was defined as a binary indicator where 1=passed and 0=allowed to repeat, failed, expelled, or dropped out during the previous or current school year. Passed or allowed to repeat was defined as a binary indicator where 1=passed or allowed to repeat and 0=failed, expelled, or dropped out during the current or previous school year. Outcomes were reported by households in column (1) - (3) and by school administrators in column (4) – (6). Complete case analysis using data from endline survey. Standard errors clustered at school level (n=19) in parentheses. *** p<0.01, ** p<0.05, * p<0.1

XV. Table S4. ITT regression results: impacts of interventions by area of residence

Intention-to-treat model	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable (DV)	Missed < 2 weeks during Mar-May 2023 (=1)	Enrolled during 2022-23 school year (=1)	Attended during 2022-23 school year (=1)	Number of school days missed during Mar-May 2023	Succeeded academic year 2022-23 (=1)	Passed or allowed to repeat (=1)
			Panel A: Urban subsample			
Treatment status						
Any intervention (=1)	0.120**	0.230***	0.165**	-8.023*	0.059	0.055
	(0.052)	(0.053)	(0.072)	(4.336)	(0.064)	(0.070)
Mean DV, control group	0.118	0.235	0.196	48.4	0.180	0.220
Mean DV, treated groups	0.238	0.465	0.361	40.4	0.239	0.275
Observations	194	195	195	195	192	192
R-squared	0.017	0.042	0.024	0.017	0.004	0.003
			Panel B: Rural subsample			
Treatment status						
Any intervention (=1)	0.031	0.169*	0.094**	-3.385**	0.078**	0.109**
	(0.022)	(0.085)	(0.040)	(1.407)	(0.032)	(0.046)
Mean DV, control group	0.000	0.000	0.000	60.0	0.000	0.000
Mean DV, treated groups	0.031	0.169	0.094	56.6	0.078	0.109
Observations	90	90	89	90	89	89
R-squared	0.009	0.054	0.028	0.018	0.023	0.033

Notes: Results from unadjusted ordinary least squares (OLS) regression models, separately for urban (Panel (a)) and rural areas (Panel (b)). Treatment status was defined as a binary indicator for any intervention with control (standard of care) as zero. Succeeded academic year was defined as a binary indicator where 1=passed and 0=allowed to repeat, failed, expelled, or dropped out during the previous or current school year. Passed or allowed to repeat was defined as a binary indicator where 1=passed or allowed to repeat and 0=failed, expelled, or dropped out during the current or previous school year. Outcomes were reported by school administrators in columns (1), (5), and (6) and by households in columns (2) - (4). Complete case analysis using data from endline survey. Standard errors clustered at school level (n=19) in parentheses. *** p<0.01, ** p<0.05, * p<0.1