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The Early Bird Gets the Cash

Early Notification and Conditional Cash Transfers for Secondary School

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Abstract

What is the optimal timing for a cash transfer targeting increased secondary school enrollment? We present evidence from a randomized trial in rural Ethiopia showing a CCT increases enrollment and reduces child marriage. The largest effects are observed for youth offered the transfer a year prior to their (potential) matriculation; in this cohort, there is also a significant increase in the probability students pass the primary school leaving exam. By contrast, reduced treatment effects are observed for prior dropouts, and a generalized random forest analysis suggests this primarily reflects different observable characteristics (lower academic performance) among this sub-sample.

Keywords: Conditional cash transfer, secondary school, Ethiopia, Productive Safety Net Program, youth

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1 Introduction

Secondary school enrollment in sub-Saharan Africa remains persistently low (Evans and Acosta, 2021), and these low rates of enrollment reflect at least in part challenges faced by rural adolescents who face long distances to secondary schools and thus high out-of-pocket costs for attendance (as well as the cost of forgone income should they enroll).[1] While a robust literature has explored the effectiveness of conditional cash transfers in increasing secondary school enrollment by alleviating liquidity constraints and compensating for any potential forgone income, there is little evidence around when it is optimal to intervene. On the one hand, earlier notification of the availability of a conditional cash transfer targeting younger cohorts of students could generate anticipatory investment effects (prior to transfer receipt) that amplify the direct effects of the transfer. On the other hand, targeting older cohorts of students (including students who have already dropped out) may more effectively identify students who are on the margin of enrollment versus dropout.

This paper uses a randomized trial conducted with extremely poor rural youth in Ethiopia to assess three interrelated questions, using comprehensive data on school enrollment as well as familial and economic outcomes and non-cognitive and cognitive skills. What are the effects of a conditional cash transfer (payable per semester upon enrollment) on engagement in secondary school as well as marriage, cognitive, and non-cognitive skills, in a context where the underlying enrollment rate is only around 33%?[2] How do these effects vary among multiple cohorts of primary school students and dropouts enrolled in the trial: eighth graders offered a cash transfer around three months prior to their participation in the primary school leaving exam, seventh graders informed of the transfer more than a year prior to potential secondary school matriculation, and dropouts (youth who have already successfully passed the primary school leaving exam, but who did not continue to secondary school)? What baseline characteristics of youth predict larger treatment-induced shifts in enrollment in a generalized random forest analysis?

[1]In Ethiopia, site of this study, there is only one secondary school for every ten primary schools, implying a very large population (overwhelmingly rural) with very constrained access to secondary school (Ministry of Education, 2023).

[2]UNICEF Ethiopia (2023)

The trial sample includes 2,005 youth (middle school students and dropouts) living in extremely poor households in rural Ethiopia that are eligible for the Productive Safety Net Program (PSNP), one of the largest social safety net program in sub-Saharan Africa (Gilligan et al., 2009).[3] The transfer offer was randomized at the subdistrict level within a sample of 116 subdistricts that did not have a secondary school (implying a higher distance, and thus higher cost, associated with secondary school attendance), and provided $300 per year of enrollment for up to two years for students who enrolled in school (paid to their families or caregivers). The objective was to fully fund the costs of accommodation, transportation, and other expenses, particularly for students required to live away from home to attend school.[4] The size of the transfer is similar, in nominal terms, to other recently evaluated secondary school transfers (Giacobino et al., 2024).

At baseline, eligible youth could be enrolled in either seventh or eighth grade (41% and 40% of the realized sample, respectively), or be post-primary dropouts (19% of the sample), and all such youth in PSNP households entered the sample; the first two cohorts of youth enrolled had not yet taken the primary school leaving exam that determines eligibility for secondary school, while the latter group by definition had already passed the exam. All cohorts could access the first tranche of the transfer only following confirmed enrollment in secondary school. We collected follow-up survey data around two years post-baseline, at the conclusion of the transfer eligibility period, and we also verify enrollment estimates using administrative data extracted from school registers. The sample was characterized by substantial barriers to educational attainment: youth were on average nearly two years behind their target age for grade at baseline, and 40% reported they did not know a single individual who had completed secondary school.[5]

[3]The cash transfers were implemented by SPIR II, a consortium supporting PSNP implementation and providing complementary livelihoods and nutrition interventions; as part of its youth programming, SPIR rolled out a targeted cash transfer for secondary school, limited to youth in PSNP households (assumed to be around the poorest 15% of rural Ethiopian households). SPIR II was led by World Vision and funded by the U.S. government.

[4]Nationwide, more than 80% of rural households in Ethiopia live in a community without a secondary school. Despite this greater distance to secondary schools, however, ex post the reported realized costs for enrollment for students in the sample were considerably lower than the transfer size — as described further below.

[5]This level of delayed progression is certainly not unique to this ultrapoor sample: data from the Young Lives longitudinal study suggests delays of up to three years in educational progression

Our findings show a substantial positive effect on enrollment in secondary school and a decline in early marriage, in conjunction with notable effects on a range of variables linked to household socioeconomic status (including increased enrollment of younger siblings). Based on survey data, youth offered a transfer are 21 percentage points more likely to ever enroll in secondary school and 15 percentage points more likely to be enrolled in secondary school at follow-up, and these effects are qualitatively similar when estimated using administrative data.[6] There is also notable heterogeneity comparing across the various cohorts of baseline enrollees (with the caveat that baseline enrollment status was not randomly assigned). Baseline dropouts show the largest effect (in both absolute and proportional terms) in ever-enrolling in secondary school post-baseline (41 percentage points, relative to a control mean of 26%) — perhaps unsurprising given that they have all passed the primary school leaving exam — but many drop out again, and by follow-up, they show a nine percentage point increase in enrollment; still, this corresponds to an enrollment rate that has more than doubled relative to the rate in the control arm (only 6%). Baseline eighth graders show a slightly larger 12 percentage point increase in enrollment at follow-up, corresponding to a 40% increase relative to the control mean of 30%, while baseline seventh graders show a 21 percentage point increase in enrollment at follow-up, corresponding to a 80% increase relative to the control mean of 26%.

The larger treatment effect for seventh graders vis-a-vis eighth graders suggests that there may be substantial benefits to early notification to facilitate planning for longer-term educational investment.[7] While another plausible hypothesis could be that the larger treatment effect reflects the differential composition of the seventh grade cohort (inclusive of more marginal students who would, otherwise, drop out even before eighth grade), we see no evidence that the positive treatment effect is concentrated among more marginal students characterized by a lower predicted probability of passing. Rather, seventh grade students characterized by a lower ex ante passing probability show a treatment-induced increase in primary school

by age 18 are typical (Young Lives Ethiopia, 2018).

[6] In fact, the effects are somewhat larger when estimated using administrative data, a point that we will probe further below.

[7] Conditional on enrollment, there is no evidence of declining academic performance within the treatment arm.

passing and secondary school enrollment, while those characterized by a higher ex ante passing probability show a treatment effect on passing probability that is small and not statistically significant, but still show a large treatment-induced increase in enrollment. This is consistent with the hypothesis that it is primarily early notification and the associated planning benefits (inclusive, but not limited to, planning for the primary school leaving exam etc.) that drives the larger effects for seventh graders (a point we will return to in our systematic analysis of heterogeneity using a generalized random forest.)

The cash transfer also leads to a significant decline in the probability of marriage prior to age 18 (1.9 percentage points) despite a low incidence in the control arm, consistent for both boys and girls.[8] The decline in early marriage is substantially driven by the sample of baseline seventh graders and concentrated among those characterized by a lower predicted probability of passing, suggesting that it is more marginal students (who may not ultimately ever pass or attend secondary school) who are more likely to benefit from the transfer in terms of postponing marriage: i.e., it is primarily the anticipatory dimension of the transfer that drives the treatment effect here.

In examining effects on other dimensions of human capital, we find generally positive but small effects on non-cognitive skills (self-esteem, agency, and aspirations). There are no statistically significant effects on cognitive skills (measured using the Raven's matrix and a simple mathematics module), suggesting that the transfer-induced increase in enrollment has only a minor effect on accumulation of human capital (and consistent with other, at least anecdotal evidence of very low educational quality in Ethiopian secondary schools). There is also no effect on any measure of engagement in economic activities, including labor on household farms as well as non-agricultural self-employment or wage labor. This null effect may appear somewhat surprising, in that youth induced into secondary school might be expected to reduce their labor supply relative to counterparts who do

[8] The mean rate of early marriage in the control arm is 10% for girls and 2% for boys, but the decline is roughly similar in absolute magnitude for both boys and girls. While child marriage rates in Ethiopia have been high in the past, the minimum age of legal marriage was raised from 15 to 18 in 2000; the very low rate of child marriage we observe is consistent with other evidence that this reform was effective (McGavock, 2021), though of course our sample is drawn from only two regions and is not representative.

not enroll or who drop out; one plausible interpretation is that schooling and work are not strongly substitutable at this margin in this rural setting, where the bulk of youth labor occurs within family enterprises and can be combined with school attendance. in addition, the timing of the survey near the close of the school year may limit the ability to detect any differences across arms.[9]

Interestingly, despite the relatively modest effects on some secondary outcomes reported for the youths themselves, we observe substantial positive effects on other household economic outcomes. School enrollment for non-targeted children increases, corresponding to a roughly 14% increase in the number of younger siblings (aged 6–18) from the baseline household enrolled in school. We also observe a significant increase in food security and assets. This is consistent with the fact that the documented transfer-induced increase in total educational expenditure is notably smaller than the transfer itself: total expenses for a semester (around $67) were in fact less than half the semester transfer value ($150), suggesting that households were effectively able to economize on school-related costs, and then devoted substantial funds to meet other household needs.

We then build on the intent-to-treat analysis by using a generalized random forest (GRF) to probe patterns of heterogeneity. We find substantial heterogeneity in the conditional average treatment effect for current secondary school enrollment: youth in clusters characterized by a higher enrollment of seventh graders and higher baseline non-cognitive and cognitive skills are more likely to be characterized by a conditional average treatment effect (CATE) in the top quartile. Conversely, youth in clusters characterized by a higher concentration of dropouts are more likely to have a CATE in the bottom quartile. However, probing the variable importance estimated by the GRF algorithm suggests that while seventh grade status at baseline is highly predictive of treatment effect heterogeneity, dropout status has little predictive power conditional on other covariates (in particular, low baseline academic performance, low socioeconomic status at baseline, and older age). This is consistent with the interpretation that the relatively weak treatment effects for baseline dropouts is fundamentally driven by different observable and unob-

[9]The probability of migration weakly declines, relative to a low base: the only statistically significant effect is a decline in the probability of migration outside the region of origin, a probability that declines by one percentage point relative to a mean of only 2% in the control arm.

servable characteristics, while larger treatment effects for seventh graders reflects the direct mechanism of enhanced scope for anticipatory investment prior to the transition to secondary school.

Finally, we assess cost-effectiveness of this transfer relative to other cash transfers in the literature. Our findings suggest that at a cost per additional year of secondary school induced of $3258 in 2021 PPP USD (as captured in Figure A2), this program is one of the more cost-effective interventions implemented in this literature, and broadly comparable to, for example, the transfers evaluated in Giacobino et al. (2024) and Baird et al. (2011).

This trial makes several contributions to the existing literature: we provide novel evidence around the relative effectiveness of offering a cash transfer linked to secondary school enrollment to three different cohorts (eighth graders immediately prior to eighth grade exams, seventh graders, and post-primary dropouts), highlighting the importance of the anticipatory investment channel. We also provide evidence around experimental effects on an unusually wide set of experimental outcomes including familial and socioeconomic outcomes and both cognitive and non-cognitive skills. While our findings that the relative effectiveness of the CCT is largest for dropouts is consistent with earlier evidence from Baird et al. (2019) for girls in Malawi, we also present more novel evidence that early notification of transfer availability can significantly increase primary school passing rates, and amplifies the overall effect on secondary school enrollment. This is consistent with recent quasi-experimental evidence from Sandholtz (2024) that expansion of secondary school access (in that case via the abolition of school fees in Tanzania) can raise primary school attainment in anticipation of potential secondary school enrollment. Another related trial of conditional cash transfers in Bogota, Colombia found that postponing delivery of part of a conditional cash transfer (until re-enrollment or matriculation in tertiary education) was the only transfer modality that increased secondary enrollment, consistent with the importance of activating greater investments in advance of transfer receipt (Barrera-Osorio et al., 2011).[10]

We also present relatively novel evidence of positive and meaningful intra-

[10] Quasi-experimental evidence from Chile analyzing the long-term effects of a conditional cash transfer targeting school enrollment of indigenous youth similarly suggests a positive effect of a transfer offered on exam performance (Lucas et al., 2025).

household spillovers of an educational CCT, where younger siblings also show a significantly increased probability of enrollment. Previous evidence in Giacobino et al. (2024) found positive spillovers within villages but not within households, and other recent papers have generally not estimated intrahousehold spillovers, with two exceptions: Barrera-Osorio et al. (2011) find negative within-household spillovers in Colombia, and Blimpo et al. (2019) find positive intrahousehold spillovers (to boys) of the elimination of secondary school fees for girls in Gambia.[11]

The paper also joins a substantial literature that has examined the effects of conditional cash transfers for school enrollment: however, the majority of this evidence focuses on primary school. Non-experimental evidence includes evidence from Cambodia that a scholarship for girls increases secondary enrollment (Filmer and Schady, 2008), and in the Dominican Republic, a government-provided CCT for secondary school increases graduation rates for both boys and girls (Hernandez et al., 2022). In both Bangladesh and Pakistan, government-provided stipends for girls in secondary school increase educational attainment (Alam et al., 2011; Hahn et al., 2018; Musaddiq and Said, 2023). In the Gambia, a similar scholarship that provides in-kind school supplies for girls increases enrollment, though with some evidence of declining school quality and test scores (Giordono and Pugatch, 2017). Other papers analyzing the large-scale elimination of school fees find positive effects for girls' enrollment and test scores in the Gambia (Blimpo et al., 2019) and delays in marriage and childbearing in Kenya (Brudevold-Newman, 2021).[12]

A number of related recent experimental studies have been conducted in sub-Saharan Africa: in Niger, a secondary school transfer for girls halved the rates of both dropout and child marriage (Giacobino et al., 2024), and an earlier trial of both unconditional and conditional transfers (conditional on attendance) for adolescent girls in Malawi found positive effects of both on school attainment in the short-term, though the effects largely attenuate to zero over time (Baird et al., 2011, 2019). Another paper evaluating a very large cash transfer in conjunction with admission to a high-quality secondary school in Uganda finds positive effects

[11]Ferreira and Sandholtz (2025) also present quasi-experimental evidence of positive intra-household spillovers linked to the abolition of secondary school fees in Tanzania.

[12]As will be described further below in the study context, there are no school fees for lower secondary school (ninth and tenth grade) in Ethiopia, implying no school fees for the enrollment years examined here, but there can be fees for eleventh and twelfth grade.

in the medium-run along multiple dimensions, including labor market and fertility outcomes (Lerva et al., 2025). A long-running randomized trial assessing scholarships for secondary school students (male and female) in Ghana finds the transfers increase educational attainment, but generate labor market gains only for women, who are better able to access government sector roles (Dupas et al., 2025a); there is also evidence of significant declines in fertility and intergenerational benefits in the form of reduced child mortality (Dupas et al., 2025b). Importantly, however, none of this literature has illuminated the anticipatory investment effect of early transfer notification: the most relevant study here is Kremer et al. (2009), demonstrating that randomized provision of a merit-based scholarship to secondary school increases test scores in late primary school for girls in rural Kenya.

Finally, we contribute to the literature around adolescent marriage, presenting rare evidence of a conditional cash transfer that effectively reduced early marriage both for girls and boys, although the risk of child marriage remains higher for girls in Ethiopia. As noted above, multiple previous papers have found that conditional cash transfers targeting adolescent school enrollment can delay marriage and/or fertility for girls (Baird et al., 2011; Brudevold-Newman, 2021; Dupas et al., 2025b; Giacobino et al., 2024; Chaudhury and Parajuli, 2010; Hahn et al., 2018; Musaddiq and Said, 2023), with similar effects for fee reduction and free school uniform programs in Kenya (Brudevold-Newman, 2021; Duflo et al., 2015). An in-kind incentive for marriage delay only (without any conditionality on school enrollment) also significantly reduced early marriage in Bangladesh (Buchmann et al., 2023). The provision of adolescent safe spaces (outside of school) has also been effective in reducing early marriage in Sierra Leone (Bandiera et al., 2020), and a big push community-level intervention (inclusive of provision of a safe space and tutoring support) dramatically reduced child marriage in Nigeria (Cohen et al., 2026).[13]

[13] A related literature has also probed the longer-term effects of more traditional cash transfer programs targeted to families with children, with no evidence of effects on marriage (but heterogeneous effects on fertility) in Honduras and Nicaragua (Millán et al., 2020; Barham et al., 2018), while in Indonesia, declining child marriage is observed in rural areas with no significant effects in urban areas (Cahyadi et al., 2020; Priebe and Sumarto, 2025). Unconditional cash transfers had no effect on early marriage for households with youth in Kenya, Malawi, or Zambia, though declines in early pregnancy are observed in Kenya (Dake et al., 2018; Handa et al., 2015).

2 Experimental design

2.1 Context

As noted above, this trial enrolled youth living in households that are beneficiaries of the Productive Safety Net Program (PSNP) in rural Ethiopia. Launched in 2005, the PSNP is one of the largest safety net programs in sub-Saharan Africa (Beegle et al., 2018), and in recent years has provided cash and/or food transfers to 8 million rural people annually (Hoddinott and Mekasha, 2020).[14] The program targets approximately the poorest 15 percent of households in the most chronically food-insecure districts, and has been shown to have generally positive effects on household food security and assets (Gilligan et al., 2009; Hoddinott et al., 2012, 2024). This trial was conducted as part of Strengthen PSNP Institutions and Resilience II (SPIR II), a five-year program (2021—2026) funded by the U.S. government and led by World Vision in partnership with ORDA Ethiopia and CARE.[15] SPIR II was implemented in vulnerable districts in Amhara, Oromia, and Tigray, supporting PSNP implementation and providing complementary livelihood and nutrition activities, including activities targeting youth.

In general, secondary school enrollment rates in Ethiopia remain relatively low, and are characterized by substantial heterogeneity across rural and urban areas. Following a large investment in primary school supply with the objective of boosting early enrollment, primary schools outnumber secondary schools nearly ten to one (Ministry of Education, 2023).[16] Moreover, enrollment requires successfully passing the primary school leaving exam; though reliable estimates on pass rates are limited, no more than half of students are estimated to pass, particularly in rural areas (Weldesilassie et al., 2025). (Those still enrolled at the conclusion of eighth grade sit the exam, and there is no out-of-pocket cost.) Detailed evidence around longitudinal trajectories of schooling collected as part of the Young Lives

[14] Estimates from another recent survey conducted by one study author suggests that the median annual transfer in the study region is around \$440 in 2011 purchasing power parity (PPP) terms, corresponding to roughly 15% of the total annualized household consumption at baseline (Hirvonen et al., 2025).

[15] SPIR II largely concluded in January 2026 — earlier than originally planned — due to the cessation of U.S. government funding.

[16] There are around 37,000 primary schools nationwide and fewer than 4,000 secondary schools.

study suggests that only about 25% of rural youth entered secondary school on time in 2016; dropout rates also begin to increase in grade seven, even prior to the conclusion of primary school (Pankhurst et al., 2018).

Formative qualitative work conducted as part of this trial suggests that for PSNP youth, financial constraints are a first-order barrier to enrollment in secondary school, given that households expect that a child enrolled in an (often distant) school will live separately during the term and incur associated costs for room and board (Leight et al., 2022). Students are not constrained to enroll in the most proximate secondary school (if any), and moving to towns for secondary school is common for rural youth (Birhanu et al., 2021; Yorke et al., 2023). (The costs do not, however, include school fees in the lower secondary years, though there can be school fees in upper secondary(Trines, 2018).)[17] Perceived physical insecurity associated with youth living away from home and perceived low school quality were also identified as barriers (Leight et al., 2022).

To characterize the secondary schools that youth in this context could plausibly access, we used data collected from those enrolled at follow-up. Across the full sample (both treatment and control arms), youth report enrollment in approximately 83 unique secondary schools, characterized by median student body size of 1000 (mean 1420) and a median number of teachers of 32, implying teacher-to-student ratios of around 30. The level of infrastructure seems at least adequate: 90% of students report their schools have some working computers as well as electricity, and virtually all report a functional library, a sports area, and latrines.

It is also important to note that this trial was implemented during a period of steadily increasing conflict and insecurity in rural Ethiopia. The Amhara region experienced a period of acute violence linked to the war in Tigray (with the northern part of the region occupied by Tigrayan forces) in 2021; following the withdrawal of these forces and a period of relative quiet, there has been renewed insecurity in Amhara since 2023 linked to conflict between the central government and regional militia forces. The Oromia region, while less acutely affected, has

[17] Virtually all available secondary schools in rural areas are public: at baseline, no student reported knowledge of any private school in his/her district, and at follow-up, only a single student reported enrollment in a private secondary school. Boarding, however, is managed privately; public schools generally do not offer boarding facilities.

also been characterized by ongoing unrest.[18]

2.2 Randomization and sample

We conducted a randomized trial with randomization at the subdistrict (kebele) level. The sample includes 116 subdistricts that were purposively selected for the trial and randomized in two phases (January and March 2023).[19] The subdistrict eligibility criteria required that the subdistrict be served by SPIR and have at least one primary school serving students in grades seven and eight; subdistricts that had secondary schools within their borders were not eligible for inclusion. (The reason for this final criterion was that youth who have a nearby secondary school typically face lower barriers to enrollment, given the reduced distance and associated costs; estimates from the Ethiopia Socioeconomic Survey suggest that in Amhara and Oromia, more than 80% of households live in a community without a secondary school.)[20] The final treatment assignment was stratified by randomization phase, district and distance to secondary school, and included 56 subdistricts assigned to the control arm and 60 subdistricts assigned to the treatment arm.[21]

Within each cluster, all youth who were living in PSNP households and who were currently enrolled in seventh or eighth grade, or who were recent eighth grade graduates who had dropped out of school, were eligible. (There was no sample stratification imposed given that the objective was to saturate the sample of eli-

[18]There was also acute violence and a protected period of post-conflict recovery in Tigray itself, but this trial was conducted solely in Amhara and Oromia. An overview of recent conflict events can be found here: https://www.cfr.org/global-conflict-tracker/conflict/conflict-ethiopia

[19]The second phase of randomization was implemented to expand the sample, given that the sample of youth recruited per cluster was smaller than projected. More details about the projected and realized sample can be found in the detailed pre-analysis plan registered post-baseline on the AEA registry site under trial number AEARCTR-0010951.

[20]The question does not specify the definition of a community, and if respondents refer to a village, that is plausibly interpreted as smaller than a subdistrict.

[21]Within each district (woreda) by phase, we defined two strata above and below the median within-district distance to the closest secondary school. Distance was calculated as the straight-line distance between the subdistrict centroid and the most proximate secondary school; the median distance is around nine kilometers across the full sample. In addition, for districts that had fewer than three sample subdistricts within their borders meeting the eligibility criteria, a "combined district" was formed with another adjacent district in pairs for the purposes of the randomization process. There were 24 randomization strata in total, 20 from the first phase of randomization and four from the second phase.

gible youth.) We identified potentially eligible youth via school visits, where we requested youth to self-identify as characterized by PSNP status, and also accessed rosters of recent eighth grade graduates (within the last two years) to screen them for dropout status; conducted name matching to identify other youth from school rosters who might be from PSNP households; and then verified eligibility through screening youth and their parents at home. More details about screening procedures are provided in the pre-analysis plan.[22] Ultimately, around 12% of youth identified in school visits were screened in as eligible, broadly consistent with the fact that around 15% of households are enrolled in the PSNP.

Assuming a base enrollment rate of 50% (approximately the rate estimated in screening surveys conducted as part of the baseline survey), the minimum detectable effect for secondary school enrollment assuming 10% attrition and an intracluster correlation of 0.05 was 19 percentage points.[23]

2.3 Intervention

The intervention was a cash transfer for secondary enrollment valued at $300 per year ($150 per semester) for youth who enrolled in secondary school in the 2023-2024 and 2024-2025 school years.[24] The grant was available to eligible students for one or two years, generally corresponding to grades nine and ten (those enrolled in the trial in seventh grade at baseline were eligible for only one year of transfer payments in the study period). Students in treatment subdistricts were initially informed of their eligibility for the cash transfer immediately following the baseline survey: given this timing, the eighth grade cohort was informed around three

[22]This trial was registered in the American Economic Association trial registry under the number AEARCTR-0010951.

[23]In the absence of any data on intracluster correlation available at the design phase, we used an indicative ICC of 0.05; again, more details are provided in the registered pre-analysis plan.

[24]The transfers were paid in birr, in a period in which the exchange rates fluctuated considerably; accordingly, the realized transfer amounts were 8,400 birr each (using an exchange rate of 56) for two installments in September 2023 and February 2024; 18,000 birr (using an exchange rate of 120) in September 2024, and 18,600 birr (using an exchange rate of 124 birr) in February 2025. While the black market exchange rate was notably higher than the official exchange rate prior to the devaluation — potentially implying that the transfer had a somewhat lower value when assessed using the black market exchange rate — it is unlikely this would be relevant for households purchasing goods and services in local markets that are substantially segmented from the international market.

months prior to the primary school leaving exam, while the seventh grade cohort had more than a year's advanced notice of transfer availability.[25]

The transfer was payable to adult caregivers of selected students in two annual payments, conditional on enrollment as verified by SPIR II staff each semester. Once disbursed, the payment could be used at the household's discretion; there was no further conditionality on receipt of or use of the transfer, beyond enrollment. The amount of the transfer was informed by estimates of accommodation costs for students living away from home as well as costs of school supplies and uniforms, estimated in our formative work to be about 1,000 birr ($20) per month for accommodation alone or around $125 per semester for five months inclusive of other costs.[26]

2.4 Data

Data were collected from enrolled youth and, if possible, their parents at baseline (February—March 2023) and in a follow-up survey conducted about 28 months later (June—July 2025).[27] The surveys included modules on educational progression, engagement in economic activities, marriage and family, non-cognitive skills, and educational aspirations. In the follow-up survey, the survey team also extensively tracked youth who had migrated to urban areas; in some cases, youth were surveyed by phone. Surveys were conducted with 2,005 youth at follow-up,

[25] Initial notification regarding transfer availability was conducted by the survey staff in each locality following the conclusion of surveys in that locality, via a household visit to each screened-in youth; the youth were provided with simple information (in both oral and written form) regarding the availability of the transfer, and contact information for local program staff. Program staff then followed up with eligible households to provide more information and complete any necessary enrollment procedures, though transfers were not ultimately delivered until enrollment of the eligible youth in the first semester of eligibility was verified.

[26] This would have also allowed for the transfer to contribute to defray school fees for students in upper secondary, if the transfer program had continued, but ultimately it did not persist.

[27] The survey was advanced around three months relative to the original planned timing of September–October 2025. While the original goal was to verify enrollment in the new school year commencing in fall 2025, uncertainty around continued funding from the U.S. government following the breakdown of USAID and the stop-work orders led the research team to advance the survey. Similarly, the research and program teams considered the option of extending eligibility for the baseline seventh graders for another years, but this was ultimately impossible due to program termination. The alteration in timing does imply that enrollment is measured at a different point in the school year.

yielding an attrition rate of only six percent. At both baseline and follow-up, informed consent was obtained from parents of minor youth, followed by assent from the youth; youth who were older than 18 provided consent directly. (Due to considerable variation in grade progression in rural Ethiopia, some youth even in grades seven and eight can be much older than the target age.)

This trial received ethical approval from the Institutional Review Board at IFPRI and the Ethiopian Society of Sociologists, Social Workers, and Anthropologists (ESSSWA). The prespecified primary outcome was enrollment in secondary school; prespecified secondary outcomes include passing rates on the primary school leaving exam; attendance and academic performance in secondary school; engagement in economic activities among youth; and early marriage.

3 Empirical findings

3.1 Baseline characteristics and balance

Table 1 reports the baseline characteristics of the youth surveyed across treatment arms, including a p-value corresponding to a regression of the covariate of interest on a treatment dummy variable, with standard errors clustered at the subdistrict level. The average age of sampled youth was nearly 16 years, rendering them around 1.8 years behind their target grade-for-age, and 45 percent were girls.[28] Only 4% are married.[29] Around 80% of youth aspire to attend university despite relatively weak academic performance in the last semester, yielding average grades in language and mathematics around 70 (on a scale of zero to 100); interestingly, every single sampled seventh and eighth grader also reported the intention to participate in the primary school leaving exam, a variable not reported in the table due to the absence of variation. The p-value corresponding to a joint test of equality across all covariates measured is $p = 0.23$, suggesting that the randomization

[28] According to official guidelines, primary school in Ethiopia begins at age seven and has a duration of six years, implying a seventh grader should be around 13 (Education Policy and Data Center, 2018).

[29] The vast majority of youth report living with a parent, stepparent, or grandparent, but 40 youth self-report they are the head of the household; there is considerable overlap between this group and those reporting marriage.

was effective in achieving balance.

We can explore balance in each cohort as well, reported in Table A1. The cohort composition of each sample cluster can vary due to random noise (varying ages of children among PSNP households), and could also be reflective of endogenous local conditions (i.e., areas that have higher dropout rates post-primary, in general, could be characterized by a higher prevalence of dropout students within the sample). Importantly, we cannot reject the hypothesis that observable characteristics are consistent across treatment arms within each cohort.

To further unpack the characteristics of the three different cohorts sampled, we also report in Table 2 a summary of covariates for students recruited in grade seven, grade eight and dropouts, pooling across the treatment and control arms; Panel B reports p-values corresponding to tests of equality across the three cohorts, from a regression including clustering at the subdistrict level. (Variables that are different by construction, such as enrollment status, are dropped, other than age as reported in the first row.) It is evident that students enrolled in seventh and eighth grades do not differ substantially on most observable characteristics; the only exception is age, and slightly higher reported aspirations for university education and self-esteem among grade eight students vis-a-vis grade seven students.

By contrast, dropout students recruited into the sample have very different observable characteristics. The dropout sample is characterized by much greater gender imbalance (only 29% female), and is (perhaps unsurprisingly) more likely to be married, consistent with their age and current enrollment status. Household asset stock and exposure to recent shocks do not significantly differ, though dropouts do show a higher probability of living in a food-insecure household (consistent with socioeconomic barriers that have led to dropout).[30] There are also several differences observed in cognitive and non-cognitive skills: aspirations for university are (unsurprisingly) lower, and reported grades in the most recent semester (prior to dropout) are also significantly lower for dropouts; self-esteem is slightly lower, but the WHO-5 score of well-being is meaningfully higher.[31] The difference in grades

[30]Households self-report the shocks they were exposed to over the previous year, including (for example) drought, floods, illness, theft, or violence.

[31]Higher subjective well-being among dropouts may suggest that continued enrollment in anticipation of a high-pressure exam at the conclusion of primary school may be generating some adverse effects for well-being for the currently enrolled youth.

is around 4% relative to the mean, though there is also a meaningful pattern of missingness (dropouts are more likely to report not recalling the grades in their final semester enrolled, and thus this gap may be a lower bound).

Figure A1 in the Appendix captures the self-reported primary reason for dropout among the baseline dropout sample. Nearly two thirds report distance to secondary school and the unaffordability of boarding costs as the primary reason for dropout; another 14% cite general financial constraints, while another 9% cite the requirement to work. Overall, around 90% seem to be identifying factors that are primarily financial — consistent with the hypothesized theory of change — while conflict, marriage, and poor school quality are only infrequently cited as underlying predictors of dropout.

3.2 Enrollment and academic outcomes

Given the randomized design, the primary treatment effects can be estimated using a simple specification, regressing outcomes of interest for individual i in subdistrict d in strata s on a binary variable for treatment assignment, including strata fixed effects and with standard errors clustered at the subdistrict level (s). We include the baseline value of the outcome of interest (Y^0_{ids}) in an ANCOVA specification, when available. We also report sharpened q-values corrected for multiple hypothesis testing following Anderson (2008); this correction is implemented within the set of all enrollment outcomes reported (Table 3), and within each set of secondary outcomes reported.[32]

$$Y_{ids} = \beta 1 T_{ds} + \eta_s + \beta_2 Y^0_{ids} + \epsilon_{ids} \quad (1)$$

We will present findings for the pooled sample, and for the various cohorts for the primary outcome (enrollment) and some secondary outcomes of interest. Given the randomized design, the estimation of treatment effects for each cohort is unbiased, but the comparison of treatment effects across cohorts is not experimental and should be interpreted cautiously; we will also return to this point further in the machine learning analysis of heterogeneity, below. For enrollment, Table 3 reports effects for the full sample in Panel A, and Panels B, C, and D focus on

[32]Each set is defined by the table in which the findings are presented.

the cohorts enrolled as grade seven students, grade eight students, and dropouts, respectively. We report estimates for three enrollment variables: ever enrolled in secondary school, current enrollment in secondary school, and current enrollment in any grade (inclusive of primary school).

Panel A suggests the transfer offer leads to a roughly 50% increase in the probability that a student self-reports ever enrolling in secondary school following the baseline survey (21 percentage points relative to a mean of 43%). In Column (2), we can see that for current enrollment, the absolute effect has attenuated to 15 percentage points, though relative to enrollment in the control arm, the relative effect has grown slightly to 62%. For current enrollment in any grade (inclusive of primary school), the absolute effect is slightly smaller (12 percentage points), but relative to a higher mean in the control arm (33%), the relative effect is noticeably lower at 36%. We also report in Columns (4) through Column (7) estimates for ever enrollment for subsamples that are of analytic interest: female and male students; and students enrolled in Amhara (characterized by widespread insecurity and associated school closures) and Oromia. In absolute magnitude, the effect is highly consistent across female and male students, though interestingly female students in the control arm have a higher probability of ever enrolling, rendering the relative effect smaller for female students (41%) than male students (60%). The effect is also slightly larger in absolute terms in Amhara, though in relative terms it is consistent across regions.

Panel B now reports the effects for seventh graders. (For this panel and subsequent panels, we will not separately report effects by gender and region, given the evidence of consistency and to avoid challenges linked to small samples.) The increase in ever enrollment is 64% for seventh graders (22 percentage points relative to a mean of 33%), and the effect on current enrollment is even larger at 80% with virtually no attenuation over time in the absolute effect (now 21 percentage points, relative to a mean of 26%). The effect on current enrollment in any grade is smaller in absolute and relative terms at 41% (17 percentage points relative to a mean of 44%), implying that more youth in the control arm have remained enrolled in primary school (after failing to pass the primary school leaving exam). The p-values document that the effect on any and current enrollment is significantly larger for the seventh grade cohort, vis-a-vis both eighth graders and dropouts.

Panel C reports parallel effects for eighth graders. The effect on ever enrollment here is meaningfully smaller at 18% (11 percentage points relative to a mean of 60%), and the effect on current enrollment is 37% (11 percentage points relative to a mean in the control arm of 30%). In other words, treatment youth have shown more persistence in secondary school vis-a-vis youth in the control arm. The effect on any enrollment is 25% (9 percentage points relative to 35% in the control arm), again suggestive of higher rates of grade repetition among the control youth.

Finally, Panel D reports the effects for dropouts. Here, the treatment effect on ever enrolled is fully 159% (41 percentage points, relative to a mean of 26%). For current enrollment, the relative effect has only slightly declined to 148%, while the absolute effect drops dramatically to nine percentage points (relative to a mean of only 6%). Put more succinctly: the vast majority of dropouts who attempted secondary school again have dropped out again, and the cash transfer did not meaningfully alter this dynamic. Nonetheless, by virtue of encouraging significantly more students to attempt secondary school again, the transfer offer did meaningfully increase the completed months of school for all cohorts inclusive of dropouts; Table A4 in the Appendix documents an at least three month increase in months of school attended during the period of follow-up, an effect that is in fact somewhat larger for dropouts at 4.3 months (given the very large short-term response in re-enrollment).[33]

Table 4 then further probes treatment effects on academic outcomes of interest: Panel A focuses on the treatment effect for probability of participating in and passing the primary school leaving exam for baseline seventh and eighth graders. For seventh graders, the offer of a cash transfer conditional on secondary enrollment leads to a 12% increase in the probability of participating in the exam (seven percentage points relative to a mean of 62%), and a 28% increase in the probability of passing the exam (13 percentage points relative to a mean of 45%). The comparable effects for eighth graders are essentially zero, unsurprising given the limited margin for response for eighth graders informed of the transfer shortly before the

[33] Given that cluster-level randomization was stratified based on median distance to secondary school, we can also assess whether there is any heterogeneity along the distance dimension; there is no evidence of such heterogeneity.

primary school leaving exam (virtually all do participate in the exam, and around 75% pass).[34] A simple decomposition suggests that the increased probability of passing the primary school leaving exam is equivalent to about 60% of the total experimental effect for secondary school enrollment for seventh graders.

Some caution in interpretation is warranted given that seventh and eighth graders likely differ along unobservable characteristics despite their similarity in observable characteristics as previously reported: the notably lower passing rate among seventh graders in the control arm, vis-a-vis eighth graders in the control arm, is strongly suggestive of a compositional shift in which the seventh grade cohort plausibly includes more marginal students, and the treatment effect might be larger for these students. To explore this hypothesis, we regress the probability of passing the primary school exit exam on baseline covariates and covariates interacted with treatment for the seventh and eighth grade cohorts, as reported in Table A2, and then generate a predicted probability of passing; we use this to assess whether there is any heterogeneity in the treatment effect for seventh and eighth graders with respect to predicted probability of passing.

The findings reported in Table A3 indicate that among seventh graders, the treatment effect on passing rates is concentrated among students with a lower predicted probability of passing (a 12 percentage point increase, relative to a control mean of 36%), while seventh graders with a higher predicted probability of passing show a small, statistically insignificant, and positive treatment effect; however, the corresponding treatment effects on enrollment are large (16 and 20 percentage points) in both subsamples. Among eighth graders, by contrast, the treatment effects on passing rates are uniformly insignificant, while the enrollment effects are sharply heterogeneous: a 15.8 percentage point increase among marginal students (relative to a control mean of 23%), and a five percentage point increase among

[34] The notably lower participation and passing rates in the control arm among students enrolled in seventh grade vis-a-vis eighth grade at baseline is suggestive of some compositional shift: those still enrolled in school in eighth grade three months before the exam are clearly those characterized by a relatively high probability of participating and passing, relative to their younger counterparts; many seventh graders in the control arm abandon their stated plans of participating in the primary school leaving exam, and many more do not pass. This is despite the fact that average self-reported academic performance was no different across seventh and eighth graders at baseline. This selection process out of exam participation may be driven by schools / teachers, families, or students themselves, and is consistent with other evidence around processes of educational triage in sub-Saharan Africa (Gilligan et al., 2022).

non-marginal students that is statistically insignificant. With late notification, it seems the short window encourages re-optimization of school versus work decisions only for marginal students – those for whom small adjustments at the enrollment margin are decisive. With early notification, the planning window is wide enough that both marginal and non-marginal students respond, the former through the joint passing-and-enrollment channel, the latter through a planning channel that operates independently of the academic constraint. The pattern is accordingly inconsistent with the hypothesis that the larger seventh-grade treatment effect is driven solely by a compositional shift toward more marginal students.

Returning to Panel B of Table 4, we can assess effects on other academic outcomes of interest observed only for students enrolled in school. This analysis is non-experimental by construction, since these outcomes are not defined for non-enrolled students, and the composition of enrolled students is shifting in the treatment arm vis-a-vis the control arm; in general, if the treatment induces enrollment from more marginal students, we anticipate that any bias on variables capturing academic investment should be downward. There is no shift in self-reported attendance (percent of school days attended) in Column (1), relative to a mean of nearly perfect attendance in the control arm,[35] and no shift in the average reported grades in the last semester: that is, even the marginally enrolled student in the treatment arm performs equally well once enrolled in school. We do observe an increase in the number of hours per day devoted to attending school and completing academic work out of school, of around 0.3 hours (Column 3). Aspirations of achieving at least a university education do not change (Column 4), relative to a high base rate of 76% in the control arm.[36] All of these outcomes are suggestive of educational outcomes of consistent or weakly increasing intensity and quality.[37]

Given the increased enrollment, how much more did treatment households

[35]Attendance is self-reported by students, who report the number of days they attended school out of the last five days in which the school was open.

[36]Educational aspirations was not measured for those currently out of school.

[37]Another channel for potentially declining educational outcomes could be increased congestion at secondary schools, but this seems unlikely given the limited scope of an intervention targeting PSNP youth only: this intervention induced an average of only 17 new students ever-enrolled in secondary school per district, and given the number of unique secondary schools observed among enrollees, this implies an additional three students per school or .3% of enrollment at the typical school observed in the sample.

spend on all school-related investments (as facilitated by the transfer)? Table A5 in the Appendix reports treatment effects on schooling expenditure, self-reported by the youth for the past semester.[38] It is evident that the treatment effect on expenditure is only about \$15, when estimated either unconditionally or conditional on current enrollment (in Column 4): about two thirds of this additional expenditure corresponds to additional costs of food and lodging away from home.[39] Total expenditure for enrolled youth in the control arm is \$51 and in the treatment arm around \$65, and in both cases around half of the costs is devoted to lodging and food (slightly higher in the treatment arm). We can see in Panel B that students in the treatment arm are much less likely to live with their parents during the week, and are more likely to live with other youth away from home (but no more likely to live with friends or relatives). Clearly, the increase in expenditure is dramatically smaller than the value of the transfer (\$150), even allowing for the fact that some youth may underestimate the relevant costs due to the long recall period and the fact that parents may manage some outlays; we will return to this point below.

We can also explore if the transfer offer induced any shifts in school choice. Again, this analysis is non-experimental by construction, since the choice of school is only observed for enrolled students, but it allows us to assess whether differential school choice may be a channel for the observed effects.[40] Within the sample of youth enrolled in secondary school who report a school name, we construct a binary variable equal to one if they attend a school different from the secondary school originally identified as most proximate to their baseline subdistrict, and an infrastructure index corresponding to the school of their choice; the objective is to test whether treatment induces youth to select different, higher-quality schools.[41]

[38] We convert the estimates to dollars using the six-month average of the exchange rate for the period preceding the survey.

[39] The youth were requested to estimate the costs of lodging and food incurred away from home, with the objective of capturing the marginal costs associated with schooling.

[40] We use fuzzy string matching to identify unique schools within the set of schools identified by youth, though 41 do not have a legible school name recorded; we drop school names reported by only a single student. This yields a set of 83 unique schools, larger than the set of 63 secondary schools identified by partners at baseline in the sample districts. This may be partly because students attended schools in other districts (where they had familial ties etc.), or could reflect newer schools that were not identified at baseline. At baseline, we identified the secondary school within this set of 63 that is most proximate using straight-line distance to the centroid of each subdistrict, and we deem this to be the most proximate known secondary school.

[41] The school infrastructure index is constructed by taking the first principal component of

We also analyze treatment effects on self-reported travel time from parental residence to the school (reported even if the student resides away during term time, and a proxy for distance to the chosen school). The findings reported in Panel C of Table A5 suggest there is relatively little evidence of differential patterns of school choice induced by treatment: in fact, only a minority of youth even in the control arm attend the secondary school most proximate to their subdistrict, and there is no significant shift in the treatment arm. There is no evidence that treatment youth are attending schools characterized by better infrastructure, and while the treatment effect on the self-reported commute in minutes is positive, it is not significant.[42] Overall, these findings suggest that the offer of the transfer is not shifting students' choice of schools, perhaps reflecting the absence of meaningful variation in quality within the secondary schools available.

3.3 Other outcomes

Given the evidence of treatment youths' increased engagement in school, Table 5 then explores effects on a range of variables linked to their marital status. In Column (1), there is a significant but small decline in the probability of a youth contracting a marriage prior to age 18: slightly under two percentage points relative to a mean in the control arm of 6%, for a proportional effect of nearly a third. (The effect is narrowly insignificant using sharpened q-values; though the declines for the seventh grade cohort is significant even using sharpened q-values.)[43] A decline in the probability of reporting any children is slightly smaller in sign and not statistically significant. For youth who are not yet married, we also observe a significant increase in the self-reported minimum desired age of marriage, as noted in Column (3), of roughly 0.8 years relative to a mean target age of 23. For the small sample of youth who did marry, we do not observe any compositional shift in the spouse's age or reported education (Columns 4 and 5) — an analysis that is likely underpowered.

the eight binary amenity indicators reported for the school, standardizing it to mean zero and standard deviation one, and then averaging across all student reports for the same school.

[42]Note that the commute time is reported even for the youth who did not report an accurate or interpretable school name, rendering the sample slightly larger.

[43]This variable is coded as missing for youth who were already married at baseline.

In Panel B, we seek to unpack the decline in early marriage in the cohorts enrolled at baseline, and show it is entirely driven by a larger decline (2.9 percentage points) among baseline seventh graders. This partly reflects the fact that given their younger age, they were mechanically more vulnerable to contracting an early marriage, and also reflects the different composition of the seventh grade sample (inclusive of a higher share of academically marginal students, as identified by their lower ultimate passing rates). The estimated coefficient for eighth graders is comparable in magnitude to the pooled coefficient but not statistically significant, while there is no evidence of any effect among dropouts. Counterintuitively, the effect is in fact slightly larger for boys than girls, as observed in Columns (4) and (5), though the rate of child marriage in the control arm is much higher for girls than boys (10% versus 2%). Within the seventh and eighth graders, the reduction in marriage is also concentrated among the marginal students with a low probability of passing, as observed in Columns (6) and (7). This suggests that the decline in early marriage is not observed among the (ultimate) passers who are eligible for, and may enter, secondary school. Rather, it reflects a decline in early marriage among the academically weakest students (primarily seventh graders), who ultimately do not pass the primary school leaving exam, but nonetheless persist longer in school to attempt it and are thus less likely to enter an early marriage.

Panel A of Table 6 then reports experimental effects for cognitive and non-cognitive outcomes; the evidence around non-cognitive outcomes, in particular, is relatively novel in this literature.[44] We can see that the treatment leads to positive but small shifts in indices of both self-esteem and agency, with the magnitude between 0.04 and 0.08 standard deviations relative to the control arm. There is no shift in reported migration intentions, however; 75% of the control arm expresses a desire to migrate, and this does not shift in the treatment arm. There is a nine percentage point increase in the probability of reporting professional aspirations (defined as reporting a desire to pursue a role as a teacher, doctor, other medical worker, or civil servant); there is also a weak positive shift in subjective well-being as captured by the WHO score, reported in Column (5).

Columns (6) through (8) report cognitive outcomes. We observe no significant shifts in reported scores on an advanced Raven's matrix score or the score on a

[44] These variables are not prespecified and should be considered exploratory.

forward digit span test. For the score on a mathematics module based on the lower secondary school curriculum, we do see a weak positive effect (4% relative to the control mean) but it is not statistically significant at conventional levels; performance is also notably low in the control arm, where the average student answers only three out of ten questions correctly. Overall, there is little evidence that even substantial increases in secondary school enrollment have led youth to accumulate more human capital, with the caveat that the available data is not extensive; this is consistent with findings of weak positive effects on cognitive test scores in Giacobino et al. (2024) and Baird et al. (2019).[45] It is also consistent with growing debates highlighting challenges linked to poor educational quality in Ethiopian secondary schools, where pass rates on the exit exam determining access to tertiary education hover in the low single digits (Addis Insight, 2025).[46]

We also explored treatment effects on self-reported engagement in labor, both non-agricultural and agricultural. Despite their age and (for some) continued enrollment in school, nearly 80% of youth report engagement in agricultural labor over the past week, and nearly half report engagement in non-agricultural labor (in both cases, substantially driven by working in family enterprises).[47] Given that the survey was conducted right around the conclusion of the school year (and classes might have already concluded in some locations), these estimates could be somewhat higher than what would be observed during more intensive periods of schooling. In any case, there is no evidence of any significant treatment effects as reported in Panel B of the same table: i.e., the transfer does not lead to a reduction in labor hours by targeted youth or any substitution to new economic activities. This absence of any labor-supply response is notable, since the substantial induced increase in secondary enrollment might be expected to crowd out work for the marginal enrollees; the most plausible interpretation is that youth labor in this setting is predominantly intermittent work within family enterprises that coexists with school attendance, rather than full-time employment that schooling would

[45] In Baird et al. (2011), there are meaningful effects on cognitive skills only for the sample of baseline dropouts.

[46] Historically, pass rates were closer to 50%, but since the enactment of strict anticheating measures requiring centralized exam scoring, pass rates have plummeted.

[47] Around a third of youth report that they have worked for anyone who is not a member of their household over the past year.

displace.

Finally, Panel C explores whether there are positive effects on household socioeconomic outcomes, using variables as reported by the youth himself / herself for his / her primary household of residence; this regression is estimated at the household level, rather than including multiple observations for those cases where multiple sample youth live in the same household. (For youth who board away during the school term but remain resident with their natal household outside of the term, they were advised to answer questions for the latter.)[48] Here, we observe notably positive effects. Households that receive the transfer show a significantly reduced probability of moderate or severe food insecurity (6 percentage points, relative to the mean in the control arm of 85%); they also show a roughly 0.2 standard deviation increase in an index of household durable assets. There is also a significant increase in the number of younger co-resident children — usually siblings — enrolled in school, measured within the set of children who were resident in the sample child's household at baseline and aged four to 16 at that time (i.e., aged six to 18 at follow-up): the number of other children resident in the household enrolled in school increases by about 13%, an effect that is roughly balanced between boys and girls.[49]

A final outcome of interest is migration for the sample youth, and here we report treatment effects in Table A 6. Overall, rates of migration remain relatively low in this sample: 9% of youth have departed from the baseline household, of which 7.6% have migrated within the baseline region, and 1.7% have migrated outside of their baseline region (inclusive of international migration). The treatment effects for all three migration variables are negative, though only the third is significant (or weakly significant when employing sharpened q -values): treatment reduces the probability of long-distance migration by half, albeit from a low base. This suggests that the transfer offer facilitating accumulation of human capital locally

[48]Given that the majority of youth are not yet married at this point and do not self-identify as head of households, they are generally reporting for a household where a parent, grandparent, or other relative is identified as head of household; however, given their relatively more advanced age at the point of the survey, they were viewed as credible reporters of the household' socioeconomic status.)

[49]In practice, these other co-resident children are likely overwhelmingly siblings, but in some case could include cousins, foster children, etc.

does have some (small) effect in deterring high-cost forms of migration.[50]

3.4 Enrollment data reported by schools

While our primary findings rely on self-reported data, we also access administrative data on enrollment compiled from secondary schools. Given the wide variation in secondary schools attended within the sample, and the absence of any systematically digitized or unified administrative data in this context, this is better conceptualized as "quasi-administrative data": the survey firm compiled reports on enrollment for target students from school officials, triangulating in some cases with students / households who would report the school in which they had enrolled (followed by verification directly with the school).[51] Another caveat regarding this data is that a small number of corrections were also provided (to the research team) by staff of the implementing organizations who were cross-verifying enrollment prior to payment, but these corrections were only for youth in the treatment arm. Data is near-universal, but excludes (by construction) seventh graders in the first year post-baseline, when they would have enrolled in eighth grade.

Estimated treatment effects on enrollment as observed in the school-reported data are reported in Table A7; as school-reported data is as of a particular year, we also report the parallel effects for the same period in the self-reported data. We observe consistently positive effects that are in fact significantly larger using school-reported data for baseline seventh and eighth graders in Panels A and B (particularly Panel B, where the estimated gap in treatment effects is around 12–15 percentage points, larger using school-reported data), though roughly consistent for dropout students (Panel C). Importantly, recorded enrollment rates are, in general, much lower using the school-reported data, and the gap is notably larger in the control arm (youth in this arm report enrollment at a rate much higher than that observed in school-reported data), driving the differential treatment effect.

The overall discrepancy between the two data sources could reflect social de-

[50] Conversely, there is certainly no evidence that youth enroll to access a cash transfer and subsequently migrate using the cash.

[51] As noted above, there is wide variation in the identity of schools that targeted youth attend, and accordingly it was infeasible to simply check enrollment registers in a local district school. Verification of enrollment with the school was conducted either by phone or in person.

sirability bias in self-reporting enrollment (that differentially affects the control youth since they are less likely to be truly enrolled) or a very low quality of school data (or both). That being said, we cannot rule out a treatment-induced shift in the quality of administrative data (i.e., if youth eligible for the transfer have ensured that their registration is promptly and accurately recorded at the school level, while youth who are not eligible have not done so; corrections provided by the NGO partner may also have contributed to this gap). We thus interpret our reliance on the self-reported data as conservative, and note that if anything, social desirability bias may be leading us to underestimate the underlying treatment effect.

3.5 Attrition

This trial achieved a low attrition rate of only 6% (notable particularly for a youth sample), and there is no significant difference comparing across the treatment and control arms. Table A8 reports a simple specification in which a binary variable for attrited is regressed on baseline characteristics of interest (the same characteristics reported in Table 1), and the interaction between baseline characteristics and treatment. In general, there is no evidence that any of these variables are significant predictors of attrition status, and the joint p-value estimated across all baseline - treatment interactions similarly fails to reject ($p = 0.423$).

3.6 Cost-effectiveness

We can also estimate the cost-effectiveness of this transfer relative to other cash transfers targeting enhanced enrollment in secondary school, focusing on the cost per additional year of secondary schooling. We focus on treatment effect estimates on student enrollment, and benchmark these treatment effects relative to the annual transfer value reported in 2021 PPP terms.[52] This yields a sample of five papers that report treatment effect estimates that are comparable, including this paper, Baird et al. (2011) with estimates of the effectiveness of both unconditional

[52] We abstract then from other programmatic costs, including the costs of targeting, if applicable, and program administration or rollout; this is partly by necessity as few of the comparison papers report comprehensive cost estimates.

and conditional cash transfers, Filmer and Schady (2008), Giacobino et al. (2024), and Lerva et al. (2025).[53] This estimate thus by necessity abstracts from other, potentially multidimensional benefits of the transfer, including enhanced human capital, delayed marriage, or other positive effects on socioeconomic characteristics at the household level.

The findings are shown in Figure A2; the first subfigure shows the full set of papers, and the second subfigure excludes Lerva et al. (2025), a high outlier in terms of cost. The costs of one additional year of secondary schooling induced vary from $884 in Cambodia up to around $20,000 in the case of Lerva et al.; the cost per additional year of schooling for the transfer examined here is $3258 in PPP terms.[54] The transfer here seems slightly better in terms of cost-effectiveness than Giacobino et al. (2024) — estimated cost per additional year of schooling $4000 — while the CCT implemented in Baird et al. was slightly more cost-effective at $2440; the transfer evaluated in Filmer and Schady (2008) is a notable positive outlier, at a cost per additional enrollment year of less than $1000, though this is also the only estimated derived from a quasi-experimental analysis rather than a randomized controlled trial.

Overall, these findings suggest that this transfer is one of the most cost-effective transfer programs evaluated in inducing additional enrollment in secondary school. Moreover, it is estimated to be even more cost-effective when using the (larger) treatment effect estimate derived from administrative data: this yields a cost per additional year of secondary school induced of $995, only 10% higher than the lowest estimate here from Filmer and Schady (2008), and lower than the other experimental estimates. In light of the low observed levels of secondary school expenditure, a lower transfer might be equally effective (and even more cost-effective), though this remains speculative.

[53] A range of other relevant papers cited in the literature review are not included in this comparison because they do not report comparable effects on this outcome, reporting (for example) only effects on graduation from secondary school, or cumulative months of school, or other relevant but distinct outcomes.

[54] The corresponding estimate for the seventh grade cohort only is around 4% lower, at $3138.

4 Unpacking heterogeneous effects

Overall, our findings suggest that the offer of a conditional cash transfer for school enrollment leads to a significant and large increase in enrollment in secondary school two years later, with the effects largest among students enrolled in seventh grade at baseline. These effects are also quite large relative to the distribution of treatment effects on enrollment observed in the broader literature on educational interventions: the effect on current enrollment in the pooled sample is 0.25 standard deviations and the effect on current enrollment in the seventh grade sample is 0.35 standard deviations, and both are above the 80th percentile of the distribution of effect sizes reported in Evans and Yuan (2022) (though of course, cash transfers are a very costly intervention). The large magnitude of the observed effect is also consistent with the targeting of the transfer to extremely poor households selected on the basis of distance from secondary schools, implying the existence of high barriers to access that may be effectively targeted by a cash transfer.

Given the preliminary evidence of treatment effects that show substantial variation across various cohorts of interest, we now use a more structured machine learning algorithm to explore treatment effect heterogeneity using a generalized random forest (GRF) (Athey et al., 2019), allowing for a data-driven exploration of heterogeneity across a rich set of baseline covariates. The GRF algorithm builds a causal random forest (CRF) that allows for the estimation of conditional average treatment effects, conditional on observable baseline characteristics; we employ the same set of baseline characteristics previously employed in the balance tests and tests of covariates across cohorts. (Wager and Athey, 2018; Davis and Heller, 2017).[55]

The first step is simply to assess how much heterogeneity in treatment effects is evident, and here we focus on the estimated treatment effect for current (at follow-up) secondary school enrollment. We estimate what is known as the "out-of-bag" conditional average treatment effect (CATE) — in which the treatment effect for each observation is predicted using only the trees for which that observation was not used in the training set — and present the distribution in Figure 1. It is evident that there is substantial mass for an effect on enrollment between 15 and

[55]We use the GRF algorithm in R.

20 percentage points — but also both lower and upper right tails.

Next, we categorize clusters into quartiles based on the predicted conditional average treatment effect and compare mean baseline observable characteristics at the cluster level in the highest and lowest quartiles. (This analysis is conducted at the cluster level to align with the unit of randomization.) We then plot the resulting differences standardized by the pooled standard deviation across the top and bottom quartiles, restricting attention to covariates for which the standardized difference is at least 0.2 standard deviations (Figure 2). Youth in clusters in the top quartile of predicted treatment effects generally have larger baseline household size, better-educated social networks, and a substantially higher share of seventh-grade students; they are also characterized by higher baseline self-esteem, aspirations, well-being, and academic performance. Interestingly, youth in these clusters also exhibit slightly higher baseline food insecurity. In contrast, female gender, baseline dropout status, and exposure to recent shocks are more prevalent among youth in clusters in the bottom quartile of predicted treatment effects.

Given the high level of correlation between these observable characteristics, how can we identify what characteristic is most predictive of variation in the CATE? We then probe the variable importance estimated by the GRF algorithm: this summarizes the relative contribution of each covariate to predicting treatment effect heterogeneity conditional on the full set of observables. The findings reported in Table 7 suggest that the most important variable predicting heterogeneity is recent academic performance followed by baseline enrollment in grade seven and the baseline WHO score. Two socioeconomic variables (household size and asset index) and age are also highly predictive, followed only then by baseline dropout status. This pattern suggests that although dropout status is strongly correlated with predicted treatment effects, its association primarily reflects other underlying characteristics—such as lower academic performance, differences in well-being, and higher age—-rather than an independent role in shaping treatment effect heterogeneity. Conditional on these factors, dropout status itself contributes little additional predictive power. By contrast, baseline enrollment in grade seven remains highly predictive even after conditioning on age and other observable characteristics (consistent with the evidence previously shown that there was no heterogeneity in the treatment effect within the seventh grade sample driven by the predicted probability of passing).

We can further assess the relative predictive power of baseline dropout status by estimating the difference in mean conditional average treatment effect across dropouts and non-dropouts (around five percentage points); and then estimating the difference for all observations conditional on their observed characteristics while shifting only the binary dropout variable, from zero to one. In the latter counterfactual exercise, the conditional average treatment effect decreases by only one percentage point. This suggests that around 80% of the difference in CATE comparing across dropouts and non-dropouts is driven by other observable characteristics. A similar comparison for the seventh grade cohort, by contrast, yields a five percentage point difference in the mean CATE comparing across seventh graders and non-seventh graders, and a shift in seventh grade status conditional on all observable characteristics leads to a four percentage point increase in CATE, suggesting that 80% of the difference for seventh graders is driven by that single observable characteristic. This is consistent with the interpretation that the seventh graders' early enrollment in the cash transfer program allows them more time to respond to the incentive offered, particularly by increasing the probability they successfully complete primary school. There is little evidence that the larger treatment effect is driven by any differential observable or unobservable characteristics of seventh graders.

It is also useful to link this evidence around the role of observables in predicting a lower conditional average treatment effect for dropout students to the primary reason for dropout self-reported at baseline (Figure A1). The majority of dropouts self-identified financial constraints as primary, but the high rate of continued dropout following the alleviation of this constraint — as well as the findings from the generalized random forest analysis — suggest that this may be an oversimplification. Financial constraints do exist, but low academic performance (a challenge rarely self-identified by the youth themselves) seems to also meaningfully shape the observed treatment effects.

5 Conclusion

This paper provides evidence around the effectiveness of a conditional cash transfer for secondary school enrollment, offered to a sample of youth from extremely poor Ethiopian households who are enrolled in the final two years of primary school, or who are primary school graduates who have not continued their school enrollment. The findings suggest that a conditional cash transfer increases school enrollment for at least two years, reduces the incidence of child marriage, and somewhat enhances non-cognitive skills and aspirations; there are also significant positive effects on household economic outcomes, and positive spillovers on school enrollment for younger siblings. Moreover, the effectiveness of the transfer is around 60% larger when offered to students one year prior to the completion of primary school, consistent with the hypothesis that facilitating earlier planning for longer-term human capital accumulation is important.

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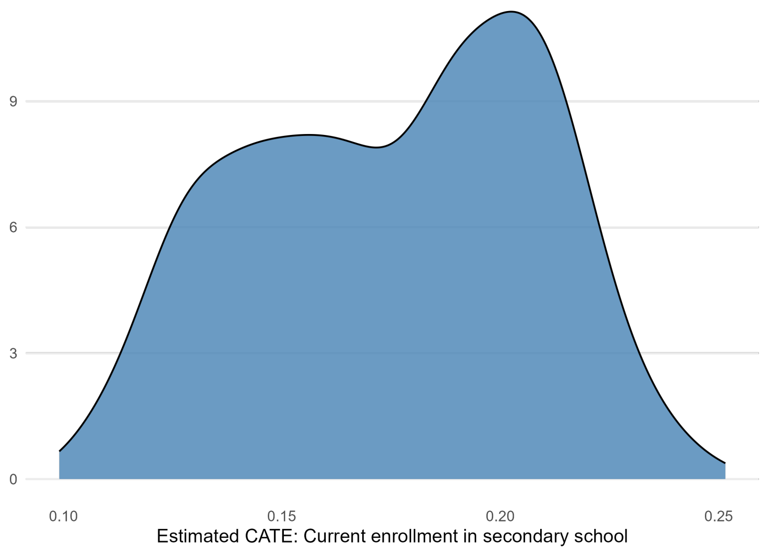
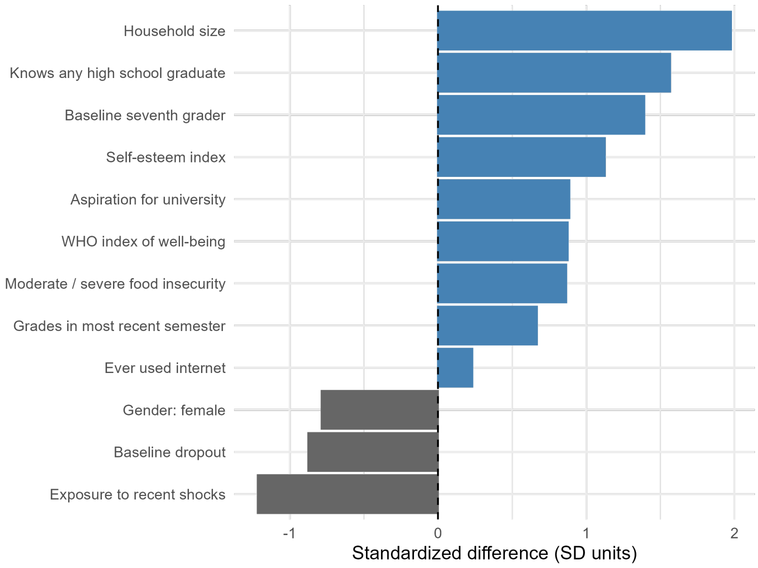
Figure 1: Conditional average treatment effect: Density function



Notes: This figure captures the conditional average treatment effect for enrollment as estimated using the generalized random forest.

Figure 2: Covariates comparing across the top and bottom quartile of CATE



Notes: This figure captures the difference in baseline covariates comparing across clusters characterized by the highest and lowest conditional average treatment effect, standardized relative to the standard deviation of those covariates observed in the control arm.

Table 1: Baseline balance

	N	Treatment mean	Control mean	Diff.	p-value
Age	2141	15.992	15.769	.223	.222
Youth gender	2134	.443	.453	-.01	.788
Highest grade completed	2141	6.838	6.78	.058	.227
Currently married	2141	.038	.042	-.004	.717
Household size	2133	6.011	5.771	.24	.285
Food insecurity	2133	.736	.745	-.01	.817
Household assets	2133	2.94	2.893	.047	.615
Recent household shocks	2133	3.241	3.29	-.049	.644
Knows high school graduate	2141	.626	.584	.042	.303
Ever used internet	2054	.082	.065	.017	.351
Aspiration for university	2141	.817	.787	.03	.193
Academic performance	1822	72.474	72.919	-.444	.66
WHO-5 well-being score	2141	12.296	12.496	-.2	.633
Self-esteem index	2141	5.042	5.016	.027	.667

Notes: The difference column is the coefficient on treatment from a regression of each baseline variable on a treatment dummy, with standard errors clustered at the subdistrict level.

Table 2: Baseline covariates by cohort

Panel A: Baseline means

	Grade 7	Grade 8	Dropout
Age	14.89	15.78	18.16
Youth gender	0.49	0.48	0.29
Currently married	0.02	0.01	0.15
Household size	5.91	5.88	5.90
Food insecurity	0.72	0.74	0.79
Household assets	2.88	2.92	3.00
Recent household shocks	3.25	3.26	3.30
Knows high school graduate	0.57	0.60	0.69
Ever used internet	0.06	0.08	0.09
WHO-5 well-being score	11.86	12.19	13.91
Aspiration for university	0.79	0.84	0.74
Self-esteem index	4.96	5.16	4.91
Academic performance	72.95	73.46	70.03
N	858	869	414

Panel B: Pairwise equality tests (cluster-robust p-values)

	Test: G7=G8	Test: G7=Dropout	Test: G8=Dropout
Age	0	0	0
Youth gender	.919	0	0
Currently married	.441	0	0
Household size	.745	.926	.875
Food insecurity	.397	.004	.05
Household assets	.485	.107	.25
Recent household shocks	.845	.585	.696
Knows high school graduate	.221	0	.002
Ever used internet	.219	.248	.712
WHO-5 well-being score	.226	0	0
Aspiration for university	.004	.06	0
Self-esteem index	0	.462	0
Academic performance	.49	.001	0

Notes: This table reports baseline covariates by cohort, for youth recruited into the sample as eighth graders, seventh graders, or post-primary dropouts. Panel A reports subgroup means at baseline. Panel B reports pairwise p-values from regressions of each baseline variable on subgroup indicators with standard errors clustered at the subdistrict level.

Table 3: Treatment effects: school enrollment

	(1) Ever enrolled in secondary school	(2) Currently enrolled in secondary school	(3) Currently enrolled in any grade	(4) Ever enrolled in secondary school (Female)	(5) Ever enrolled in secondary school (Male)	(6) Ever enrolled in secondary school (Amhara)	(7) Ever enrolled in secondary school (Oromia)
				Panel A: Full sample			
Treatment	0.214***	0.147***	0.120***	0.206***	0.223***	0.254***	0.180***
	(0.033)	(0.033)	(0.033)	(0.039)	(0.037)	(0.046)	(0.041)
	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]
Control Mean	0.427	0.239	0.337	0.487	0.375	0.527	0.330
N	2,005	2,005	2,005	907	1,093	995	1,010
				Panel B: Enrolled in Grade 7 at baseline			
Treatment	0.218***	0.210***	0.170***				
	(0.047)	(0.048)	(0.045)				
	[0.000]	[0.000]	[0.000]				
Grade 7 = Grade 8	0.076	0.078	0.116				
Grade 7 = Dropout	0.020	0.063	0.155				
Control Mean	0.338	0.262	0.441				
N	805	805	805				
				Panel C: Enrolled in Grade 8 at baseline			
Treatment	0.109**	0.117**	0.091*				
	(0.046)	(0.045)	(0.046)				
	[0.023]	[0.014]	[0.051]				
Grade 8 = Dropout	0.000	0.645	0.919				
Control Mean	0.602	0.300	0.355				
N	820	820	820				
				Panel D: Dropouts at baseline			
Treatment	0.408***	0.092**	0.085**				
	(0.058)	(0.039)	(0.041)				
	[0.000]	[0.023]	[0.042]				
Control Mean	0.257	0.061	0.067				
N	380	380	380				

Notes: $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. All regressions include strata fixed effects and controls for the baseline outcome variable, if available; standard errors are clustered at the subdistrict level.The variables reported in Panels A, B and C are enrollment variables as specified in the column labels. Panel A reports results for the full set of cohorts (with subsamples reported in Columns 4 through 7). Panels B through D report results for the cohorts of youth enrolled in seventh and eighth grade and as dropouts, respectively. The final rows report p-values from pairwise tests of the equality of the treatment effect across cohorts, for each of the three enrollment outcomes (Columns 1 through 3), estimated from a pooled model that interacts cohort with treatment and with the strata fixed effects.

Table 4: Treatment effects: academic outcomes

	(1)	(2)	(3)	(4)
	Panel A: Primary school leaving exam			
	Baseline 7th graders		Baseline 8th graders	
	Participated	Passed	Participated	Passed
Treatment	0.065	0.128**	0.007	0.022
	(0.041)	(0.049)	(0.018)	(0.044)
	[0.304]	[0.040]	[0.733]	[0.733]
Control Mean	0.62	0.45	0.93	0.73
N	805	805	820	820
	Panel B: Academic outcomes (enrolled in secondary school)			
	Attendance	Hours on schoolwork	Last semester grades	Aspirations: university+
Treatment	-0.004	0.327***	-0.440	0.025
	(0.010)	(0.120)	(1.286)	(0.022)
	[0.733]	[0.040]	[0.733]	[0.531]
Control Mean	0.98	10.01	70.06	0.92
N	623	633	633	633

Notes: $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. The variables reported in Panel A are binary variables for participating in and passing the primary school leaving exam, for baseline seventh and eighth graders respectively. The variables reported in Panel B are reported only for youth currently enrolled in secondary school and include attendance, average grades in the most recent semester, total hours devoted to school and academic work in a typical day, and a binary variable for stated aspiration of at least a university education. All regressions include strata fixed effects and controls for the baseline outcome variable, if available; standard errors are clustered at the subdistrict level.

Table 5: Treatment effects: Marital status

Panel A: Full Sample

	(1)	(2)	(3)	(4)	(5)
	Early marriage	Has children	Minimum desired age of marriage	Spouse's age	Spouse's education
Treatment	-0.019**	-0.016	0.765***	1.052	-1.069
	(0.010)	(0.013)	(0.238)	(0.736)	(1.409)
	[0.115]	[0.383]	[0.020]	[0.313]	[0.516]
Control Mean	0.06	0.07	22.73	22.95	5.47
N	1,927	2,005	1,720	241	241

Panel B: Effects on early marriage by cohort and subsample

	Baseline 7th graders	Baseline 8th graders	Baseline dropouts	Females	Males	Below-median predicted passing	Above-median predicted passing
Treatment	-0.029**	-0.019	0.018	-0.014	-0.018**	-0.029**	-0.012
	(0.012)	(0.019)	(0.019)	(0.020)	(0.007)	(0.012)	(0.019)
	[0.054]	[0.454]	[0.468]	[0.516]	[0.054]	[0.054]	[0.544]
Control Mean	0.05	0.07	0.03	0.10	0.02	0.05	0.06
N	791	812	324	879	1,043	674	687

Notes: $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The variables reported in Panel A include a binary variable for early marriage, a binary variable for reporting any children, the minimum desired age of marriage reported by youth who are unmarried at follow-up, and the reported age and education level of the spouse for youth reported married at follow-up. Panel B reports findings for the binary variable for early marriage, reported for the various specified cohorts. All regressions include strata fixed effects and controls for the baseline outcome variable, if available; standard errors are clustered at the subdistrict level.

Table 6: Treatment effects: Skills, labor supply, and household outcomes

Panel A: Cognitive and non-cognitive skills

	(1) Self-esteem	(2) Self-agency	(3) Migration intention	(4) Prof. aspirations	(5) WHO score	(6) Raven matrix	(7) Forward digit span	(8) Math module
Treatment	0.081**	0.039***	0.004	0.087***	0.411*	-0.044	0.108	0.161
	(0.031)	(0.015)	(0.018)	(0.023)	(0.216)	(0.077)	(0.082)	(0.117)
	[0.032]	[0.032]	[0.930]	[0.003]	[0.119]	[0.783]	[0.311]	[0.305]
Control Mean	0.22	0.26	0.75	0.59	9.82	2.04	5.46	2.91
N	2,005	2,005	2,005	2,005	2,005	2,005	2,002	2,005

Panel B: Labor supply

	(1) Any ag. work	(2) Hours ag. work	(3) Any non-ag. work	(4) Hours non-ag. work	(5) Non-ag. income
Treatment	-0.012	0.588	-0.014	0.174	-0.036
	(0.027)	(1.023)	(0.026)	(1.917)	(0.413)
	[0.797]	[0.783]	[0.783]	[0.930]	[0.930]
Control Mean	0.77	25.52	0.43	20.86	2.85
N	2,005	1,528	2,005	744	619

Panel C: Household outcomes

	(1) Mod./ severe food insecurity	(2) Household asset index	(3) Younger co-resident children enrolled	(4) Male children enrolled	(5) Female children enrolled
Treatment	-0.056***	0.204***	0.243***	0.135**	0.108**
	(0.019)	(0.059)	(0.077)	(0.054)	(0.048)
	[0.020]	[0.007]	[0.013]	[0.037]	[0.059]
Control Mean	0.85	-0.02	1.58	0.80	0.78
N	1,830	1,830	1,407	1,407	1,407

Notes: $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. The variables reported in Panel A include indices for self-esteem and self-agency (both created as weighted indices following Anderson et al.; binary variables for stated intention to migrate, and aspirations for professional employment; the WHO score capturing subjective well-being; the Raven's matrix score; the forward digit span score; and score on a mathematics module. The variables reported in Panel B include binary variables for any youth engagement in agricultural and non-agricultural work as well as hours worked over the past week, and income earned from non-agricultural sources. The variables reported in Panel C include a binary variable for the household experiencing moderate or severe food insecurity; a household asset index; the number of younger co-resident children (aged 6–18 at follow-up, within the subsample reported resident in the same household at baseline) currently enrolled in school; and the number of male and female co-resident younger children enrolled. All regressions include strata fixed effects and controls for the baseline outcome variable, if available; standard errors are clustered at the subdistrict level.

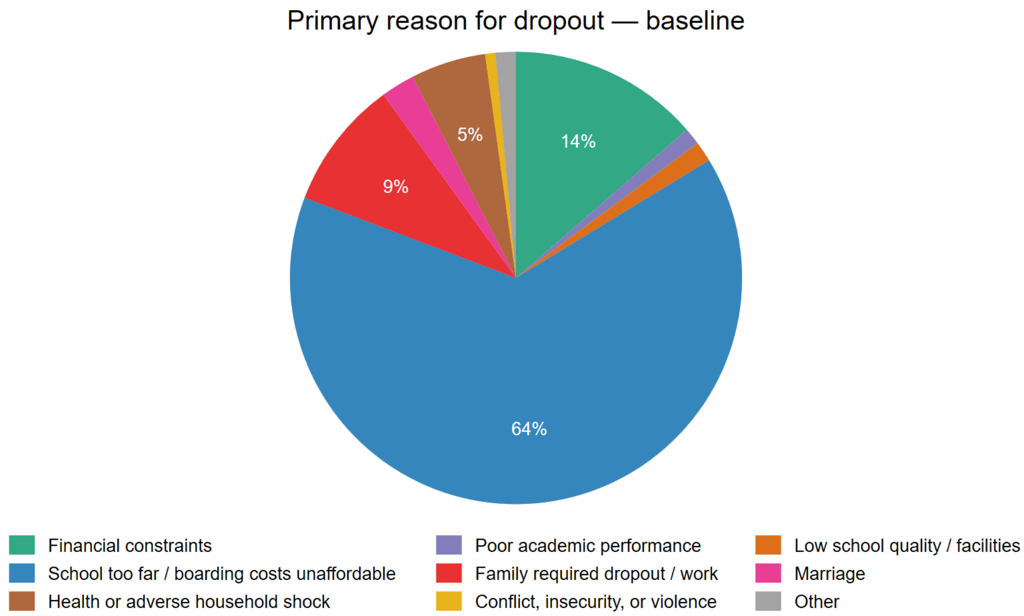
Table 7: Baseline characteristics used in GRF analysis and variable importance

Grades in most recent semester	0.19
Baseline seventh grader	0.15
WHO index of well-being	0.12
Household size	0.11
Age	0.11
Asset index	0.11
Baseline dropout	0.05
Exposure to recent shocks	0.05
Knows any high school graduate	0.03
Self-esteem index	0.02

Notes: This table reports the variable importance of baseline covariates included in the generalized random forest analysis.

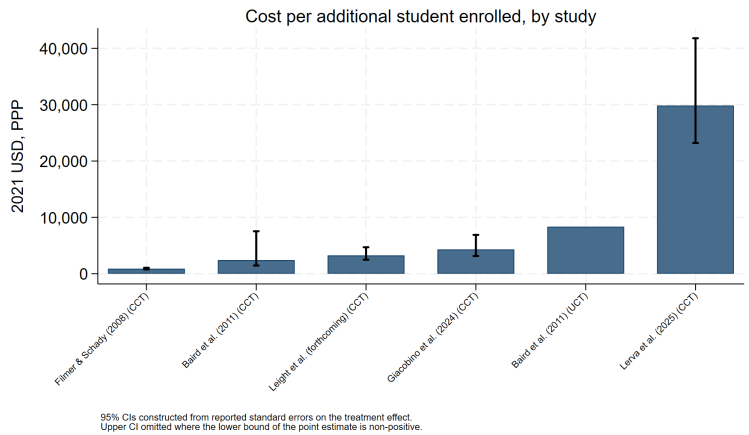
Appendix

Figure A1: Self-reported primary reason for dropout: Baseline dropouts

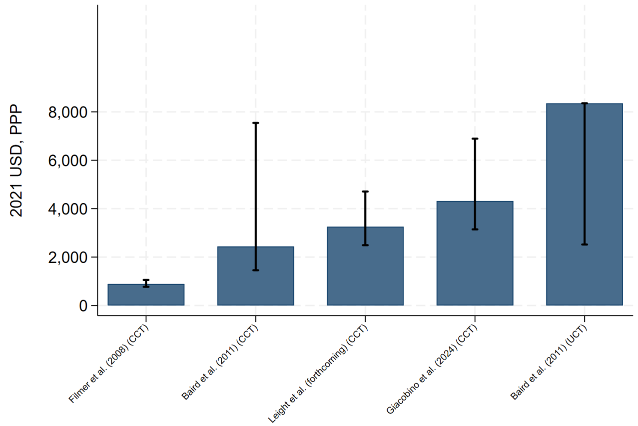


Notes: This figure summarizes the self-reported primary reason for dropout among the dropout sample at baseline.

Figure A2: Cost per marginal year of secondary school enrollment



(a) Full sample



(b) Sample excluding Lerva et al. (2025)

Notes: This graph presents estimates of the cost per marginal year of additional schooling induced for various secondary school cash transfer programs, estimated in 2021 PPP USD. The 95% CIs are constructed from reported standard errors on the treatment effect, and the upper CI is omitted where the lower bound of the point estimate is non-positive.

Table A1: Balance by grade cohort

	N	Treatment mean	Control mean	Diff.	p-value
Panel A: Baseline 7th graders					
Age	857	15.044	14.745	0.299	0.088
Youth gender	856	0.467	0.505	-0.037	0.449
Highest grade completed	857	6.019	6.007	0.012	0.143
Currently married	857	0.012	0.021	-0.009	0.280
Household size	855	6.040	5.777	0.263	0.311
Food insecurity	855	0.725	0.711	0.014	0.781
Household assets	855	2.879	2.871	0.008	0.944
Recent household shocks	855	3.249	3.249	0.001	0.996
Knows high school graduate	857	0.599	0.547	0.052	0.236
Ever used internet	814	0.066	0.062	0.004	0.866
Aspiration for university	857	0.790	0.792	-0.002	0.958
Academic performance	750	71.947	73.983	-2.036	0.160
WHO-5 well-being score	857	11.699	12.007	-0.308	0.529
Self-esteem index	857	4.965	4.946	0.019	0.829
Panel B: Baseline 8th graders					
Age	869	15.850	15.695	0.155	0.399
Youth gender	867	0.484	0.483	0.001	0.977
Highest grade completed	869	6.994	7.000	-0.006	0.069
Currently married	869	0.011	0.012	-0.002	0.846
Household size	867	6.036	5.696	0.341	0.150
Food insecurity	867	0.732	0.743	-0.011	0.805
Household assets	867	2.953	2.878	0.075	0.525
Recent household shocks	867	3.240	3.289	-0.049	0.714
Knows high school graduate	869	0.624	0.576	0.049	0.339
Ever used internet	839	0.090	0.066	0.025	0.292
Aspiration for university	869	0.865	0.821	0.043	0.130
Academic performance	769	74.059	72.746	1.314	0.237
WHO-5 well-being score	869	12.227	12.154	0.074	0.888
Self-esteem index	869	5.191	5.124	0.067	0.355
Panel C: Baseline dropouts					
Age	413	18.131	18.193	-0.061	0.855
Youth gender	411	0.308	0.274	0.034	0.541
Highest grade completed	413	8.100	8.036	0.063	0.022
Currently married	413	0.145	0.151	-0.006	0.886

	N	Treatment mean	Control mean	Diff.	p-value
Household size	409	5.900	5.894	0.007	0.982
Food insecurity	409	0.765	0.824	-0.060	0.293
Household assets	409	3.032	2.968	0.064	0.658
Recent household shocks	409	3.226	3.399	-0.173	0.403
Knows high school graduate	413	0.683	0.693	-0.009	0.869
Ever used internet	399	0.098	0.071	0.027	0.416
Aspiration for university	413	0.769	0.708	0.061	0.150
Academic performance	302	69.595	70.536	-0.941	0.548
WHO-5 well-being score	413	13.597	14.255	-0.658	0.268
Self-esteem index	413	4.878	4.932	-0.054	0.666

Notes: $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. The difference column is the coefficient on treatment from a regression of each baseline variable on a treatment dummy within the specified cohort; standard errors are clustered at the subdistrict level.

Table A2: Baseline covariates predictive of passing rates

	(1) Pooled	(2) 7th graders	(3) 8th graders
Treatment	0.050	0.429**	0.004
	(0.044)	(0.193)	(0.057)
Age	-0.055***	-0.045***	-0.054***
	(0.008)	(0.013)	(0.017)
Treatment × Age	0.026**	0.031	0.025
	(0.013)	(0.022)	(0.021)
Female	0.028	0.068	-0.058
	(0.034)	(0.049)	(0.058)
Treatment × Female	0.050	0.039	0.059
	(0.051)	(0.077)	(0.080)
Highest grade completed	-0.026	-0.246	0.153
	(0.036)	(0.183)	(0.267)
Treatment × Highest grade completed	-0.118***	0.349	0.000
	(0.044)	(0.250)	(.)
Household size	-0.026***	-0.035**	-0.018
	(0.009)	(0.014)	(0.012)
Treatment × Household size	0.013	0.002	0.008
	(0.012)	(0.021)	(0.017)
Food insecurity	-0.019	0.069	-0.151**
	(0.037)	(0.054)	(0.063)
Treatment × Food insecurity	-0.004	-0.101	0.161**
	(0.055)	(0.086)	(0.076)
Household assets	-0.011	0.021	-0.037**
	(0.014)	(0.027)	(0.015)
Treatment × Household assets	-0.006	-0.042	0.030
	(0.019)	(0.036)	(0.030)
Recent household shocks	0.008	0.010	0.016
	(0.013)	(0.019)	(0.015)
Treatment × Recent household shocks	-0.021	-0.003	-0.044*
	(0.020)	(0.027)	(0.023)
Knows high school graduate	-0.074**	-0.074	-0.090
	(0.036)	(0.058)	(0.060)
Treatment × Knows high school graduate	0.087*	0.106	0.124
	(0.052)	(0.092)	(0.080)
Ever used internet	0.024	-0.009	0.056
	(0.063)	(0.102)	(0.089)
Treatment × Ever used internet	-0.064	-0.164	-0.059

	(1)	(2)	(3)
	Pooled	7th graders	8th graders
	(0.084)	(0.152)	(0.121)
Aspiration for university	0.028	0.032	-0.008
	(0.038)	(0.068)	(0.050)
Treatment × Aspiration for university	-0.029	-0.003	-0.130
	(0.065)	(0.101)	(0.085)
Academic performance	0.003	0.004	0.003
	(0.002)	(0.003)	(0.003)
Treatment × Academic performance	0.002	-0.000	0.002
	(0.002)	(0.004)	(0.004)
WHO-5 well-being score	-0.011***	-0.014***	-0.004
	(0.003)	(0.005)	(0.005)
Treatment × WHO-5 well-being score	0.006	0.014**	-0.004
	(0.005)	(0.007)	(0.007)
Self-esteem index	0.065***	0.048	0.020
	(0.023)	(0.034)	(0.029)
Treatment × Self-esteem index	-0.009	0.030	-0.015
	(0.028)	(0.046)	(0.038)
p-value: joint test of interactions	0.259	0.437	0.307
N	1,652	677	702

Notes: $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. This table reports OLS regressions of passing the primary school leaving exam on demeaned baseline covariates interacted with treatment status, for the sample of baseline seventh and eighth graders only. The treatment coefficient reflects the treatment effect at the sample mean of all covariates. Standard errors are clustered at the subdistrict level.

Table A3: Heterogeneous effects with respect to predicted passing probability

	(1)	(2)	(3)	(4)	(5)	(6)
	Pooled		7th graders		8th graders	
Subsample	Below med.	Above med.	Below med.	Above med.	Below med.	Above med.
	Panel A: Passing primary school leaving exam					
Treatment	0.096*	-0.017	0.124*	0.066	0.017	-0.008
	(0.050)	(0.040)	(0.064)	(0.054)	(0.058)	(0.048)
Control Mean	0.442	0.800	0.353	0.597	0.652	0.861
N	690	689	339	338	351	351
	Panel B: Currently enrolled in secondary school					
Treatment	0.185***	0.077*	0.161***	0.195***	0.158***	0.046
	(0.046)	(0.046)	(0.057)	(0.058)	(0.049)	(0.056)
Control Mean	0.215	0.394	0.180	0.370	0.226	0.418
N	690	689	339	338	351	351

Notes: $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. This table reports treatment effects on current secondary enrollment, stratified by a predicted probability of passing the primary school leaving exam. The predicted index is constructed by regressing exam passing on baseline covariates in the control arm and generating fitted values for all observations. Below-median and above-median subsamples are split at the median of this index. All regressions include strata fixed effects and controls for the baseline outcome variable, if available; standard errors are clustered at the subdistrict level.

Table A4: Treatment effects: Months of school attended since baseline

	(1)	(2)	(3)	(4)
	Months of school attended since baseline			
Treatment	2.645***	2.410***	2.419***	4.265***
	(0.550)	(0.554)	(0.781)	(0.953)
	[0.000]	[0.000]	[0.002]	[0.000]
	All sample	Baseline 7th graders	Baseline 8th graders	Baseline dropouts
Control Mean	11.95	15.26	12.24	4.03
N	2,005	805	820	380

Notes: $^{*}p < 0.10, ^{**} p < 0.05, ^{***} p < 0.01$. All regressions include strata fixed effects and controls for the baseline outcome variable, if available; standard errors are clustered at the subdistrict level.

Table A5: Treatment effects: Schooling expenditure, residence, and school choice

Panel A: Schooling expenditure

	All students			Currently enrolled in secondary		
	(1)	(2)	(3)	(4)	(5)	(6)
	Total school exp.	Food / lodging exp.	All other exp.	Total school exp.	Food / lodging exp.	All other exp.
Treatment	18.857***	11.756***	7.101***	15.806***	11.436***	4.370**
	(2.681)	(1.969)	(1.035)	(4.885)	(3.862)	(1.799)
	[0.000]	[0.000]	[0.000]	[0.004]	[0.008]	[0.025]
Control Mean	18.74	9.55	9.19	51.53	27.62	23.91
N	2,005	2,005	2,005	633	633	633

Panel B: Currently enrolled in secondary school

	Lived with parents	Lived with other adults	Lived with other youth
Treatment	-0.197***	0.064	0.128***
	(0.061)	(0.044)	(0.049)
	[0.004]	[0.205]	[0.017]
Control Mean	0.45	0.29	0.26
N	633	633	633

Panel C: School choice

	Attended nearest school	School infrastructure index	Commuting time (minutes)
Treatment	-0.038	0.029	11.977
	(0.073)	(0.101)	(9.629)
	[0.664]	[0.775]	[0.260]
Control Mean	0.32	-0.02	109.28
N	584	592	633

Notes: $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. All regressions include strata fixed effects and controls for the baseline outcome variable, if available; standard errors are clustered at the subdistrict level.

Table A6: Migration

	(1) Departed from baseline household	(2) Migrated within baseline region	(3) Migrated out of region (incl. international)
Treatment	-0.021	-0.013	-0.008**
	(0.014)	(0.013)	(0.004)
	[0.190]	[0.337]	[0.091]
Control Mean	0.092	0.076	0.017
N	2,005	2,005	2,005

Notes: $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. The dependent variables are binary variables for any departure from the baseline household; outmigration within the baseline region of residence; or outmigration beyond the baseline region of residence (including international outmigration). All regressions include strata fixed effects and controls for the baseline outcome variable, if available; standard errors are clustered at the subdistrict level.

Table A7: Treatment effects: Enrollment using school-reported data

	Administrative records		Self-reported	
	Fall 2023	Fall 2024	Fall 2023	Fall 2024
Panel A: Enrolled in Grade 7 at baseline				
Treatment		0.259***		0.176***
		(0.049)		(0.043)
		[0.000]		[0.000]
Control Mean		0.252		0.547
N		858		805
Panel B: Enrolled in Grade 8 at baseline				
Treatment	0.305***	0.198***	0.140***	0.078
	(0.058)	(0.037)	(0.031)	(0.051)
	[0.000]	[0.000]	[0.000]	[0.127]
Control Mean	0.274	0.159	0.687	0.432
N	864	869	820	820
Panel C: Dropouts at baseline				
Treatment	0.296***	0.097***	0.365***	0.111**
	(0.056)	(0.031)	(0.059)	(0.045)
	[0.000]	[0.003]	[0.000]	[0.018]
Control Mean	0.058	0.062	0.274	0.117
N	410	414	380	380

Notes: $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. All regressions include strata fixed effects and controls for the baseline outcome variable, if available; standard errors are clustered at the subdistrict level.

Table A8: Attrition

	Attrited
Treatment	-0.009
	(0.013)
Age	0.005
	(0.004)
Youth gender	0.005
	(0.020)
Highest grade completed	-0.019
	(0.012)
Household size	0.002
	(0.003)
Food insecurity	0.021
	(0.021)
Household assets	-0.008
	(0.006)
Recent household shocks	0.007
	(0.009)
Knows high school graduate	0.014
	(0.018)
Ever used internet	-0.037
	(0.031)
Aspiration for university	0.019
	(0.020)
Academic performance	-0.001
	(0.001)
WHO-5 well-being score	-0.001
	(0.002)
Self-esteem index	-0.006
	(0.010)
Treatment × Age	0.000
	(0.006)
Treatment × Youth gender	-0.028
	(0.024)
Treatment × Highest grade completed	0.026
	(0.017)
Treatment × Household size	0.006
	(0.005)
Treatment × Food insecurity	-0.025
	(0.028)
Treatment × Household assets	0.008
	(0.009)
Treatment × Recent household shocks	-0.017*
	(0.010)
Treatment × Knows high school graduate	-0.012
	(0.025)
Treatment × Ever used internet	0.010
	(0.043)
Treatment × Aspiration for university	0.012
	(0.024)
Treatment × Academic performance	0.002
	(0.001)
Treatment × WHO-5 well-being score	0.001

	Attrited
	(0.002)
Treatment × Self-esteem index	-0.007
	(0.013)
Mean attrition in control	0.070
p-value for joint test	0.423

Notes: $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. This table reports a regression of a binary variable for attrited on baseline covariates and the interaction of baseline covariates and treatment. All regressions include strata fixed effects, and standard errors are clustered at the subdistrict level.

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